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MICHAŁ KOKOWSKI

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Editorial

Od Redakcji

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Evolutionary transformation of the journal. Part 6

Abstract

The article outlines the sixth phase of the development of the journal *Studia Historiae Scientiarum* (previous name *Prace Komisji Historii Nauki PAU / Proceedings of the PAU Commission on the History of Science*).

The information is provided on the following matters: the journal obtaining the award in the ministerial program "Support for scientific journals 2019–2020" (in April 2019), the evaluation of the magazine in "ICI Master Journal List 2017" (published at the end of 2018) and in "List of journals of the Ministry of Science and Higher Education of the Polish Republic 2019" (published on 31 July 2019), the indexation of the journal in the Scopus database (from September 2019), the implementation of the service Similarity Check (Crossref), the works on

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updating the journal's website in OJS (3.1.2.1.), the number of foreign authors and the number of reviewers of the current volume of the journal.

Keywords: *Studia Historiae Scientiarum*, *Prace Komisji Historii Nauki PAU / Proceedings of the PAU Commission on the History of Science*

Ewolucyjna transformacja czasopisma. Część 6

Abstrakt

Naszkicowano szósty etap rozwijania czasopisma *Studia Historiae Scientiarum* (wcześniejsza nazwa *Prace Komisji Historii Nauki PAU*).

Podano informacje o zdobyciu przez czasopismo wyróżnienia w ramach ministerialnego programu „Wsparcie dla czasopism naukowych 2019–2020" (w kwietniu 2019 r.), ewaluacji czasopisma w „ICI Master Journal List 2017" (opublikowanej u końca 2018 r.) i „Wykazie czasopism MNiSW 2019" (opublikowanym 31 lipca 2019 r.), indeksacji czasopisma w bazie Scopus (od września 2019 r.), wdrożeniu usługi Similarity Check (Crossref), pracach nad aktualizacją strony internetowej czasopisma w OJS (3.1.2.1.) oraz liczbie zagranicznych autorów i liczbie recenzentów bieżącego tomu czasopisma.

Słowa kluczowe: *Studia Historiae Scientiarum*, *Prace Komisji Historii Nauki PAU*

1. Changes made so far

The journal's development since 2013 is described in the following texts: Kokowski 2013; 2014; 2015; 2016; 2017; 2018. In this text we are announcing additional modifications introduced in 2018/2019.

2. "Support for scientific journals 2019–2020" organized by the Ministry of Science and Higher Education in Poland

The journal *Studia Historiae Scientiarum* has won the competition "Support for scientific journals 2019–2020" organized by the Ministry of Science and Higher Education in Poland. This means that the articles published in the journal can be taken into account as credits regarding scientific degrees and professorship in Poland.

Our journal received the maximum number of points (100 out of 100). The numerical result aside, our journal received a positive opinion of the ministerial expert:

> The periodical "Studia Historiae Scientiarum" is an exceptional journal devoted to the history of science, known for many years on the Polish publishing market of scientific journals. Already the current level of this periodical places it high among Polish scientific journals. [...] The reviewed periodical "Studia Historiae Scientiarum" is an interdisciplinary journal *par excellence* due to its research profile. If we strive both in Polish and global science for an ever greater interdisciplinarity, then we can say with a high degree of probability that the activity of this type of periodical is a very important contribution to this kind of effort. When assessing the "Studia Historiae Scientiarum", it is necessary to emphasize the high scientific level of the texts published in the journal, proving its international significance and prestige. [...]. Both the current and planned activities of the applicant in this respect are promising in the raise of the visibility of the journal and its range on the international arena. The high level of the journal is a guarantee of the journal's significant impact on the development of the humanities, including the disciplines of history and philosophy.

The words of the ministerial expert about our journal are not only a great honour to us, but also a commitment to further diligent work.

3. Evaluation of the journal

In 2018/2019, the journal underwent evaluation at the Index Copernicus International and at the Ministry of Science and Higher Education in Poland.

In the "ICI Master Journal List 2017" (published at the end of 2018) the journal *Studia Historiae Scientiarum* was ranked first among the Polish journals in the history of science and related disciplines: history, philosophy of science, and science of science (there are no results yet for 2018).[1]

In the "List of journals of the Ministry of Science and Higher Education 2019" (published on July 31, 2019) our journal received two ratings: 20 and 40 points! 20 points was received by our modernized journal under the name: *Studia Historiae Scientiarum* (used since volume 15 (2016)), and 40 points by our unmodified journal under the previously used name: *Prace Komisji Historii Nauki Polskiej Akademii Umiejętności / Proceedings of the PAU Commission on the History of Science*. This issue is currently being clarified by the Editorial Board.[2]

4. Indexation of the journal in the Scopus database

On 14 June 2019, the journal *Studia Historiae Scientiarum* was accepted into the Scopus indexing database. On 16 September this year an agreement was signed on indexing the journal in this database. It is worth emphasizing that this is the first Polish magazine on the history of science that has been accepted into this database as a result of submitting an application for admission.

5. Implementation of the Similarity Check (Crossref) service

To protect the journal from plagiarised articles Similarity Check (Crossref) is implemented beginning with the current volume.

6. Website update

Work is underway to update our journal's website in the Open Journal System (version 3.1.2.1). Finalizing this task will facilitate reporting or indexing the journal in, among others, Google Scholar, Crossref, Scopus and DOAJ.

7. Foreign authors

The percentage of foreign authors in the previous volume was 33% of all authors, and in the current volume – 31% of all authors.

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1 See Kokowski 2019, Tables 1–4.

2 *Ibidem*; MNiSW 2019, No. 27873 & No. 28528.

8. Foreign reviewers

The percentage of foreign reviewers in the previous volume was 31% of all reviewers, and in the current volume – 43% of all reviewers.

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Kokowski, Michał 2019: Wykazy czasopism MNiSW 2017 i 2019, ICI Journal Master List 2014–2017, a polskie czasopisma z historii nauki, historii, filozofii nauki oraz naukoznawstwa (*Lists of journals of the Ministry of Science and Higher Education in Poland 2017 & 2019*, *ICI Journal Master List 2014–2017*, and the Polish journals on the history of science, history, philosophy of science and science of science). *Studia Historiae Scientiarum* 18, pp. 505–513. DOI: 10.4467/2543702XSHS.19.015.11021.

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Słowa kluczowe: *Studia Historiae Scientiarum*, *Prace Komisji Historii Nauki PAU*

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Keywords: *Studia Historiae Scientiarum*, *Prace Komisji Historii Nauki PAU / Proceedings of the PAU Commission on the History of Science*

1. Wprowadzone dotąd zmiany

Rozwój czasopisma od 2013 roku opisują następujące teksty: Kokowski 2013; 2014; 2015; 2016; 2017; 2018. W tym tekście informujemy o dodatkowych modyfikacjach wprowadzonych w roku 2018/2019.

2. Ministerialny program „Wsparcie dla czasopism naukowych 2019–2020"

Czasopismo *Studia Historiae Scientiarum* zostało laureatem konkursu MNiSW „Wsparcie dla czasopism naukowych 2019–2020" (lp. 177). Oznacza to, że artykuły opublikowane w czasopiśmie będzie można uwzględniać w dorobku na stopnie naukowe i tytuł profesorski.

W ocenie wniosku konkursowego nasze czasopismo otrzymało 100 punktów na 100 możliwych. Poza wynikiem liczbowym, warto zacytować opinię ministerialnego eksperta o naszym czasopiśmie:

> Periodyk *Studia Historiae Scientiarum* to czasopismo wyjątkowe, poświęcone dziejom nauki, od lat znane na polskim rynku wydawniczym czasopism naukowych. Już dotychczasowy poziom tego periodyku sytuuje go wysoko wśród polskich czasopism naukowych. […] Recenzowany periodyk *Studia Historiae Scientiarum* z racji swego profilu badawczego jest czasopismem *par excellence* interdyscyplinarnym. Jeżeli dążymy zarówno w nauce polskiej, jak i światowej do coraz większej interdyscyplinarności, to z dużą dozą prawdopodobieństwa możemy powiedzieć, że działalność tego typu periodyku jest bardzo istotnym wkładem na rzecz tego rodzaju starań. Oceniając *Studia Historiae Scientiarum*, należy podkreślić wysoki poziom naukowy zamieszczanych w czasopiśmie tekstów, świadczących o jego międzynarodowym znaczeniu i prestiżu. […] Zarówno dotychczasowe, jak i planowane przez wnioskodawcę działania w tym zakresie zapowiadają podniesienie rozpoznawalności czasopisma i jego zasięgu na arenie międzynarodowej. Wysoki poziom naukowy czasopisma jest gwarantem znaczącego wpływu czasopisma na rozwój nauk humanistycznych, w tym dyscypliny „historia" i „filozofia" [kursywa i znaki cudzysłowu – M.K.].

Opinia ministerialnego eksperta na temat naszego czasopisma jest dla nas wielkim wyróżnieniem, ale także zobowiązaniem do dalszej rzetelnej pracy.

3. Ewaluacja czasopisma

W roku 2018/2019 czasopismo poddało się ewaluacji w Index Copernicus International i w MNiSW.

W „ICI Master Journal List 2017" (opublikowanej u końca 2018 r.) czasopismo *Studia Historiae Scientiarum* uzyskało pierwszą lokatę wśród polskich czasopism z historii nauki i pokrewnych dyscyplin: „historia", „filozofia nauki" oraz „naukoznawstwo" (nie ma jeszcze wyników za rok 2018)[1].

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[1] Zob. Kokowski 2019, tabele 1–4.

W „Wykazie czasopism MNiSW 2019" (opublikowanym 31 lipca 2019 r.) nasze czasopismo uzyskało dwie oceny: 20 i 40 punktów! Pierwszą ocenę – 20 punktów otrzymało nasze unowocześnione czasopismo pod nazwą: *Studia Historiae Scientiarum* (używaną od tomu 15 (2016)), zaś 40 punktów – nasze czasopismo pod poprzednio używaną nazwą: *Prace Komisji Historii Nauki Polskiej Akademii Umiejętności / Proceedings of the PAU Commission on the History of Science*[2]. Kwestia ta jest aktualnie wyjaśniana przez redakcję czasopisma.

4. Indeksacja czasopisma w bazie Scopus

W dniu 14 czerwca 2019 r. czasopismo *Studia Historiae Scientiarum* zostało przyjęte do bazy indeksacyjnej Scopus. W dniu 16 września br. podpisana została umowa w sprawie indeksowania czasopisma w tej bazie. Warto podkreślić, że jest to pierwsze polskie czasopismo z historii nauki, które zostało włączone do tej bazy po uprzednim złożeniu wniosku o indeksowanie czasopisma w tej bazie.

5. Wdrożenie usługi Similarity Check (Crossref)

Aby zabezpieczyć czasopismo przed splagiatowanymi artykułami, od aktualnego tomu wdrożona została usługa Similarity Check (Crossref).

6. Aktualizacja strony internetowej

Trwają prace nad aktualizacją strony internetowej czasopisma w Open Journal System (wersja 3.1.2.1). Sfinalizowanie tego zadania ułatwi raportowanie lub indeksowanie czasopisma m.in. w Google Scholar, Crossref, Scopus oraz DOAJ.

7. Zagraniczni Autorzy

Udział procentowy Autorów zagranicznych w poprzednim tomie wyniósł 33% wszystkich Autorów, a w obecnym tomie – 31% wszystkich Autorów.

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[2] *Ibidem*; MNiSW 2019, lp. 27873 i lp. 28528.

8. Zagraniczni Recenzenci

Udział procentowy Recenzentów zagranicznych w poprzednim tomie wyniósł 31% wszystkich Recenzentów, a w aktualnym tomie – 43% wszystkich Recenzentów.

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Science in Poland

Nauka w Polsce

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Rola historii nauki w działalności naukowej, dydaktycznej i filozoficznej Mariana Smoluchowskiego

Abstrakt

Marian Smoluchowski (1872–1917) był wybitnym polskim fizykiem, znanym m.in. jako pionier fizyki statystycznej. Jego krótka praca o historii fizyki w Polsce stanowi pionierskie opracowanie tego zagadnienia, była cytowana wiele razy, tworząc punkt wyjścia do dalszych badań nad historią fizyki w Polsce. Należy jednakże podkreślić, że nigdy nie poddano systematycznej analizie zagadnienia roli historii nauki w działalności naukowej Smoluchowskiego. W niniejszej pracy skupiono się na trzech obszarach działalności związanych z historią nauki: badawczym, dydaktycznym i filozoficznym. Wskazano, że doniosłość historii nauki dla Smoluchowskiego brała się z jej kulturowego znaczenia. Historia nauki odegrała ważną rolę w procesie krystalizacji koncepcji filozoficznych wybitnego

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fizyka, a także w dydaktyce fizyki, ukazując wewnętrzną dynamikę rozwoju nauki i inspirując do nowych odkryć. To ostatnie zagadnienie jest ściśle związane ze specyficznym podejściem metodologicznym Smoluchowskiego do fizyki, nazwanym przez niego „romantyzmem nauki". W artykule wskazano nie tylko, że Smoluchowski jest pionierem historii fizyki w Polsce, ale też że przygotował podstawy dla przyszłego rozwoju tej dyscypliny.

Słowa kluczowe: *Marian Smoluchowski*, *historia fizyki*, *filozofia w nauce*, *historia nauki w dydaktyce fizyki*, *historia nauki a kultura*

The role of the history of science in Marian Smoluchowski's scientific, didactic and philosophical activities

Abstract

Marian Smoluchowski (1872–1917) was an outstanding Polish physicist, known e.g. as a pioneer of statistical physics. His short paper about history of physics in Poland represents the initial study in this field. It was cited many times, creating the starting point for the historiography of physics in Poland. However, the role of history of science played in Smoluchowski's activities was never systematically analyzed before. This article concentrates on three main domains of Smoluchowski's activities involved with history of science: scientific, didactic and philosophical. It reveals that for Smoluchowski the importance of history of science was determined by its cultural impact. History of science played the important role in crystallization of his philosophical concepts, as well as in didactics revealing the internal dynamics of science and inspiring to new discoveries. The last issue is tied with specific methodological approach to physics called by Smoluchowski 'romanticism of science'. This paper shows that Smoluchowski was not only a pioneer of history of physics in Poland, but also prepared some foundations for future development of this field of research.

Keywords: *Marian Smoluchowski*, *history of physics*, *philosophy in science*, *history of science and didactics of physics*, *history of science and culture*

1. Wstęp

Odpowiedzi na pytania o rolę historii nauki w działalności wybitnego fizyka Mariana Smoluchowskiego (1872–1917) silnie zależą od przyjętej koncepcji historii nauki. Historiografia nauki – jak każda historiografia – kształtowana jest bowiem przez akceptowane założenia o charakterze ogólnym. Dotyczą one m.in. podzielanych wartości, koncepcji epistemicznych, przyjmowanych punktów widzenia, podporządkowane są również konkretnym celom badawczym bądź praktycznym. W tym świetle wątpliwe staje się poszukiwanie ponadczasowego, uniwersalnego modelu uprawiania historii nauki. Różnorodność podejść i różnorodność celów, którym ma ono służyć, warto potraktować jako wyraz bogactwa tej dziedziny i zachętę do analizy różnorodnych koncepcji[1]. Próbując zrekonstruować i ocenić znaczenie historii nauki w działalności Mariana Smoluchowskiego, zmuszeni jesteśmy do porzucenia prób wartościowania jego dokonań z perspektywy współczesnych poglądów na temat tej dyscypliny. W przeciwnym wypadku nie będziemy w stanie zrozumieć w adekwatny sposób roli, jaką jego dokonania odegrały w działalności naukowo-dydaktycznej, a także w rozwoju historii nauki w Polsce.

O dokonaniach Smoluchowskiego na gruncie historii nauki pisano rzadko, choć często cytowano jego pracę „Zarys dziejów fizyki w Polsce" (Smoluchowski 1917a). Pierwsze wzmianki o „Zarysie..." zawarł w swej recenzji Samuel Dickstein[2]. Podsumowując ocenę „Zarysu...", pisał, że świadczą „o sumiennej pracy w nie [tj. w dzieło] włożonej, o erudycyi i talencie pedagogicznym Autora, które śmiemy to twierdzić, stoją na wysokości jego twórczego w dziedzinie nauk fizycznych umysłu" (Dickstein 1917, s. 251). Świadectwo to ukazuje, jak w drugiej dekadzie XX wieku odbierano pierwsze polskie próby syntezy historii nauki i jak duże było ich znaczenie pedagogiczne. Dziś z perspektywy stulecia znaczenie to nie jest już oczywiste, zatem entuzjastyczne uwagi Dicksteina pozwalają lepiej zrozumieć rolę, jaką odegrał skromny i niekiedy krytykowany dziś „Zarys..." w rozwoju polskiej historiografii fizyki.

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[1] Przegląd koncepcji i sposobów uprawiania historii nauki znaleźć można np. w monografii: Kragh 1987, s. 120n. Zob. też Kokowski 2019, Appendix 1. „Preliminary methodological considerations about the historical method".

[2] Warto dodać, że Samuel Dickstein był jednym z inicjatorów systematycznych prac nad historią nauki na ziemiach polskich (zob. Majkowska 2003).

Kolejne uwagi na ten temat opublikowano dopiero czterdzieści lat później. Władysław Krajewski zwracał uwagę, że „obok zagadnień filozoficznych w kręgu zainteresowania Smoluchowskiego pozostają – ściśle z nimi związane – zagadnienia historii nauki" (Krajewski 1956, s. 177). Rok później ważne oceny opublikował Armin Teske (1957); powrócimy do nich w końcowej części tekstu. O „interesujących uwagach z teorii i historii fizyki" wspominał natomiast Stanisław Kamiński w monografii *Pojęcie nauki i klasyfikacja nauk* (Kamiński 1981, s. 97). W opublikowanej w latach 80. XX wieku książce *Dzieje nauczania historii nauki i historii techniki w Polsce* Irena Stasiewicz-Jasiukowa pisała: „Dzieje fizyki z uniwersyteckich katedr upowszechniali Władysław Heinrich oraz Marian Smoluchowski" (Stasiewicz-Jasiukowa 1982, s. 173). Dwie dekady później wspomniana została również (niezbyt ściśle) jedyna *stricte* historyczna praca Smoluchowskiego. Bronisław Średniawa i Zofia Pawlikowska-Brożek w opracowaniu „Polskie podręczniki historii fizyki, astronomii i matematyki" napisali: „ogłosił obszerny 27-stronicowy artykuł «Zarys dziejów fizyki w Polsce», gdzie przedstawił historię fizyki w Polsce od dzieła Witelona do czasów sobie współczesnych" (Średniawa, Pawlikowska-Brożek 2005, s. 102). Mając na uwadze większość dotychczasowych opracowań, zrozumiałe staje się, że pytania o rolę historii nauki w działalności naukowej Smoluchowskiego mogą wydawać się nieinteresujące lub nawet źle postawione. Bliższa analiza spuścizny Smoluchowskiego pozwala jednak ukazać w innym świetle to zagadnienie, co jest celem niniejszego artykułu.

2. Uwagi metodologiczne

Z pewnością zagadnienie historii nauki w ujęciu Smoluchowskiego podejmowane było nieczęsto, ponieważ w pracach badawczych Smoluchowskiego, stanowiących większość jego publikacji, siłą rzeczy uwaga skupiona była na rozwiązaniu problemów badawczych. Bliższa analiza publikacji Smoluchowskiego pokazuje jednak, że próbował on umieszczać w prawie każdej pracy jakieś uwagi lub odniesienia historyczne ukazujące tło opisywanego problemu. Inaczej natomiast rzecz się ma z wykładami – zarówno akademickimi, jak i popularyzatorskimi, tam odniesienia są liczne i pełnią istotną rolę. Dogodną perspektywą do wyjaśnienia wspomnianego faktu jest Gerarda Holtona (1922–) koncepcja nauki prywatnej i publicznej (*private science vs public science*). Należy przypomnieć, że w czasach działalności Smoluchowskiego panował stosunkowo sztywny kanon pisania prac naukowych (Holtonowskie *public science*). Sam Smoluchowski podkreślał znaczenie takiego kanonu jako ograniczającego rozwój fizyki. Pozytywistyczne nastawienie, szczególnie mocno wyrażane w środowisku wiedeńskim, z którego wywodził się Smoluchowski, wymuszało rugowanie bądź silne ograniczanie „zbędnych" kontekstów filozoficznych i historycznych. (Co ciekawe, zupełnie pozbawione jakichkolwiek odniesień historycznych były jedynie wczesne anglojęzyczne prace Smoluchowskiego powstałe podczas prac w laboratorium Lorda Kelvina). Ograniczeń takich nie nakładała natomiast konwencja wykładu, w której można było swobodnie pokazywać kulisy powstawania wiedzy naukowej. W takim ujęciu zrozumiałe staje się, że historia nauki ujawnia się u Smoluchowskiego głównie w „nauce prywatnej", której wyrazem były odczyty i wykłady[3]. Trzeba dodać, że już Armin Teske zauważał, iż historia nauki kształtowała również w pewnym stopniu „oficjalną" działalność polskiego uczonego (Teske 1957, ss. 686–687).

Należy w tym miejscu zastrzec, że poglądy Smoluchowskiego na historię nauki dalekie są od obecnie przyjmowanych, nie należy zatem przykładać współczesnych miar do koncepcji, które powstawały wiek wcześniej. Reguła ta typowa jest dla badań historycznych, gdyż powodowana jest chęcią bardziej adekwatnego zrozumienia minionej rzeczywistości. Trudno więc doszukiwać się u Smoluchowskiego wielu współczesnych cech historii nauki, można za to znaleźć koncepcje, które tworzyły grunt pod stworzenie i rozwój tej dyscypliny w Polsce. Z tego względu spełniły one ważną historycznie rolę, nawet jeśli dziś kształt historii nauki jest już wyraźnie inny.

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[3] Zagadnienie to pozostawało jednak w dużej mierze nieznane, ponieważ do niedawna większość materiałów związanych z tym aspektem działalności wybitnego fizyka pozostawała w rękopisach przechowywanych w zbiorach Biblioteki Jagiellońskiej. Z okazji 100. rocznicy śmierci Smoluchowskiego opublikowane zostały najważniejsze odczyty, w świetle których możemy bardziej adekwatnie ocenić miejsce historii nauki w działalności słynnego uczonego. Teksty te znajdują się w okolicznościowym numerze czasopisma *Zagadnienia Filozoficzne w Nauce* nr 62 (2017), które dostępne jest pod adresem: http://www.zfn.edu.pl/index.php/zfn/issue/view/28.

3. Historia nauki oczyma wybitnego fizyka

Smoluchowski opisał swój pogląd na historię nauki w odczycie „Dzisiejszy stan teoryi atomistycznej" (1913). Uznał on perspektywę historyka za najlepszą dla wytłumaczenia współczesnego stanu nauki. Wyróżnił wówczas dwa typy historiografii nauki. Pierwszy polega na „rejestracji faktów zewnętrznych (po części nawet przypadkowych)", przy czym autor nie precyzował użytego pojęcia faktu zewnętrznego. Drugi typ historii nauki bazuje na pierwszym, ale ma cele bardziej ogólne:

> Jeżeli ów historyk będzie **umysłem głębszym** […] będzie dążył do tego, aby zdać sprawę z **ukrytych sprężyn rozwoju naukowego**, **z ogólnych prądów** umysłowych objawiających się w nauce; to jest dopiero **właściwa historia nauki** (Smoluchowski 1913, s. 355–356, wyróżnienia P.P.).

Ta wyraźna autodeklaracja Smoluchowskiego wskazuje, że za istotę uprawiania historii nauki uważał on odkrywanie wewnętrznej dynamiki nauki i jej filozoficznego podłoża (ogólne prądy). To wyjaśnia, dlaczego historia nauki była blisko związana u Smoluchowskiego z refleksją filozoficzną, o czym wspominał niegdyś Krajewski (1956, s. 177). Tenże komentator doceniał „antyfaktograficzne" nastawienie historiografii w ujęciu Smoluchowskiego i widział w takim podejściu doniosłe znaczenie światopoglądowe. Trudno zgodzić się na zawężenie przez Krajewskiego poglądów filozoficznych Smoluchowskiego jedynie do światopoglądu. Krajewski, przyjmując w swej ocenie dogmatyczne pozycje materializmu dialektycznego, uznał, że Smoluchowski nie był w stanie adekwatnie zrozumieć owych „ukrytych sprężyn rozwoju", bo „nie widział rzeczywistych źródeł rozwoju nauki, tkwiących w społecznej praktyce produkcyjnej" (Krajewski 1956, s. 178). Rzeczywiście Smoluchowski nie mógłby zgłosić akcesu do stanowiska podzielanego przez Krajewskiego, ponieważ widział on głównie internalistyczne czynniki rozwoju naukowego[4], a jedynym czynnikiem, który mógłby być ewentualnie

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[4] Odrzucał m.in. psychologiczne wyjaśnienia zmian naukowych. Pisał następująco o postawie odróżniającej fizyków od humanistów: „Ale wyznawcy nauk ścisłych są to ludzie twardzi, uparci w swych dążeniach, którzy nie ustępują pod wpływem ogólnych nastrojów, a dają się przekonać tylko argumentami namacalnymi" (Smoluchowski 1913, s. 359).

interpretowany jako eksternalistyczny, był „ogólny stan kultury umysłowej”. Inne czynniki eksternalistyczne, jak na przykład zbyt niskie dotacje na rozwój laboratoriów, postrzegał on jako warunki konieczne bądź ograniczenia rozwoju, a nie źródła (Smoluchowski 2017a, s. 274).

Opisane dwa typy historiografii nauki wiążą się ze Smoluchowskiego koncepcją różnic między naukami humanistycznymi a przyrodniczymi (ścisłymi). Różnice te przedstawił on bowiem na paradygmatycznym przykładzie historii (powszechnej) i fizyki. Według niego:

> Historyk zachowuje się przeciwnie [niż fizyk]; jego nauka polega na możliwie dokładnym stwierdzeniu i spisaniu faktów indywidualnych, choć nie ma żadnego prawdopodobieństwa, żeby one się kiedykolwiek tak samo powtórzyły (Smoluchowski 1917b, s. 10).

Jak widać, jest to zadanie zbliżone do pierwszego typu historiografii nauki. Z takiego postawienia sprawy widać, że Smoluchowski, choć znał doskonale fizykę, to posiadał jedynie stereotypowe wyobrażenia o pracy historyka. Łatwo można dostrzec, że konieczność stawiania wyjaśnień, którą postuluje Smoluchowski w drugim typie badań, jest w zasadzie typowa dla pozytywistycznego nastawienia do historiografii (por np. Topolski 2008, s. 131). To pozytywistyczne nastawienie historiograficzne prowadzi w oczywisty sposób do tego, że działalność historyka nauki drugiego typu jest bliższa charakteryzowanej przez Smoluchowskiego pracy fizyka (poszukiwanie prawidłowości). Powtórzmy też, że nosi ona również znamiona pracy filozofa poprzez odkrywanie ogólnych założeń działalności naukowej. Zresztą Smoluchowski chętnie używał w kontekście rozważań historycznych terminu ‘filozofia przyrody’ („jak Anglicy słusznie powiadają...”), aby określić owe najogólniejsze kwestie dotyczące fizyki. Historia nauki jest więc dyscypliną, która stanowi pomost między naukami humanistycznymi i fizyką. Dodajmy, że do tego aspektu koncepcji Smoluchowskiego odwoływał się czterdzieści lat później Armin Teske (1957).

Przeglądając spuściznę Smoluchowskiego, można dostrzec, że uprawiał oba typy historii (utrwalił np. sylwetki kilku wybitnych uczonych), ale zdecydowanie przeważał w jego działalności typ drugi. Można stwierdzić, że interesował go rodzaj racjonalnej rekonstrukcji rozwoju idei i teorii naukowych. Interesujące światło na tę kwestię rzuca poniższy cytat pochodzący z *Poradnika dla samouków*:

> Właściwa bowiem historja fizyki nie powinna się ograniczać do opisywania życiorysów uczonych i analizowania dzieł, przez nich napisanych, ani do wyliczania wynalazków, lecz powinna uplastyczniać nam stopniowy rozwój poglądów naukowych i związek jego z ogólnym stanem kultury umysłowej (Smoluchowski 1917c, s. 288).

Historia nauki nie jest więc tylko próbą „plastycznej" rekonstrukcji rozwoju poglądów naukowych, ale musi być osadzona w szerokim kontekście kultury umysłowej, jest bowiem jej istotną częścią, której nie da się zupełnie wyizolować z całości. Zapewne taka koncepcja historii fizyki była podyktowana osobistymi zainteresowaniami uczonego, który wprost pisał:

> Jest to rzeczą ogromnie pouczającą śledzić zmienne losy teoryi naukowych; są one ciekawsze od zmiennych losów ludzi, bo każda z nich zawiera w sobie coś nieśmiertelnego, choć pewną cząstkę wiecznej prawdy (Smoluchowski 1913, s. 357).

Tak pojęta historia nauki była dla niego również najlepszym narzędziem analizy, które pozwalało w pełni ukazać istotne aspekty danej teorii. Doskonałym przykładem takiej analitycznej historii nauki jest artykuł (Smoluchowski 1899), w którym zrekonstruowana została historia badań przewodnictwa cieplnego gazów, oraz artykuł (Smoluchowski 1914) ukazujący m.in. historię badań fluktuacji termodynamicznych i ruchów Browna. Rekonstrukcja rozwoju teorii oraz towarzyszącego temu procesowi ciągu eksperymentów służyła polskiemu uczonemu do wskazania kierunków dalszych badań, zarówno teoretycznych, jak i doświadczalnych:

> Ze sprawozdania tego okaże się, co wobec zawiłych kwestyj doświadczalnych i teoretycznych można uważać za rezultat pewny, oraz w jakim kierunku dalsze prace prowadzone być muszą (Smoluchowski 1899, s. 33).

Taka historia nauki pozwala zrozumieć aktualny stan problematyki – to niezbędny krok, aby celowo (a nie na ślepo) podejmować dalsze poszukiwania. Rekonstrukcja historii badania i wyjaśniania zagadnień naukowych odsłaniała przy okazji zagadnienia filozoficzne w fizyce (np. zagadnienia teoriopoznawcze, zob. Smoluchowski 1914, s. 187).

Większość uwag filozoficznych Smoluchowskiego odnajdziemy właśnie w bliższym lub dalszym kontekście historycznym.

Można również pokusić się o konstatację, że historyczny punkt widzenia ewolucji zagadnień fizycznych był jednym z czynników decydujących o specyfice uprawiania fizyki przez Smoluchowskiego. Podobnie jak E. Mach dostrzegał on głębokie znaczenie ewolucji koncepcji. Natomiast źródeł wyjątkowości podejścia Smoluchowskiego należy szukać w jego wybitnych zdolnościach matematycznych, które pozwalały mu dostrzegać głębokie związki struktur matematycznych[5]. W procesie rozwoju teorii dostrzegał on ewolucję struktur i wykorzystywał to w swych wykładach akademickich, aby poprzez śledzenie rozwoju struktur i ich interpretacji prowadzić słuchacza do zrozumienia finalnej teorii (zob. np. Smoluchowski 1910).

W odczycie „O metodach fizyki doświadczalnej" (wygłoszonym w 1913 r.) Smoluchowski posłużył się charakterystycznym pojęciem „historia wiedzy" (Smoluchowski 2017a, s. 255). Wskazuje ono na obiektywistyczny rys historiografii nauki i sugeruje, że rozwój nauki redukuje się poprzez to pojęcie do rozwoju wiedzy naukowej, pojęcie wiedzy traktowane jest tu jednak bardzo szeroko. Smoluchowski odwoływał się w tym samym referacie do różnorodnych aspektów fizyki: historii badań ujawniających kwestie metodologiczne, historii zasad mechaniki, historii badań eksperymentalnych.

4. Kwestia źródeł i warsztatu historyka nauki

Źródła są podstawą historiografii[6], warto zatem postawić pytanie o to, czy i jakie źródła wykorzystywał Smoluchowski w swych rozważaniach historycznych. Zasadniczą trudność w rozstrzygnięciu tej kwestii stwarza fakt, że w wykładach i pracach z reguły przemilczane są źródła, na które powołuje się polski fizyk. Jedynie w przypadku *Poradnika dla samouków* znaleźć możemy spis prac z historii nauki, zalecany przez Smoluchowskiego do studiów tego zagadnienia. Można więc domniemywać, że wymienione tam prace były jednym ze źródeł wiedzy

[5] Podkreślano to w licznych wspomnieniach: Loria 1953, ss. 10–11, 35–36; Gostkowski 1953, ss. 234–235. Przykładami takich pogłębionych opracowań są również wspomniane artykuły (Smoluchowski 1899; 1914).

[6] Zob. np. Kragh 1987; Kokowski 2019, Appendix 1, s. 406.

Smoluchowskiego. W terminologii Kragha będą to zatem źródła drugorzędne. Z pewnością Smoluchowski korzystał też ze źródeł pierwszorzędnych, którymi były dla niego oryginalne publikacje naukowe w postaci książek i artykułów[7]. Z dokładności i szczegółowości stawianych niekiedy uwag można wnioskować o tym, że Smoluchowski często czytał prace źródłowe (nie wyklucza to jednak faktu, że niektóre ustalenia historiograficzne powtarzał za źródłami drugorzędnymi). Inną poszlaką jest podkreślenie wartości wznowień oryginalnych prac badawczych zawarte również w *Poradniku dla samouków*.

Trudno natomiast ustalić dziś, czy polski fizyk prowadził samodzielnie jakiekolwiek studia nad innymi typami źródeł historii nauki, jak np. korespondencja, niepublikowane rękopisy itd. Być może wnikliwa analiza dzienników oraz korespondencji Smoluchowskiego pozwoli w przyszłości rzucić nieco światła na to zagadnienie.

Przyglądając się jedynemu większemu tekstowi Smoluchowskiego poświęconemu wyłącznie historii nauki, jakim jest krótki, niespełna dziesięciostronicowy „Zarys dziejów fizyki w Polsce" (Smoluchowski 1917a), można odnieść wrażenie braku odpowiedniego warsztatu historycznego Smoluchowskiego. Błędem byłoby jednak ocenianie tej publikacji w kategoriach artykułu badawczego. Cel Smoluchowskiego zbliżony był do encyklopedycznego. Chodziło o syntetyczne i skrótowe przedstawienie historii fizyki w Polsce – „szkicowy obraz ogólny historycznego rozwoju tej nauki u nas" (Smoluchowski 1917a, s. 300). Biorąc pod uwagę ówczesny stan badań nad historią nauki, łatwo zrozumieć wiele nieścisłości i nietrafnych opinii powtarzanych przez Smoluchowskiego. Tekst ten miał być jednak raczej rodzajem zachęty i przewodnika do własnych badań, których konieczność wybitny fizyk dostrzegał bardzo wyraźnie:

> Samouk, mający zamiar zająć się gruntowniej historją fizyki w Polsce, musi się podjąć pewnego rodzaju pracy źródłowej, gdyż na polu tym **prawie wszystko jeszcze pozostaje do zrobienia** (Smoluchowski 1917a, s. 309, wyróżnienie P.P.).

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[7] O szeroko zakrojonych pracach, z których tylko część została wykorzystana w ostatecznym opracowaniu, wspominał w prywatnym liście do swego przyjaciela Kazimierza Twardowskiego z 25 lutego 1917 r. (w zbiorach Połączonych Bibliotek WFiS UW, IFiS PAN i PTF, jedn. Archiwum Kazimierza Twardowskiego, sygn. K-02-1-33, kk. 169–169v). Smoluchowski uskarżał się również na problemy

Fakt, że w działalności samokształceniowej pokładano nadzieję na rozwój badań źródłowych nad historią fizyki w Polsce, stanowi sam w sobie wymowne świadectwo stanu polskiej historiografii nauki u zarania niepodległości. Trzeba koniecznie dodać, że choć prezentowane uwagi kierowane były głównie do samouków lub jako pomoc dydaktyczna dla nauczycieli, to ujawniła się w nich troska Smoluchowskiego o warsztat naukowy historyka – odsyłał czytelnika do zasad opisanych wcześniej przy okazji pokrewnych badań nad historią matematyki i astronomii. Innymi słowy, historia fizyki polskiej, w kształcie, o którym myślał Smoluchowski, miała zostać dopiero napisana.

5. Działalność Smoluchowskiego na tle polskiej historiografii fizyki początku XX wieku

Dobrą perspektywę oceny wkładu Smoluchowskiego w rozwój polskiej historii nauki daje porównanie go z ówczesnymi polskimi próbami historiografii fizyki. Proponuję więc skupienie uwagi na kilku ważnych przykładach, które pozwolą lepiej zrozumieć znaczenie skromnej próby syntezy opublikowanej przez Smoluchowskiego oraz jego nawiązań do historii fizyki czynionych przy innych okazjach.

Śledząc rozwój polskiej historiografii nauki, można stwierdzić, że od początku pojawiło się w niej podejście zbliżone do scharakteryzowanego przez Smoluchowskiego podejścia „antyfaktograficznego". Już w artykule Osińskiego (1802) możemy znaleźć próbę napisania historii najważniejszych eksperymentów, idei oraz teorii w fizyce XVIII wieku. Cele, jakie stawiał sobie Osiński, są nieco podobne do celów Smoluchowskiego. Niemniej jednak za przeszło sto lat późniejszą próbą Smoluchowskiego stoi odwołanie do bogatej już literatury przedmiotu i kieruje nią szerszy obraz fizyki. Smoluchowski jako pierwszy dostrzegł też filozoficzne znaczenie historii fizyki, co ukazuje jeden z ważniejszych aspektów oryginalności jego podejścia.

Wiele o znaczeniu i poziomie opracowania Smoluchowskiego mówią wcześniejsze wydania *Poradnika dla samouków*. W pierwszym wydaniu

pełne negatywnych emocji (co jest u niego rzadkością) stwierdzenie: „przeklinałem pracę […] redakcyjną, a w ogóle wątpię, czybym(!) był się zdecydował na podjęcie się tego zadania, gdybym był miał pojęcie, ile(?) mnie ono będzie kosztować pracy" (tamże, k. 169). Ostatnia uwaga skłania do ostrożności w przypisywaniu różnorodnych błędów w tej publikacji (np. błędów w pisowni nazwisk uczonych) jedynie Smoluchowskiemu.

w ogóle nie pojawiła się kwestia historii fizyki, natomiast w drugim, zasadniczo zmienionym, Wiktor Biernacki ulokował ją w najwyższym IV stopniu edukacji odpowiadającym edukacji uniwersyteckiej. Niemniej zagadnieniu temu poświęcił bardzo mało miejsca, wymieniając jedynie dwie publikacje godne uwagi: *Biographisch-Litterarisches Handwörterbuch* J.C. Poggendorffa oraz *Geschichte der Physik* F. Rosenbergera (Biernacki 1901, ss. 73–74). Jak widać, opracowanie Smoluchowskiego wydane 16 lat później stanowiło ogromny jakościowy skok w rozumieniu znaczenia i roli historii fizyki. Z drugiej jednak strony, jeśli porównać tekst Smoluchowskiego z nieco wcześniejszymi opracowaniami Stefana Kwietniewskiego dotyczącymi historii matematyki i historii matematyki w Polsce (Kwietniewski 1915a, ss. 490–512; 1915b, ss. 513–528) (opublikowanymi w tej samej serii *Poradników*), to widać wyraźnie, że opracowania dotyczące historii matematyki górują zarówno obszernością, jak i poziomem refleksji metodologicznej nad historią fizyki w ujęciu Smoluchowskiego. Trzeba jednak lojalnie przyznać, że Smoluchowski odsyła czytelnika do tegoż opracowania, uznał bowiem, że kwestie warsztatowe zostały tam dobrze opracowane. Wydaje się, że to pragmatyczne rozwiązanie było uzasadnione w sytuacji, w której samodzielnie pisał tekst o całej fizyce, gdy tekst o matematyce podzielony był między kilku uczonych. Zresztą choćby systematyka źródeł historii matematyki mogła uchodzić za bardzo dobrą i stosującą się również do historii fizyki. Podsumowując, można stwierdzić, że kształt historii fizyki, jaki znajdziemy w *Poradniku dla samouków*, nie jest z pewnością ostatnim słowem w tej kwestii, które chciałby wypowiedzieć Smoluchowski. Jak wiemy, nagła śmierć przerwała wiele jego ważnych planów naukowych. To dodatkowa trudność w ocenie dokonań Smoluchowskiego, którą koniecznie trzeba wziąć pod uwagę.

Warto odnotować, że umiejscawianie historii nauki w odczytach publicznych i wykładach nie odbiegało zbytnio od innych ówczesnych ujęć. Porównując odwołania do historii fizyki u Smoluchowskiego z dłuższym artykułem Ludwika Brunera (1907) opublikowanym mniej więcej w tym samym czasie co odczyty i wykłady Smoluchowskiego, można zauważyć wiele podobieństw w podejściu do historiografii fizyki, które dziś może wydawać się anachroniczne[8]. Tym, co zdecydowanie

[8] Na marginesie warto zaznaczyć, że Bruner więcej uwagi od Smoluchowskiego poświęcił kwestii konkretnej aparatury badawczej, zamieszczając nawet wybrane ilustracje w tekście. Różnica może jednak wynikać z odmiennego charakteru źródeł –

wyróżniało Smoluchowskiego od Brunera, była z pewnością bardziej zaawansowana refleksja nad koncepcją historii fizyki oraz jej rolą. Tym aspektom decydującym o specyfice podejścia wybitnego fizyka poświęcone zostały kolejne części niniejszej publikacji.

Koniecznie należy również zestawić „Zarys..." pióra Smoluchowskiego z wymienianym przez niego „jako jedyne opracowanie ogólne całego przedmiotu" historii fizyki w Polsce artykułem Stanisława Kramsztyka „Dzieje fizyki w Polsce" opublikowanym w *Wielkiej Ilustrowanej Encyklopedii Powszechnej* (Kramsztyk 1898). W tym opracowaniu, wymienianym przez Smoluchowskiego jako jedyne, które znał w czasie pisania swego „Zarysu...", historia fizyki w Polsce zredukowana została w zasadzie do historii podręczników fizyki. Encyklopedyczny cel publikacji spowodował, że jest to w zasadzie jedynie spis kolejnych podręczników opatrzonych niekiedy drobnymi uwagami. Jednej tylko książce Drzewińskiego poświęcono więcej uwagi. Poza ten schemat Kramsztyk wykroczył w zakończeniu, wspominając 24 autorów polskich publikacji badawczych z fizyki powstałych w ostatnich dekadach XIX wieku. Historia fizyki zredukowała się u Kramsztyka jedynie do historii publikacji podręcznikowych i autorów publikujących prace z fizyki. Pomijając tak silne zawężenie tematu oraz encyklopedyczny charakter opracowania, i tak wiele można dziś zarzucić tej publikacji w warstwie merytorycznej. Na tym tle synteza Smoluchowskiego jest jednak kamieniem milowym na drodze ku nowoczesnej historiografii fizyki. Z pewnością też praca Smoluchowskiego wyznaczała program badań nad historią fizyki w Polsce, czego nie uczyniła wcześniej żadna inna praca. Smoluchowski, w przeciwieństwie do Kramsztyka, korzystał też w wyraźny sposób z opracowań innych autorów dla zbudowania swej syntezy. Co prawda powtarzał bez krytycznego sprawdzenia pewne informacje, ale wybitny fizyk przede wszystkim dążył do kompletności i zrozumiałości tworzonego obrazu dziejów fizyki.

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większość rozważań historycznych Smoluchowskiego związana była z wykładami, a rozdziały w *Poradniku* skupione były na pisanych źródłach historii fizyki i opracowaniach, zatem z założenia pomijały kwestię historii aparatury badawczej. Trudno wyrokować na tej podstawie o braku zainteresowania Smoluchowskiego historią aparatury, gdyż w wykładzie o metodach fizyki doświadczalnej przytaczał wiele ważnych przykładów historycznej aparatury w argumentacji dotyczącej specyfiki badań eksperymentalnych w fizyce. To interesujące zagadnienie wymaga jednak osobnych, pogłębionych studiów nad całością spuścizny Smoluchowskiego, które nie są wciąż jednak obecnie możliwe.

6. Kulturowe znaczenie historii nauki

> Historja fizyki jest historją – nie zaś fizyką. Tym pozornie trywialnym powiedzeniem zaznaczyć pragniemy różnicę w celach, w metodach i w znaczeniu tych nauk (Smoluchowski 1917c, s. 287).

Takimi słowami rozpoczął Smoluchowski wstęp do działu poświęconego historii fizyki w *Poradniku dla samouków*. Odrzucał on jakąkolwiek zależność wyników badań fizycznych od badań historycznych, podważał też celowość gruntownych studiów bibliograficznych przed przystąpieniem do rozwiązania zagadnienia fizycznego. Niezależność fizyki od jej historii podkreślił dosadnym stwierdzeniem: „dla badacza dzisiejszego zaś jedynym źródłem poznania jest przyroda sama, a jedynym autorytetem jego własny umysł", po czym jeszcze dodał: „żadne z wielkich odkryć nowoczesnych nie wyrosło z badania historycznego" (Smoluchowski 1917c, s. 287).

Dlaczego więc zawarł Smoluchowski w wykładzie dla ostatniego, III stopnia część poświęconą historii fizyki oraz historii fizyki w Polsce? Czy był to jedynie popis erudycji albo zbiór ciekawostek dla studiujących? Z pewnością Smoluchowski nie traciłby tak wiele czasu na zagadnienie, które nie miałoby istotnego znaczenia. Owo znaczenie określił on mianem „ogólno-kulturalne". Taki sposób widzenia historii nauki, w którym jest ona „najwspanialszym działem historii kultury", jest typowy dla oświeceniowo-pozytywistycznej tradycji filozoficznej. Postęp cywilizacyjny uzyskiwany dzięki nauce określił Smoluchowski jako „prawdziwszy wskaźnik postępu cywilizacji, niż historia królów i wojen" (Smoluchowski 1917c, s. 288).

W tym kontekście zrozumiałe staje się, dlaczego Smoluchowski poświęcił tak wiele miejsca historii fizyki w Polsce. Zadanie to miało wyraźny cel patriotyczny – ukazywało i propagowało dorobek kulturowy Polaków, przy okazji też prostując niektóre nadużycia historiografii niemieckiej powodowane rozbudzonym nacjonalizmem[9]. Taki sposób widzenia historii nauki zbliżał Smoluchowskiego do poglądów na społeczną i patriotyczną rolę nauki typowych dla polskich pozytywistów.

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[9] Komentując książkę A. Hellera, napisał: „Razi niemiecka tendencyjność w przedstawieniu kwestji narodowości Kopernika" (Smoluchowski 1917c, s. 291).

Dostrzegał on równocześnie, że ten dział historii kultury narodowej jest niedoceniany przez rodaków,

> mimo że poszukiwania historyczne na innych polach – zwłaszcza literatury i sztuki – tak wielką u nas cieszą się popularnością (Smoluchowski 1917a, s. 300).

Najważniejszym zadaniem historii nauki według Smoluchowskiego – i to zadaniem o znaczeniu ogólnokulturowym – było kształcenie postaw antydogmatycznych.

> Jest to rzeczą wielkiej doniosłości, żeby uczący się nie uważał nauki za system dany dogmatycznie, za jakieś objawienie boskie (Smoluchowski 1917c, s. 289).

Na gruncie fizyki ów antydogmatyzm był dla Smoluchowskiego jedną z najważniejszych cech badacza, typową dla charakteryzowanej przez niego postawy romantyzmu naukowego. Interesujące jest to, że zaledwie kilkanaście lat po sformułowaniu przełomowych teorii fizycznych podważających XIX-wieczną wizję niezmiennych praw naukowych do nauczania wprowadzono tezy o konieczności akceptacji zmienności teorii i uczono otwartości na ciągłą rewizję wiedzy. Antydogmatyzm wprowadzany poprzez historię nauki pozwalał również na krytyczne podejście do istniejących, niekiedy szkodliwych tradycji metodologicznych, np. pozwalał krytykować Smoluchowskiemu niemiecki podział na fizykę teoretyczną i doświadczalną (Smoluchowski 2017a). Roli historii fizyki dla metodologii oraz filozofii fizyki przyjrzymy się bliżej w jednej z kolejnych części niniejszego tekstu.

Odnotujmy tu natomiast, że w dobie gdy Smoluchowski publikował *Poradnik dla samouków*, trwała wojna światowa, w której wyładowywał się szowinizm i fanatyzm narodowy. Znamienna jest w tym kontekście jeszcze jedna uwaga o roli antydogmatyzmu historii nauki: „ostrzega go przed fanatyczną wiarą w pewien system i uczy go tolerancji dla innych zapatrywań". Co prawda uwaga skierowana została oryginalnie do badacza, ale opisane uprzednio kulturowe znaczenie nauki wskazuje, że może mieć ona też istotne zastosowanie w szerszym kontekście.

7. Historia nauki w służbie dydaktyki

Pojmując naukę jako część kultury, Smoluchowski dostrzegał dydaktyczne znaczenie historii nauki w fizyce. Wskazywał, że droga historyczna jest najdogodniejsza do zrozumienia skomplikowanych teorii nowoczesnej fizyki, ponieważ jest drogą „najnaturalniejszą". Dostrzegał więc, że posługiwanie się historią nauki jako narzędziem dydaktycznym może warunkować możliwość coraz szybszego rozwoju fizyki. Posługiwał się również odwołaniami do kontekstu historycznego w wykładach popularyzujących wiedzę fizyczną, choć były to niekiedy wykłady na wysokim poziomie merytorycznym, kierowane do środowisk techników lub nauczycieli fizyki.

Smoluchowski doceniał również dydaktyczne znaczenie biografii naukowców, widząc w nich źródło inspiracji dla kolejnych pokoleń adeptów fizyki. W ten sposób historia fizyki lokowała się w kręgu żywotnych zainteresowań osób, którym leżał na sercu rozwój fizyki.

Wybitny uczony dostrzegał również znaczenie krytycznych wydań oryginalnych prac – choć są one przede wszystkim istotne dla badań historycznych, to posiadają również pewne znaczenie dla samej fizyki. Interesujące jest uzasadnienie Smoluchowskiego:

> Często przeczytanie tekstu oryginalnego daje najlepszy sposób poinformowania się o przedmiocie samym, a w każdym razie przerobienie wzorowej pracy otwiera pouczające perspektywy na sposób, w jaki dokonywają się wielkie odkrycia (Smoluchowski 1917c, s. 298).

Teksty oryginalne nie tylko dają możliwość zapoznania się z sytuacją problemową w takim kształcie, jaki miała w czasie utworzenia przełomowego rozwiązania. Pozwalają one również na śledzenie samego procesu twórczego, czego nie daje nigdy tekst podręcznikowy. Zapoznawanie się z tekstami oryginalnych prac jest zatem metodą pobudzania twórczości naukowej. Za doskonały przykład uznawał polskie wydawnictwo *Z dziejów rozwoju fizyki*, o którym pisał, że wybór tekstów źródłowych „pobudza […] zainteresowanie czytelnika, łącząc go więzami sympatji osobistej z autorami ustępów cytowanych i ożywia wykład rzeczy naukowej" (Smoluchowski 1917d, s. 137).

Bardziej od publikacji poszczególnych prac Smoluchowski doceniał wagę wydawania dzieł zebranych „wybitniejszych autorów", które pozwalały zapoznawać się gruntownie z twórczością godnych naśladowania uczonych. Niekiedy zbiory takie posiadały według niego „wartość aktualną" – miał tu na myśli m.in. prace Boltzmanna, Kelvina, Rayleigha. Z kontekstu *Poradnika dla samouków* można domyślać się, że prace tych uczonych poruszały tematy, które były aktualne jeszcze w drugiej dekadzie XX wieku i mogły służyć za źródło inspiracji dla samodzielnej pracy naukowej.

Smoluchowski uważał, że historia fizyki spełnia ważną rolę dydaktyczną również na kilku innych polach. Świadczą o tym opublikowane niedawno teksty jego odczytów. Uczony wskazywał, że historia doskonale ukazuje różnice podejść badawczych i ich źródła (Smoluchowski 2017b). Historia nauki ukazuje również rolę spekulacji teoretycznej w fizyce, co było ważne w kontekście antyteoretycznych stanowisk empiriokrytyków. W wykładach akademickich Smoluchowski wskazywał, że geneza teorii pozwala ukazać jej istotne cechy i fundamentalne założenia (np. Smoluchowski 1908). Zwracał też uwagę na to, że historia fizyki, ukazując ewolucję pomysłów, ułatwia zrozumienie ostatecznej, abstrakcyjnej teorii – dobrym przykładem zastosowania tej idei jest struktura wykładu teorii Maxwella i teorii elektronowej (Smoluchowski 1910). Innym świadectwem tego, że Smoluchowski w praktyce docenił znaczenie historii nauki dla dydaktyki fizyki, są odpowiednie rozdziały w *Poradniku dla samouków* (Smoluchowski 1917a; 1917c).

8. Filozoficzne znaczenie historii nauki w ujęciu Smoluchowskiego

Co prawda kulturowe znaczenie historii fizyki według Smoluchowskiego było najważniejsze, jednak uczonego o wiele bardziej zajmowało filozoficzne znaczenie tej dziedziny. Smoluchowski był bardzo ostrożny w używaniu terminu „filozofia", co widać zarówno w jego najwcześniejszym, jak i w ostatnim manuskrypcie (Smoluchowski 2017c; 2017d). Uprawianą przez siebie refleksję filozoficzną ściśle związaną z własnymi badaniami naukowymi odróżniał w ten sposób od rozbudowanych koncepcji filozoficznych mających luźny związek z praktyką badawczą fizyki[10]. W tym kontekście warto więc podkreślić, że w *Poradniku dla*

[10] Więcej na temat filozofii można znaleźć w opracowaniach Polak, Dziekan 2017; Polak 2009. Z licznymi zastrzeżeniami należy przyjąć natomiast opracowanie Krajewski 1956, w którym dokonano poważnych i celowych nadinterpretacji oraz wypaczeń myśli

samouków wprost scharakteryzował filozoficzne znaczenie historii nauki, koncentrując się – zgodnie z ówczesnymi tendencjami filozoficznymi – na aspektach epistemologicznych nauki[11]:

> Co się tyczy stosunku historji fizyki do innych nauk, podnieść należy zwłaszcza znaczenie jej dla filozofji (psychologji oraz teorji poznania). Historja fizyki jest ciągłym szeregiem przemian w poglądach naukowych i dlatego nadaje się bardzo jako punkt zaczepienia dla krytycznej analizy podstaw nauki i dla roztrząsań metod naukowych (Smoluchowski 1917c, s. 289).

Przy okazji Smoluchowski wskazał też nie wprost dwa główne źródła inspiracji swych przemyśleń nad rolą historii fizyki, pisząc:

> Istotnie też pierwszorzędni uczeni, jak Mach i Duhem, łączą krytykę podstaw z badaniem historycznym, dostarczającym ogólnego tła i specjalnych ilustracji do wywodów ogólnych (Smoluchowski 1917c, s. 289).

Charakterystyczną cechą stylu naukowego Smoluchowskiego jest to, że do historii fizyki najczęściej odwoływał się w odczytach, aby ukazać historyczno-filozoficzne tło zagadnienia. Jednym z najważniejszych motywów była kwestia podkreślanego przez niego wielokrotnie przełomu w ówczesnej fizyce, który wprowadzała teoria kinetyczno-atomistyczna. Dzięki kontekstowi historycznemu można było adekwatnie zrozumieć wagę przełomu i określić, jakie poglądy ogólne musiały zostać poddane rewizji w wyniku przyjęcia nowej teorii.

Drugie bardzo ważne zadanie historii nauki Smoluchowski scharakteryzował w odczycie inauguracyjnym na kursie uzupełniającym dla

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Smoluchowskiego, aby uczynić zeń „nieuświadomionego materialistę". Jeśli pominąć liczne wyrazy hołdu spłaconego przez Krajewskiego wobec panującego wówczas systemu politycznego, należy zaznaczyć, że wiele jego analiz zawartych w rozdziale IV jest wartościowych i stanowią o tym, że warto po tę lekturę ostrożnie sięgnąć, mimo że jest ona smutnym świadectwem swoich czasów.

[11] Należy nadmienić, że dowartościowanie roli teorii poznania wiązało się również z poglądami Smoluchowskiego na dydaktykę fizyki. Dostrzegał on duże znaczenie teorii poznania jako podstawy do nauczania metodologii fizyki i doceniał w tym kontekście znaczenie propedeutyki filozofii w szkołach (por. Smoluchowski 1917d, s. 101).

nauczycieli szkół średnich we Lwowie z dnia 12 marca 1913 r. Twierdził wówczas, że jest nim odkrywanie roli „ogólnego nastroju umysłowego epoki" zwanego też często zamiennie „ogólnym prądem naukowo-filozoficznym" (Smoluchowski 1913). Pod tym pojęciem kryły się u Smoluchowskiego epistemiczne wzorce działalności naukowej, jak i zbiór dominujących założeń filozoficznych. Stosowne wydaje się również rozpatrywanie owego „ogólnego nastroju umysłowego" w kategoriach G. Holtona, jako zespołu podzielanych *themata*.

Według Smoluchowskiego historia pokazuje też, że pewne idee schodzą na drugi plan i są tłumione przez „ogólny nastrój". Przykładem były bliskie mu poglądy L. Boltzmanna. Smoluchowski był jednak konsekwentny w uznaniu decydującej roli czynników internalistycznych w rozwoju fizyki. Choć „ogólny nastrój epoki" może mieć przemożny wpływ na działalność badaczy, to ostatecznie i tak zawsze decydującą rolę odgrywają „argumenty faktyczne" (empiryczno-teoretyczne). W tym kontekście zrozumiałe staje się, że historia nauki ukazuje niekiedy toczącą się na łonie nauki walkę o charakterze światopoglądowym. Smoluchowski w wykładzie wygłoszonym w Getyndze w 1914 r. odwoływał się do żywotnie dlań ważnego przykładu sporu światopoglądu „termodynamiczno-energetycznego" z „atomistyczno-kinetycznym" (Smoluchowski 1956). Co ciekawe, w tym wykładzie skierowanym do fizyków Smoluchowski twierdził, że odwołanie do historii jest konieczne, aby zrozumieć współczesny stan teorii atomistycznej – dostrzegał więc rolę elementów tradycji w badaniach fizycznych, które ujawniają się w okresach przełomowych.

Wspomniałem już, że według Smoluchowskiego historia nauki pomagała kształtować i wspierała antydogmatyczną postawę naukowców w ich pracach, tak konieczną do rewizji ukrytych filozoficznych założeń. Historia fizyki pozwala więc odkrywać filozoficzną rolę tej nauki. Na przykład w wykładzie „O teoryach elektryczności" wygłoszonym we Lwowie w roku 1901 ukazywał obalenie naiwnych wyjaśnień substancjalistycznych natury ciepła poprzez doświadczenia i obserwacje Rumforda.

Smoluchowski zauważył również, że historia nauki wiedzie do pytań o prostotę praw i prostotę samej przyrody. Dlaczego znajdujemy ową prostotę w rzeczywistości fizycznej? W wykładzie „O metodach fizyki doświadczalnej", wygłoszonym w 1913 r. w UJ, odpowiadał, że decydują o tym obiektywne „własności matematyczno-geometryczne"

(Smoluchowski 2017a, ss. 262–263), unikał jednak z różnych względów rozwijania tego tematu.

Bez wątpienia najważniejszym obszarem refleksji filozoficznej była dla Smoluchowskiego metodologia fizyki. Tutaj wielokrotnie wykorzystywał odniesienia do historii fizyki. Historia ukazuje bowiem, jak wiele teorii odrzucono – w tej perspektywie pojawiają się pytania o status, rolę i cel teorii (Smoluchowski 2017b). Dzieje fizyki Smoluchowski postrzegał jako ciągłą ewolucję wyjaśnień naukowych, w obrazie tym rewolucje są tylko gwałtownie przyspieszającą ewolucją (Smoluchowski 1907). Zasadnicza nieciągłość występować może na płaszczyźnie światopoglądowej, natomiast kolejne teorie są próbami dostarczenia coraz lepszych wyjaśnień w świetle nowych obserwacji i zmian struktury teoretycznej dokonujących się na innych obszarach.

Smoluchowski w pełni akceptował rewidowalność całej wiedzy naukowej. Już w 1900 r. w odczycie wygłoszonym podczas walnego posiedzenia Towarzystwa Przyrodników im. Kopernika, zatytułowanym „O wynikach nowszych badań nad promieniowaniem", podważał pogląd o niezmienności teorii fizycznych, przywołując przykład optyki (Smoluchowski 1900, ss. 86–87). Natomiast w późniejszym odczycie „Ewolucja teoryi atomistycznej" (Smoluchowski 1911) wygłoszonym w Polskiej Akademii Umiejętności w Krakowie w roku 1911 traktował naukę jako zbiór wygodnych hipotez. Zdecydowanie stwierdzał, że „o wszystkim wolno wątpić". W tym odczycie dał również świadectwo temu, że rozumiał holistyczną naturę systemu wyjaśnień teoretycznych w fizyce – być może inspirował się w tej kwestii poglądami Duhema, którego wielokrotnie przywoływał w swych pracach.

Smoluchowski sugerował również, że historia fizyki pozwala rozstrzygnąć kwestię, czy praktyczna użyteczność teorii decyduje o ocenie jej wartości. Na przykładzie elektrostatyki wskazywał teorię, która była teoretycznie doniosła, ale pozostawała bez zastosowań praktycznych (Smoluchowski 2017b, s. 197). Na usprawiedliwienie wybitnego fizyka należy przypomnieć, że na początku XX wieku wydawało się, iż wiele zaawansowanych teorii fizycznych pozbawionych jest znaczenia praktycznego. Pogląd taki dominował m.in. we wczesnych fazach recepcji teorii względności A. Einsteina.

Historia nauki pełniła według Smoluchowskiego ważną rolę w analizach metodologii nauki (filozofii nauki). W bardzo ważnym wykładzie „O metodach fizyki doświadczalnej" (1913) ukazywał, że historia fizyki pełni rolę źródła materiału dla tego typu analiz (Smoluchowski 2017a, s. 268). Pozwala ona ustalić miejsce metody indukcji w fizyce – Smoluchowski dostrzegał rolę tej metody tylko w dziedzinach bez wypracowanej teorii. Na podstawie argumentów historycznych wskazywał również ograniczenia metody indukcji. Na historycznym przykładzie sformułowania teorii Maxwella polski fizyk ukazywał złożone relacje teoria – eksperyment.

Smoluchowski zauważył również, że niekiedy w konkretnych przypadkach trudno jednoznacznie określić metodę badania:

> W historyi fizyki oprócz tych dwóch typowych metod badania, indukcyjnej i dedukcyjnej (ze sprawdzeniem a posteriori) napotykamy też często przykłady, gdzie obie się splatają ze sobą w dziwny sposób. Przykładem tego jest n.p. odkrycie radu i polonu przez Państwo Curie (Smoluchowski 2017a, s. 268).

Widać, że Smoluchowski był świadomy złożoności problematyki metodologii fizyki, niestety nie rozwinął bardziej analizy tego zagadnienia.

Wybitny polski fizyk zwrócił również uwagę na pewien aspekt zwykle umykający uwadze metodologów fizyki. Wskazywał on na problem dokładności obserwacji ujawniający się w pełni dopiero w perspektywie historycznej. Przykład Keplera i Tycho de Brahe był dla niego doskonałą ilustracją tezy, że zbyt duża dokładność wyników pomiarowych może być przeszkodą w rozwoju fizyki – wymaga bowiem stworzenia zaawansowanej teorii wyjaśniającej (i w domyśle – odpowiedniej mocy obliczeniowej), która na danym etapie rozwoju może być nieosiągalna. Argumentację Smoluchowskiego można skrytykować, przytaczając przykłady obserwacji, które długo nie miały zadowalającego wyjaśnienia ani opisu teoretycznego, a mimo to nie zablokowały rozwoju fizyki i dopiero po wielu latach doczekały się wyjaśnień. Tak było na przykład ze zjawiskami określanymi dziś jako chaos deterministyczny. Wartość uwagi Smoluchowskiego bierze się natomiast z tego, że świadomie wskazał nowe, nie brane dotychczas pod uwagę uwarunkowania procesu tworzenia teorii. Dodajmy, że dla wybitnego fizyka stopniowe zwiększanie dokładności pomiarów pozwalać miało na tworzenie teorii coraz bliższych prawdy – rozwój fizyki miał więc charakter iteracyjny i aproksymacyjny, warunkowany możliwościami zarówno ulepszania pomiarów i doświadczeń, jak i tworzenia coraz bardziej wyrafinowanych teorii matematycznych. Owo przybliżanie się do prawdy wiązało się często z upraszczaniem struktury (formy) teorii. Ten aspekt historii fizyki jest ważny dla metodologa, ale z punktu widzenia celów dydaktycznych może niekiedy odgrywać szkodliwą rolę (Smoluchowski 1917c, s. 288, przyp. 1).

Perspektywa historyczna była również dla Smoluchowskiego istotna w ocenie roli specjalizacji w fizyce. To właśnie z historii zaczerpnął argument obalający naiwny pogląd o braku konieczności specjalizacji w fizyce:

> Narzekają ludzie, zwłaszcza laicy, często na przesadną specyalizacyę w nauce dzisiejszej. Historya atomistyki jest doskonałym dowodem, jak konieczna jest specyalizacya, jak drobiazgowe, precyzyjne zbadanie pewnych pozornie znikomych zjawisk [...] (Smoluchowski 1913, s. 371).

Powyższy przegląd wskazuje, jak wiele uwag filozoficznych poczynił Smoluchowski w kontekście rozważań historycznych. O ile więc sprzeciw budzić może jednoznaczne i dokonywane bez zastrzeżeń określanie Smoluchowskiego jako historyka nauki rozwijającego badania źródłowe, o tyle trzeba przyznać, że był fizykiem i myślicielem, który nader często wykorzystywał historię fizyki, przyczyniając się do rozwoju zainteresowania tą tematyką. Nie można również przyjąć, że historia fizyki pełniła u Smoluchowskiego rolę marginalną i drugorzędną. Perspektywa historyczna wywarła decydujący wpływ na oryginalność jego podejścia do fizyki[12], które nazywał on mianem „romantyzmu nauki". Takie rozszerzone spojrzenie na fizykę było inspirujące jeszcze cztery dekady po śmierci uczonego, czego dowodem jest interesujący artykuł historyka nauki Armina Teske, który ukazuje wpływ Smoluchowskiego jako historyka nauki.

9. Kontynuacja idei Smoluchowskiego w ujęciu Armina Teske

Na łamach *Kwartalnika Historii Nauki i Techniki* w roku 1957 opublikowano tłumaczenie artykułu „Historia i filozofia nauk przyrodniczych na studiach fizyki". Autorem tegoż był filozof Norwood Russell Hanson

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[12] Na oryginalność metody Smoluchowskiego związanej z twórczym wykorzystaniem odwołań do historii fizyki pierwszy zwrócił uwagę Armin Teske (1957).

z Uniwersytetu w Cambridge. Swoistym komentarzem i próbą wyartykułowania własnego stanowiska stał się natomiast artykuł Armina Teske opublikowany w tym samym zeszycie *Kwartalnika*. W artykule noszącym znamienny tytuł „O elementy humanistyczne w studiach nauk przyrodniczych" (Teske 1957) polski historyk nauki próbował ukazać historię nauki jako „naturalny pomost między naukami przyrodniczymi i humanistyką". W interesującym i ważnym tekście Teske odwoływał się do koncepcji historii fizyki i jej zastosowań u Smoluchowskiego. Powtarzał dość dokładnie myśli o historii fizyki jako szkole antydogmatyzmu. Inspiracje z pewnością nie są przypadkowe, gdyż postać Smoluchowskiego była w centrum zainteresowań badawczych Armina Teske. Według niego historia nauki w ujęciu Smoluchowskiego była prezentacją „wycinka z historii myśli ludzkiej".

Teske, porównując styl Smoluchowskiego i Einsteina, zauważył ważny cel eksplanacyjny odwołań do historii fizyki:

> Zapewne, Smoluchowski mógłby był niejedno z tego wyłożyć bez perspektywy historycznej przez puste, że tak powiem, anonimowe rozpatrzenie argumentów. Czy jednak ewolucja myśli nie daje obrazu pełniejszego? A ileż mniej żywy byłby wykład, ileż mniej poglądowy! (Teske 1957, s. 687).

Historia zagadnienia naukowego stanowi więc część niezbędnego wyjaśnienia w fizyce. Stąd elementy humanistyczne nie są tylko pomocą dydaktyczną, ale stanowią istotny element wyjaśnień:

> Przekonać się, że można do danego zjawiska wyprowadzić wzór, który sprawdza się doświadczalnie, to oczywiście rzecz ważna. Lecz dopiero wyjaśnienie związków [możliwe tylko w świetle historii] zaspokaja naszą potrzebę rozumowania (Teske 1957, s. 686).

Jak widać na tym przykładzie, idee Smoluchowskiego zainspirowały przynajmniej jednego historyka fizyki. Jest to kolejny ważki argument za tym, aby przybliżyć dokonania Smoluchowskiego związane z historią fizyki.

10. Zakończenie

Smoluchowskiego odwołania do historii fizyki, jak i pionierskie syntetyczne opracowanie dziejów fizyki w Polsce stanowiły na początku XX wieku ważne punkty we wczesnym etapie rozwoju historii fizyki w Polsce. Dzisiejszy stan tej dyscypliny nie pozwala adekwatnie dostrzec znaczenia tej części dorobku wybitnego fizyka. Stąd niniejsza praca jest próbą wyjścia poza oceny z perspektywy aktualizmu. Jest więc próbą nakreślenia szerszego obrazu jego poglądów na historiografię nauki i roli, jaką odegrała w jego działalności naukowej. W wielkim skrócie można powiedzieć, że Smoluchowski traktował działalność z zakresu historii nauki jako integralną część uprawiania nauki i jej dydaktyki. Pełniła ona również kluczową rolę w jego refleksji filozoficznej uprawianej na gruncie fizyki (odkrywanie założeń, znaczenie nauki dla metafizyki, metodologia). Porównując dokonania i koncepcje Smoluchowskiego z pracami jemu współczesnych, odkrywamy zapomniane już nieco znaczenie tego uczonego dla rozwoju rodzimej historiografii fizyki.

Niniejsza praca z pewnością nie wyczerpuje całości zagadnienia. Przedmiotem przyszłych systematycznych badań powinny być źródła inspiracji Smoluchowskiego w kwestii koncepcji i roli historii nauki. Konieczne jest w tym celu systematyczne przebadanie całej obfitej spuścizny rękopiśmiennej uczonego, która wciąż czeka na docenienie jej wagi zarówno dla fizyki, jak i dla polskiej humanistyki. Na zakończenie pozwolę sobie wyrazić nadzieję, że polscy humaniści docenią kiedyś, jak ważny wkład do kultury polskiej wniosły takie osoby jak wybitny fizyk Marian Smoluchowski.

11. Podziękowania

Autor chciałby złożyć podziękowania dla anonimowego Recenzenta I za celne uwagi, które wydatnie przyczyniły się do wzbogacenia treści niniejszego artykułu.

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Polish mathematicians and mathematics in World War I. Part II. Russian Empire

Abstract

In the second part of our article we continue presentation of individual fates of Polish mathematicians (in a broad sense) and the formation of modern Polish mathematical community against the background of the events of World War I. In particular we focus on the situations of Polish mathematicians in the Russian Empire (including those affiliated with the University of Warsaw, reactivated by Germans, and the Warsaw Polytechnic, founded already by Russians) and other countries.

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Polscy matematycy i polska matematyka w czasach I wojny światowej. Część II. Cesarstwo Rosyjskie

Abstrakt

W drugiej części artykułu kontynuujemy przedstawianie indywidualnych losów matematyków polskich (w szerokim sensie) oraz kształtowanie się nowoczesnego polskiego środowiska matematycznego na tle wydarzeń I wojny światowej. W szczególności skupiamy się na sytuacji matematyków polskich w Cesarstwie Rosyjskim (także tych związanych z reaktywowanym przez Niemców Uniwersytetem Warszawskim i utworzoną jeszcze przez Rosjan Politechniką Warszawską) i innych krajach.

Słowa kluczowe: *polskie środowisko matematyczne*, *I wojna światowa, Cesarstwo Rosyjskie*, *Towarzystwo Kursów Naukowych w Warszawie*, *Polskie Kolegium Uniwersyteckie w Kijowie*, *nauczanie akademickie poza tradycyjnymi instytucjami*, *Mathematics Subject Classification: 01A60, 01A70, 01A73, 01A74*

1. Introduction

This article extends the study of Polish mathematicians and mathematics in World War I done in Domoradzki, Stawiska 2018. As before, we do not use any clear-cut criteria here, either for "Polish mathematicians" or "Polish mathematics". Such criteria always seem inadequate (Tatarkiewicz 1998; Duda 2012), especially if applied to the years from 1795 until 1918, when there was no Poland on the political map of Europe. We already considered a number of men born in Galicia (or in the Polish Kingdom) and educated there, speaking Polish as one of their primary languages, including a few who were ethnically Jewish and one German. In this part we talk primarily about men and women from the Polish Kingdom, but also about individuals born in other lands of the Russian empire to Polish-speaking families. Finally, we consider some who spent the war years in other countries, on either side of the conflict (Germany, France and Italy[1]) or neutral (Switzerland).

The Russian partition of Poland consisted of the Polish Kingdom and Annexed Territories. The Kingdom of Poland was created in 1815 by the decisions of the Congress of Vienna from the territories of the former Duchy of Warsaw. First connected by personal union with Russia, it was gradually politically integrated into the Russian Empire, finally losing the remnants of its autonomy in 1867 as a result of the January Uprising (1863–1864). Since 1874 the country was officially referred to as "the Vistula Country" (Kraj Nadwiślański; Privislinskij Kraj).[2] This was the territory that initiated the most struggles for national independence, but all these efforts were lost and the country was subject to severe repressions. Some of them affected education and culture. All public high schools conducted instruction in Russian and the Main School in Warsaw gave way to the Russian-language Imperial University and Polytechnic. The use of Polish in official communication was forbidden. The administration and courts were staffed by Russians. Poles often had to look for education and career opportunities elsewhere, and many of them established themselves in other places in the Russian Empire. But the end of the 19th century in the Kingdom brought an amazing development of unofficial education in Polish at all levels, including academic one.

Russia entered the war as part of Triple Entente. Few university students and faculty were drafted into the army (it was not usual); some volunteered.[3] Academic teaching and research could go on, affected not so much by military operations of World War I,[4] but rather by compul-

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[1] Italy was a member of the Triple Alliance with Germany and Austro-Hungary, but in May 1915 it revoked the alliance and entered the war on the side of the Allied Powers.

[2] The name of "the Kingdom of Poland" was never formally abolished. Rarely used, it was preserved in the statute book "Svod Zakonov Rossijskoi Imperii".

[3] In Russia, the number of citizens mobilized for WWI was 13 million men, or 7.4% of the male population, plus 5,000 – 6,000 women (Beckett 2001). On the other hand, some Polish subjects of the Empire volunteered into Polish Legions, on the Austro-Hungarian side (Kutrzeba 1988).

[4] After 1917 educational activities inside Russian territories were disrupted by two revolutions and the civil war that ensued, as well as by the fighting for independent Ukraine, which created multiple short-lived Ukrainian states (Demidov 2015; Yekelchyk 2007).

sory evacuations,[5] travel restrictions, food and raw materials shortages. Even enemy aliens interned in the Empire could participate in scholarly activities under special circumstances (as did Wacław Sierpiński). New Polish academic institutions were organized (University of Warsaw, Warsaw Polytechnic, Polish University College in Kiev). Polish mathematical societies were established in Moscow and Kiev and talks were given. Monographs and textbooks appeared, journals were published (*Wiadomości Matematyczne*, *Prace Matematyczno-Fizyczne*, *Wektor*) or planned (*Fundamenta Mathematicae*, launched in 1920). Mathematicians were aided by physicists, astronomers, engineers and philosophers who in the circumstances of the war engaged in teaching mathematics at the academic level or in the activities of learned societies (this is why our definition of a "mathematician" here is somewhat broad). On the other hand, some mathematicians (Stanisław Leśniewski, Zygmunt Janiszewski) extended their activity to teaching at a high-school or elementary level when and where the need arose. A few also engaged themselves outside of mathematics and education: in political activities (Wiktor Staniewicz) or writing on cultural and religious Jewish themes (Chaim Müntz).

Did any of the scholars mentioned in this article or its previous part contribute their specific knowledge to the war effort in the years 1914–1918? It does not seem so, at least not directly. Leon Lichtenstein worked for the Siemens company in Berlin, which did support the German military effort, but he dealt with electric cables, not weapons or anything primarily associated with combat. His work, however, was deemed important enough to earn him German citizenship in the early days of the war.[6] No other mathematicians mentioned in our articles had ties to industry or institutions of war research. The military service in general did not call for higher mathematical skills, although basic knowledge of mathematics and engineering was required sometimes. Eustachy

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[5] The Russians evacuated about 130 industrial enterprises and 200 educational institutions – their personnel, equipment etc. – from the Polish Kingdom and part of Eastern Galicia, over 600 thousand people in total.

[6] His fate contrasts with that of Chaim Müntz, another Russian-born German-educated Polish-Jewish mathematician (known for the celebrated Müntz-Szasz theorem), who, despite being a serious researcher, educator and thinker, lost his job at a school in Hessen during the war.

Żyliński had to learn several engineering subjects in his officer's training in the Russian army in order to become an instructor to future officers. Stanisława Liliental (later Nikodymowa) taught basic mathematics[7] to Polish army recruits while on leave from her studies at the Warsaw University. Edward Stamm (see Domoradzki, Stawiska 2018 and the references therein) served in the Austrian army as a radiotelegraphist, officially translating cablegrams from French, English and Italian, but unofficially might have been involved in deciphering (we have no direct evidence of this, but in 1921 he published a treaty "On application of logic to the cipher theory" [O zastosowaniu logiki do teorii szyfrów]).

While we cannot find substantial evidence of anybody's research being directed by war needs, we can point out several instances of mathematical interests being influenced by war-related circumstances. The most notable cases are those of Stefan Banach,[8] Bronisław Knaster and Kazimierz Kuratowski. All had to interrupt their studies because of the war – Banach and Kuratowski in engineering, Knaster in medicine – but they found opportunities for pursuing mathematics and later became pillars of Lwów and Warsaw mathematical schools. Another interesting case is that of Tadeusz Banachiewicz, an astronomer working in Dorpat, who oriented his research in a theoretical direction after the instruments from his observatory were evacuated deeper inside the Russian Empire. We count him here because the interest in computational methods he developed at that time allowed him later to make some lasting contributions to mathematics (however, already in 1909 he published a paper concerning a problem in number theory). There were other cases of changing interests, but none of them suggestive of switching from pure mathematics to war-inspired applications. However, for Poland the war extended beyond November 11, 1918. The reborn state had to defend itself against the competing interests of its neighbors.

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[7] We were not able to find a curriculum of these courses. According to Aubin, Goldstein 2014, such training was also offered in other armies and included distance measurement and elements of ballistics. Popular texts were *Soldaten-Mathematik* by Alexander Wittig (Leipzig 1916) and *Elementary Mathematics for Field Artillery* by Lester R. Ford (Louisville, KY, 1919; first circulating as lecture notes).

[8] Banach's mathematical career was spurred by his serendipitous meeting with Hugo Steinhaus in 1916 in Kraków (Domoradzki, Stawiska 2018 and the references therein).

The fighting continued until 1921 (the Peace of Riga with Soviet Russia and Soviet Ukraine as well as the Third Silesian Uprising against Germany). During the Polish-Soviet war of 1920–21 Stefan Mazurkiewicz, Wacław Sierpiński and Stanisław Leśniewski were engaged in cryptography work (Nowik 2010; McFarland, McFarland, Smith 2014). Breaking the Soviet ciphers and getting access to secret information through radio intelligence contributed to Polish victory in the battle of Warsaw and thwarting the Soviet offensive. While Mazurkiewicz, Sierpiński and Leśniewski returned to their research in topology, set theory and logic, respectively, a new generation of codebreaking mathematicians grew in the Second Republic of Poland. The work of Marian Rejewski, Jerzy Różycki and Henryk Zygalski, young adepts of a cryptography course at the newly created University of Poznań,[9] was instrumental in breaking the code of the German cipher machine Enigma (Rejewski 1980a; Rejewski 1980b/1982; Christensen 2007).

2. Polish mathematicians in the Russian Empire

2.1. Warsaw

At the beginning of the 20th century two academic-level schools existed in the Polish Kingdom where mathematics was taught:[10] the Imperial University in Warsaw and the Polytechnic Institute in Warsaw. The language of instruction was Russian.[11] A few future Polish mathematicians graduated from the University, e.g. Kazimierz Żorawski (in 1888) and Wacław Sierpiński (in 1903). In the later years some Poles boycotted

[9] The university was established in 1919, under the name of Polish University in Poznań (Uniwersytet Polski w Poznaniu). Its roots go back to the Jesuit College confirmed in 1611 by the Polish king Sigismund III Vasa.

[10] There were 4 institutions of higher education in the Kingdom in 1914; see Bartnicka 2014.

[11] Mathematics in the Russian institutions in the Polish Kingdom represented quite a high level. The most prominent mathermaticians were Dmitri Dmitrievich Mordukhai-Boltovskoi (1876–1952), who worked at the Warsaw Polytechnic, and Georgy Feodosevich Voronoi (1868–1908), who worked at the University. Nikolai Yakovlevich Sonin (1849–1915) spent his entire career at the Imperial University, starting in 1871. Vsevolod Ivanovich Romanovskii (1879–1954), who worked in Warsaw in the years 1911–1915, followed as a professor to Rostov-on-Don, and in 1918 to Tashkent (Duda 2016; M. B. Nalbaldian, Yu. S. Nalbaldian 1995).

these institutions, but those who could not go to other provinces of the Empire or abroad still sought their education in the Kingdom. **Zygmunt Chwiałkowski (1884–1952)** graduated from the Imperial University in 1913 and stayed there to prepare for an academic career. He published a monograph on functional equations in Russian in 1914 (NN6 2014; M. B. Nalbaldian, Yu. S. Nalbaldian 1995).[12]

At the same time, the Society for Scientific Courses (Towarzystwo Kursów Naukowych) organized education in Polish at pre-academic and academic level in multiple disciplines, which was not officially recognized, just tolerated. The mathematician **Samuel Dickstein (1851––1939)**, a graduate of the Imperial University (who started his studies when instead of the university there was the Main School), was an active promoter of Polish education and scientific organizations. He published at his own expense two mathematical journals, the first such ones in Polish: *Wiadomości Matematyczne* since 1897 and *Prace Matematyczno--Fizyczne* since 1888. He co-founded the Warsaw Scientific Society (Warszawskie Towarzystwo Naukowe) and donated a library of mathematical books to be used by the Mathematical Study within the Society. In the years 1906–1916 he was active in the Mathematical and Physical Circle, which brought together over 100 teachers from the Polish Kingdom.

The strife of Poles for restoration of Polish higher education in the Russian partition and liberalization of education in general[13] culminated in the massive school strike in the years 1905–1908. At that time the authorities made only small concessions, but the break of the war in 1914 brought a mitigation of the Russian policies towards the Polish society. As early as August 14, 1914, the Grand Duke Nikolai Nikolaevich issued an address in which he pledged unification of "self-governing" Poland under the rule of tsars.[14] With the hopes for freedom rekindled, the newly established Warsaw Civic Committee (Komitet Obywatelski Miasta Warszawy) set up a proposal of restoring the University of Warsaw,[15] which would continue the traditions of

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[12] In the independent Poland Chwiałkowski taught mathematics at high schools and co-wrote a geometry textbook in 1935 with Wacław Schayer (1905–1959) and Alfred Tarski (1901–1983) (A. McFarland, J. McFarland, Smith 1995).

[13] One of the postulates was admission of women to academic education.

[14] The address did not have tsar's authorization.

[15] Dickstein contributed to this plan.

the Royal University (1816–1831) and the Main School (1862–1869). Meanwhile, as the Central Powers advanced in the spring and summer of 1915, the Imperial University was evacuated. The files, libraries and equipment went first to Moscow, where they were followed by the personnel. Then the University moved to Rostov-on-Don, where it remained ever since.[16] The Polytechnic Institute was also evacuated, to Nizhny Novgorod.[17]

On August 5, 1915, the German army entered Warsaw. The Polish Kingdom was divided into two occupational zones, German and Austro-Hungarian. On September 4, the Germans created the Warsaw general governorate with General Hans von Beseler (1850–1921) as the Governor-General. An idea emerged of creating a Polish state as one of buffer states in *Mitteleurope* under political, economical and military control of Germany.[18] Efforts were made to Polonize the administration and court system. **Władysław (Ladislaus von) Bortkiewicz (1868–1931)**, a professor of statistics at the Friedrich-Wilhelm University in Berlin since 1901 (born in St. Petersburg to a Polish family and educated there), was a "scientific statistical support worker" for the Civil Administration of the General Governorship of Warsaw (Zivilverwaltung des Generalgouvernements Warschau) from November 1916 to February 1917 (Sheynin 2011).

Opening Polish institutions of higher education was important. Such institutions would prepare future specialists and administrators for the new state in a way that would suit the controlling powers and keep young people out of trouble. Moreover, their existence would improve the attitude of Poles towards the German Empire, so the Central Powers could mobilize Polish men and use resources from the occupied territories. In these favorable circumstances, the Civic Committee's project was revisited. The Section for Higher Education (Sekcja Szkół Wyższych) was created, divided into two commissions: the University Commission and the Polytechnic Commission. The mathematician **Stefan Mazurkiewicz (1888–1945)**, a native of Warsaw, who studied

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[16] Even though officially renamed in 1917, it used the name "Warsaw University" until 1924.

[17] It later gave rise to the Nizhny Novgorod State Technical University.

[18] The idea was officially announced in a joint declaration by the two respective Governors-General, von Beseler and Karl Kuk (1853–1935) on November 5, 1916.

in Kraków, Göttingen and Lwów, was a member of the subcommission for mathematics and natural sciences. In the fall of 1915 a Polish university and a Polish polytechnic school were established. Count Bogdan Hutten-Czapski (1851–1937), a Polish aristocrat in German state service, was named a curator, whose function was to act as an official contact between the General-Government and the administrative structures of the new schools. Józef Brudziński (1874–1917), a physician, became the rector of the University. The rector of the Polytechnic School was **Zygmunt Straszewicz (1860–1927)**,[19] a graduate in mechanical engineering of Eidgenössische Technische Hochschule in Zürich and a former student of mathematics at the Imperial University,[20] who until 1916 lectured on differential and integral calculus in the Technical Section of the Society for Scientific Courses and until 1919 taught mathematics and mechanics at the private Mechanical-Technical School of Hipolit Wawelberg (1843–1901) and Stanisław Rotwand (1893–1916) (Bartnicka 2015; Kauffman 2015; Garlicki 1982; Duda 2016; Kutrzeba 1988; Chwalba 2014).

According to Kauffman (2015), in the 1915–1916 academic year, the university's teaching staff included thirty-six lecturers ("wykładający"), the highest rank afforded to teaching staff at that time, twenty-three assistants (asystenci), and six foreign-language instructors (lektorzy). There were 1,039 students enrolled in 1915–1916. The polytechnic school comprised four departments, where 25 teaching staff instructed about 600 students. **Kazimierz Kuratowski (1896–1980)**, a Warsaw native who had to interrupt his engineering studies in Glasgow because of the war, was one of the first students at the University. The introductory courses in mathematics he took in 1915 were the following: projective geometry taught by **Stefan Kwietniewski (1874–1940)** (in Kuratowski's reminiscence, his lecture was very modern and thorough), analytic geometry by **Juliusz Rudnicki (1881–1948)**, and algebra by Samuel Dickstein. Along with Kuratowski, the freshman class included, among others, **Bronisław Knaster (1893–1980)**,[21] who studied medicine in Paris before the war, but in Warsaw switched his interest first to

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[19] Stefan Straszewicz's uncle.

[20] Expelled in 1883 in connection with so-called *apuchtinada*.

[21] In 1914 Knaster married Maria Morska (1895?–1945), later a renowned actress, a columnist, and a muse to the Skamander poetic group (Koper 2011).

logic (he translated Louis Couturat's *L'Algèbre de la logique* into Polish), then to matematics; as well as **Stanisław Saks (1897–1942)**, a graduate of the private gymnasium of Michał Kreczmar in Warsaw[22] (Kuratowski 1973; Duda 1987; Zygmund 1982). The lectures in mathematics at the Polytechnic also included some advanced contents. **Szolem Mandelbrojt (1899–1983)**, who started his studies there in 1917, recalled Rudnicki presenting Weierstrass's example of a continuous nowhere differentiable function. However, Mandelbrojt found mathematics at the University more attractive, appreciating both lectures and the possibility of private interaction with the faculty, in particular with Zygmunt Janiszewski. He spent two years as a student in Warsaw, published a paper on number theory in 1919, and continued his education in Kharkov, Berlin and Paris (Mandelbrojt 1985).

The number of students at the University was rising through years, reaching about 4,500 in 1918. **Antoni Zygmund (1900–1992)**, who as a gymnasium student in 1914 was evacuated with his family to Poltava, returned to independent Poland in 1918, completed his education in Kazimierz Kulwieć's Gymnasium[23] and entered University of Warsaw in 1919. Zygmund became a student of **Aleksander Rajchman (1890–1940)**, a graduate of Sorbonne, who before the war was giving private lessons (to Szolem Mandelbrojt, among others), spent the year 1914/15 in Vienna on a scholarship from Władysław Kretkowski's[24] fund (Ciesielska 2016; Maligranda, Piotrowski 2017), and then worked at the University of Warsaw since 1919, starting at the rank of a junior assistant (Domoradzki, Pawlikowska-Brożek, Węglowska 2003).[25] Finally, women gained full access to higher education.

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[22] The Russians allowed private Polish schools, but their number was limited and they were subject to frequent inspections. After the school strike the situation eased. Kreczmar's school was known for its patriotic atmosphere.

[23] Kazimierz Kulwieć (1871–1943), a naturalist and explorer, organized a Polish gymnasium in Moscow in 1915, which he directed for 3 years. After returning to Warsaw he established a school for re-immigrants from Russia.

[24] Władysław Kretkowski (1840–1910), a mathematician and a benefactor of science, graduated from Sorbonne, got a PhD from Jagiellonian University and taught as a lecturer (*docent prywatny*) at Lwów Polytechnic and Lwów University.

[25] Since 1926 Rajchman was a *docent* at the University and an associate professor (*profesor nadzwyczajny*) at the Free Polish University (Wolna Wszechnica Polska). He worked on functions of a real variable and probability.

Stanisława Nikodym (née Liliental, 1897–1988)[26] enrolled at the University in 1916, but took a break in the year 1918–1919 to teach mathematics to army recruits. She returned to her studies and went on to receive her PhD in mathematics in 1925 as the first woman in Poland, to work as an assistant at the Warsaw Polytechnic and to publish several research papers in mathematics (Ciesielska 2017a; Ciesielska 2017b; NN4 1983).

Many of the faculty had previous connection to the Society for Scientific Courses. Kwietniewski, who got his PhD at the University of Zürich in 1902, concentrated his activities on popularizing mathematics, especially geometry, as well as translating and editing foreign textbooks and monographs. In the years 1907–1909 he taught at the Society for Scientific Courses and later contributed to the "Guide for the self-study". In the independent Poland he continued his university lectures in geometry on a basis of annual contracts. Rudnicki, a graduate of Sorbonne and later a PhD recipient from Jagiellonian University, taught in private Warsaw schools for men and women since 1912. At the same time he conducted lectures in mathematics at the Society for Scientific Courses (TKN) and at advanced pedagogical courses for women. After the Russians' retreat from Warsaw he was a member of the Polytechnic Commission of the TKN (the electrotechnical-mechanical sub-commission). After creation of the Warsaw Polytechnic he taught mathematics and (briefly) physics there. He was also active in the Society for Aid to the Victims of War (Towarzystwo Pomocy Ofiarom Wojny). Later he became a professor in Vilnius University (Królikowski 1989).

Among the faculty of the University there also were Stefan Mazurkiewicz and **Zygmunt Janiszewski (1888–1920)** (later the recipients of the first two chairs in mathematics). This was remarkable on two accounts. First, both of them were rather young; second, both were previously connected to Lwów (Janiszewski taught there as a senior lecturer (*docent*[27]) and Mazurkiewicz got his doctorate under Sierpiński in 1913) and Beseler (contrary to the wishes and efforts of the Civic Committee)

[26] Otto Nikodym's wife.

[27] In Janiszewski's folder there is also a nomination for the position of an assistant for the period 1917–1919. He had a license to lecture; lectures were planned.

did not want too many professors from Austrian universities coming to Warsaw.[28] However, they had significant mathematical achievements and scholarly output: Mazurkiewicz published 16 papers and Janiszewski 20 papers before 1916. Janiszewski was available only part-time, as he was still serving in the military. Since 1916 Janiszewski and Mazurkiewicz conducted a seminar in topology at the university, possibly the first one in the world in this new discipline. Kuratowski, Knaster and Saks participated as students (Kuratowski 1979).

Student organizations played a major role in the academic life. For example, the analysis of Stanisław Zaremba's *Theoretical Arithmetic* by **Jan Łukasiewicz (1878–1956)**[29] in his course on methodology of deductive sciences prompted a discussion involving professors and students. The discussion of related issues continued in the meetings of Mathematical and Physical Circle of the Warsaw University, with Kuratowski giving a two-part talk on December 6 and 13, 1917, "On the definition of a quantity", which soon became his first scholarly publication (see Kuratowski 1979). On a larger scale, events related to Polish history and Polish national heroes were commemorated. However, the arrest of two students after celebrating the anniversary of the 3rd of May Constitution, led to the students' strike in 1917 and temporary closing of the schools. The occupational authorities transferred the control of the schools to the Temporary Council of State, the first government of the Kingdom of Poland. Józef Piłsudski, who held the authority over the military matters, resigned from the Council, which led to the so-called Oath Crisis. The Council was ultimately disbanded in August 1917. In the fall of 1918 Polish army started to organize itself and students were joining in great numbers. The Academic Legion (Legia Akademicka) – a unit comprised entirely of students – was formed.

The creation of Polish academic institutions did not eliminate the need for the Society for Scientific Courses. There still were many people

[28] According to Kuratowski (1979), Mazurkiewicz supported the candidacy of the Polonized archduke Karol Stefan Habsburg-Lotaryński (1860–1933) for the throne of the Polish Kingdom under the auspices of Austro-Hungary and Germany.

[29] Łukasiewicz was a philosopher and a logician whose work was taking more and more mathematical character. He was a pioneer of multi-valued logic and an inventor of the Polish notation.

aspiring to higher education with insufficient credentials for admission, so the Society continued its activity during the war. The Department of Mathematics and Physics separated from the Department of Mathematics and Natural Sciences in 1915. In 1915/16 it run the following compulsory courses: descriptive and projective geometry, taught by **Wacław Gniazdowski (1864–1938)**;[30] analytic geometry, taught in the fall by **Romuald Witwiński (1890–1937)**[31] and in the spring by **Tadeusz Gutkowski (1881–1962)**;[32] introduction to analysis by **Władysław Wójtowicz (1874–1942)**.[33] The optional courses in the first semester were differential and integral calculus, taught by Juliusz Rudnicki, and vector calculus, by **Wacław Werner (1879–1948)**.[34] Lectures by Stanisław Leśniewski and Stefan Mazurkiewicz were also planned, but did not run. In the first and second semester there were respectively 27 and 19 students. In 1916/17, **Franciszek Włodarski (1889–1944)**,

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[30] Gniazdowski, a textile engineer, taught mathematics and technological subjects at the Technical School of the Warsaw-Vienna Railway (Szkoła Techniczna Drogi Żelaznej Warszawsko-Wiedeńskiej). After the school's evacuation he founded his own private 7-grade school of technology and transportation (Majewski 2007). He also taught mathematics at the Real 7-Grade School directed by Witold Wróblewski (1839–1927) with instruction in Polish in the years 1915–1918. Later he was a senior lecturer (*docent*) at the Warsaw Polytechnic, teaching principles of perspective at the Department of Architecture (NN1 1938).

[31] Witwiński authored several papers and problem books in geometry and at least 2 papers in number theory. Dates of birth and death after (Schinzel 1993).

[32] Tadeusz Gutkowski, an optical engineer, graduate of Institut d'Optique in Paris, taught mathematics in Warsaw high schools, and later worked in the optical industry (Gutkowski 2012).

[33] Władysław Wójtowicz – a teacher of mathematics, textbook author and translator of mathematical works. An editor of the journal *Wektor* for teachers, editor of a series published by the Mianowski Fund, author of high school geometry textbooks and logarithmic tables for the school use, later director of the Methodical Center for Mathematics (Piotrowski 2003).

[34] Wacław Werner studied electrotechnology in Darmstadt, mathematics and physics in Kraków, Göttingen and Fribourg. He received a doctorate from the Faculty of Mathematics and Natural Sciences in Fribourg. In 1909–1939 he taught physics in high school in Warsaw. In 1916/17 he was the dean of the Department of Mathematics and Physics in the Society for Scientific Courses. During that time he co-managed family-owned metal works. Since 1915 he worked at Warsaw Polytechnic, lecturing and conducting experiments; given the title of professor in 1948 (Werner 1998).

a geometer with doctorate from the University of Fribourg, started to lecture (Maligranda 2017; Błędowski *et al.* 1917–1919).

Mathematical subjects were also taught at the Department of Technology, among them trigonometry by **Tomasz Świętochowski**,[35] algebra with geometry by **Bruno Winawer (1883–1944)**[36] and analytic geometry by **Lucjan Zarzecki (1873–1925)**.[37] The recitation classes were taught by **F. Łazarski**[38] (differential and integral calculus), R. Świętochowski[39] (descriptive geometry), A. Winawer[40] (high school mathematics), W. Wójtowicz (higher mathematics as well as analytic geometry, together with the lecture). Later the Society also gave rise to the Free Polish University (Wolna Wszechnica Polska), a fully accredited private university operating in the years 1918–1952 in Warsaw and Łódź (Błędowski *et al.* 1919).

The publication of the series "Guide for the self-study" (Poradnik dla samouków)[41] continued during the war. A volume on mathematics, starting the second series, was published in 1915. It contained chapters written by Jan Łukasiewicz (On Science), Zygmunt Janiszewski (General Introduction; Introduction to Level III; Ordinary Differential Equations; Functional, Difference and Integral Equations; Series Expansions; Topology; Foundations of Geometry; Logistics; Philosophical Issues of Mathematics; Conclusion; Informational Section), Stefan Kwietniewski (Level I; Level II; Methodology of Teaching; Analytic

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35 Świętochowski taught mathematics at the Real 7-Grade School directed by Witold Wróblewski with instruction in Polish in the years 1915–1919 (NN3 1977). We were not able to find the dates of birth and death for him (see the footnote below about R. Świętochowski).

36 Bruno Winawer – a physicist, writer and popularizer of science, a graduate of University of Heidelberg.

37 Lucjan Zarzecki – a mathematician and educator, a graduate of St. Petersburg University in 1897.

38 We were not able to find the full first name and the dates of birth and death for him.

39 Born 1882 (Kiepurska 1981); probably a misprint of the initial.

40 Probably a misprint of the initial.

41 The series appeared in several cycles in the years 1898–1932, financed by the Mianowski Fund. It was meant as an educational aid at an academic level. Each volume presented the development and the state-of-the-art of a given scientific topic, along with exhaustive bibliography. The editors were Aleksander Heflich (1866–1936) and Stanisław Michalski (1865–1949).

Geometry; Synthetic and Descriptive Geometry; Differential Geometry; History of Mathematics: History of Mathematics in General; History of Mathematics in Poland), Wacław Sierpiński (Arithmetics; Number Theory; Higher Algebra; Set Theory; Theory of Functions of a Real Variable; Differential and Integral Calculus; Differential Calculus and Summation), Stanisław Zaremba (Theory of Analytic Functions; Differential Equations with Partial Derivatives; Theory of Groups of Transformations; Calculus of Variations) and Stefan Mazurkiewicz (Theory of Probability) (Pawlikowska-Brożek 1992b). Marian Smoluchowski contributed a chapter on physics to the 1917 volume.

2.2. St. Petersburg (1914–1924 Petrograd; 1924–1991 Leningrad)

Polish presence was very strong in the Russian capital. In 1910 the number of Poles living there reached its historical maximum of about 65,000 (3.4% of the total population of the city). Polish nationals could be found among officers, civil servants, artists and scholars (Garczyk 2016).

Julian Karol Sochocki (Yulian Vasilievich Sokhotsky, 1842–1927), born in Warsaw, was educated at the University of Saint Petersburg and was a professor of mathematics there. His results in the field of one complex variable (Sochocki-Casorati-Weierstrass theorem, Sochocki-Plemelj formula) became classic. **Jan Ptaszycki (1844–1912)** was a professor of mathematics at the University of Saint Petersburg and at the Mikhailovskaya Military Artillery Academy. His work dealt with elliptic functions and algebraic differentials. **Wiktor Emeryk Jan Staniewicz (1866–1932)**, born in Samara, educated in St. Petersburg, held the chair of mathematics at St. Petersburg Polytechnic Institute since 1902. He worked in number theory and mathematical analysis. In 1909 his state service was suspended for three years, because illegal political activities were discovered to go on in dormitories that he supervised. In that period he lectured as a contract professor. In the years 1915–1917 he was the dean of the Faculty of Civil Engineering, in 1917–1918 a vice-rector. Polish mathematicians in St. Petersburg did not form a separate learned society, but were active in the Polish Union of Physicians and Naturalists (Związek Polski Lekarzy i Przyrodników). Sochocki also presided over St. Petersburg Mathematical Society in the years 1884–1927 (Domoradzki, Pawlikowska-Brożek 1999).

The year 1917 brought dramatic political and social changes to the Russian Empire. The (changing) authorities were trying to win the support of Poles. In December 1916 Tsar Nicholas II as the commander in chief issued an order number 870 to land and maritime armed forces, which among the goals of further campaign mentioned the "creation of free Poland". In March 1917 the Provisional Government stated that it counted on forming a "free military union" with Poland in the future, while the Petrograd Soviet of Workers' and Soldiers' Deputies (later taken over by the Bolsheviks) announced Poland's right to complete political independence, and the general right of nations to "self-determination" (Zasorin 2017). In the circumstances favorable to the Polish cause, in July 1917 Staniewicz became a president of the Polish Radical-Democratic Union in Lithuania and Belarus (Polski Związek Radykalno-Demokratyczny na Litwie i Białorusi) and took part in the attempts to form the Polish National Executive Commission (Polska Narodowa Komisja Wykonawcza) in Russia. In October 1919 he moved to independent Poland and became a professor of mathematics at Stefan Batory University in Wilno (Vilnius), serving as its rector in the years 1921–1922. He was the first president of the Polish Mathematical Society (actually, the Society first elected Kazimierz Żorawski, who was absent from the meeting and could not take office [Jackiewicz 2002; Iwiński 1975]).

2.3. Moscow

According to the census from 1897, there were 9236 Poles living in Moscow, 0.89% of its population. The massive evacuations from the Polish Kingdom at the beginning of war raised this number.

Bolesław Młodziejowski (Boleslav Kornel'jevich Mlodzeevskii, 1858–1923) was born in Moscow in a physician's family.[42] He graduated from Moscow University in 1880 and became a professor of mathematics there in 1892. His research interest was in geometry. In 1902 he served as an opponent in the doctoral defense of Antoni Przeborski.[43] In 1911 he resigned from his position in protest against decisions of

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[42] Młodziejowski's maternal grandfather was a Czech musician Vincenz (Vikientii) Lemoch (1792–1862?), a brother of Wojciech Ignacy Lemoch (1802–1875), who was a professor of geometry and a rector of the Lwów University (NN2 1970; Pawlikowska-Brożek 1972).

[43] Other faculty refused, citing anti-Polish regulations from 1864 (Odinets 2014).

the enlightment minister Lev Aristidovich Kasso (1865–1914).[44] He continued his teaching activities at the Higher Courses for Women as well as at Moscow City People's University.[45] At the latter, he conducted lectures in differential geometry and introduced modern-style seminars. In January 1914, he chaired the organizing committee of the Second All-Russian congress of lecturers in mathematics, in which 20 speakers from Polish territories took part. After the February revolution in 1917 Młodziejowski returned to the university. In 1921 he became the first director of the newly created Research Institute of Mathematics and Mechanics at the Moscow University (Zverkina, Pugina 2009; Odinets 2014).

Stanisław Leśniewski (1886–1939), born in Serpukhov in the Moscow governorate and brought up in Irkutsk, studied philosophy and mathematics in Germany, Switzerland and Russia. He completed his doctorate in philosophy in 1912 at the Lwów University under the direction of Kazimierz Twardowski. Afterwards he taught at a school in Warsaw. When the war broke out, he went to Moscow.[46] He taught matematics at a Polish gymnasium and at the Real School of the Polish Committee of Aid to the War Victims (Szkoła Realna Polskiego Komitetu Pomocy Ofiarom Wojny), founded for boys from families that were evacuated from the Polish Kingdom.[47] Leśniewski was also active in the Polish Scientific Circle (Polskie Koło Naukowe). Through the Circle, he published his book *Foundations of General Set Theory, Part I* in 1916. The second part was planned for 1917, but never came out.

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[44] Kasso proposed new ways of staffing vacant chairs at Russian academic institutions, which met with disagreement of the professors.

[45] The People's University was also referred to as Shanyavskii's University, after its founder Alfons Lvovich Shanyavskii (Alfons Fortunat Szaniawski; 1837–1905), a general of Polish origin in the Imperial army. It was a research-oriented university, open to anyone regardless of their origin, education, gender, age, nationality, or religious beliefs. It operated in the years 1908–1918 (Ragulsky 2011).

[46] It is not clear why Leśniewski went there. It could be as a result of an evacuation or in connection with his political activities as a member of Social Democracy of the Kingdom of Poland and Lithuania (Socjaldemokracja Królestwa Polskiego i Litwy).

[47] The school was directed by the distinguished educator Władysław Giżycki (1875–1947). Among the students there was the future poet Konstanty Ildefons Gałczyński (1905–1953) (Gałczyńska 2003).

Despite the title, the book treated Leśniewski's own theory of parts, wholes and concrete collections, which was later developed into his system of Mereology. In 1918 he returned to Warsaw and on December 14 he submitted his habilitation dissertation in logic and philosophy of mathematics to be evaluated by Wacław Sierpiński.[48] In 1919 Leśniewski became a professor of philosophy of mathematics at the University of Warsaw. In 1920, along with Stefan Mazurkiewicz and Wacław Sierpiński, he contributed to breaking Soviet codes in the Polish-Soviet war (Betti 2009; A. McFarland, J. McFarland, Smith 2014; Simons 2015).

Wacław Sierpiński (1882–1969) – a native of Warsaw, a graduate of the Imperial University (under the direction of Georgy Voronoi) and a PhD recipient from the Jagiellonian University, was an associate professor (*profesor nadzwyczajny*) of mathematics and the interim leader of the Chair at the Lwów University since 1910. The outbreak of the war found him in Poznajów near Witebsk (Belarus), in the estate of his parents-in-law. As an Austro-Hungarian citizen, and hence an enemy alien, he was interned in the city of Vyatka (nowadays Kirov). Thanks to the efforts of Moscow mathematicians (mainly Dmitri Fyodorovich Egorov, 1869–1931) he was allowed to relocate to Moscow in 1915. The Rectorate of the Lwów University was notified of Sierpiński's internment in Moscow through the American consulate[49] in Vienna in February 1916. The university administration made efforts to transfer to Sierpiński his overdue (since 1914) salary using the same diplomatic channels, but they were unsuccessful.

The Moscow period was very fruitful for Sierpiński. It marked a beginning of his deep studies of the axiom of choice and its role in mathematics. He gave a talk on the subject at the meeting of the Moscow Mathematical Society on February 21, 1917.[50] He also started

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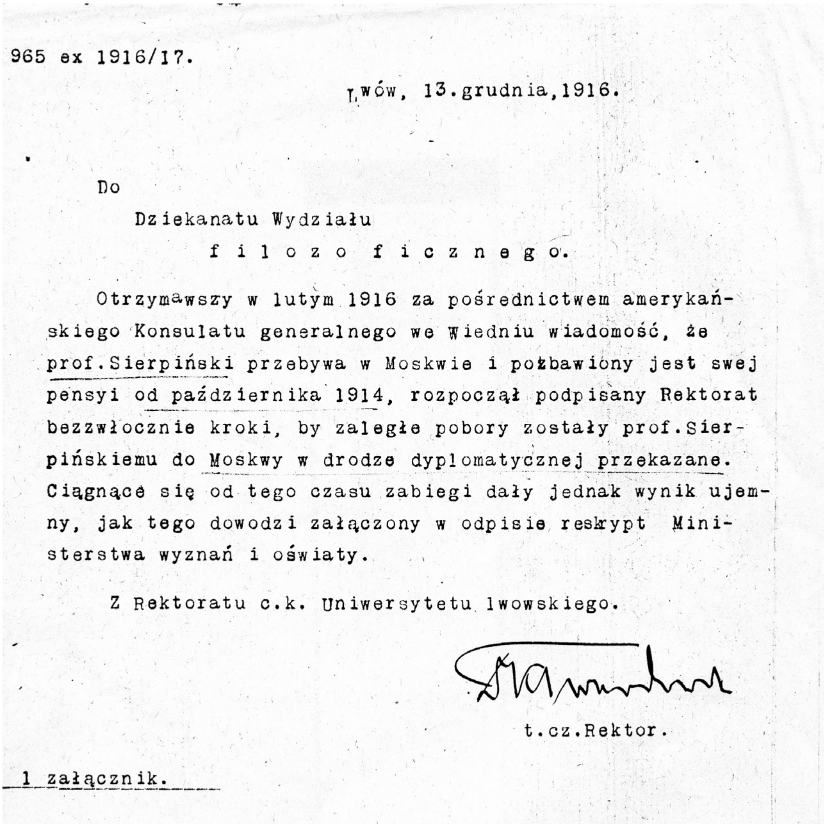
[48] The dissertation consisted of the works "Problems of the General Theory of Sets, I" and "A Criticism of the Logical Principle of the Excluded Middle".

[49] The United States of America remained neutral in the war until April 6, 1917.

[50] It was preceded by a note in *Comptes Rendus* of the French Academy in 1916. The expanded version of the talk was later published in French as "L'axiome de M. Zermelo et son rôle dans la théorie des ensembles et l'analyse" in *Bulletin International de l'Académie des Sciences de Cracovie. Classe des Sciences Mathématiques et Naturelles, Série A* 1918, pp. 97–152 and in Russian as "Aksioma Zermelo i eio rol' v teorii mnozhestv i analize" in *Matematicheskii Sbornik* 1922, vol. 31(1), pp. 94–128. See also Lewandowska 2013.



965 ex 1916/17.

Lwów, 13.grudnia,1916.

Do

Dziekanatu Wydziału
f i l o z o f i c z n e g o.

Otrzymawszy w lutym 1916 za pośrednictwem amerykańskiego Konsulatu generalnego we Wiedniu wiadomość, że prof.Sierpiński przebywa w Moskwie i pozbawiony jest swej pensyi od października 1914, rozpoczął podpisany Rektorat bezzwłocznie kroki, by zaległe pobory zostały prof.Sierpińskiemu do Moskwy w drodze dyplomatycznej przekazane. Ciągnące się od tego czasu zabiegi dały jednak wynik ujemny, jak tego dowodzi załączony w odpisie reskrypt Ministerstwa wyznań i oświaty.

Z Rektoratu c.k. Uniwersytetu lwowskiego.

t.cz.Rektor.

1 załącznik.

Fig. 1. An official note about a failed attempt of the Lwów University to pay Sierpiński his overdue salary through diplomatic channels (Lviv District Archive, personal file 26.5.1723; copy made for the first named author for non-commercial use)

a collaboration and friendship with Nikolai Nikolaevich Lusin (1883–1950).[51] In the years 1915–1918 he published 41 papers, 4 of them jointly with Lusin and 3 on problems related to Lusin's research (Sierpiński 1974). While interned, Sierpiński was active in the Polish Scientific Circle (Polskie Koło Naukowe) established in November 1915 in Moscow. Through the Circle he published the first volume of *Mathematical Analysis*, which he dedicated to the Polish University

[51] The scientific relations later also extended to Lusin's students, who visited Poland and published in Polish journals.

in Warsaw.[52] He also gave talks in the Moscow Mathematical Society. In February 1918 he returned to Poland through Finland and Sweden. He resumed his lectures in Lwów in the summer semester 1918, but in the fall he moved to Warsaw. He was nominated for a full professor (*profesor zwyczajny*) of mathematics at the philosophical faculty of the Warsaw University by the decree of the Chief of State from March 28, 1919.[53] He announced his resignation from the chair in Lwów in a letter dated May 19, 1919, thanking his colleagues for the kindness they offered him during his stay in Lwów (Mioduszewski 1998; Sinkiewicz 1995; LDA-Sierpiński).

Kazimierz Jantzen (1885–1940) got a doctorate in astronomy in Munich in 1912. In the years 1912–1914 he was at the astronomical observatory in Potsdam. The outbreak of the war found him in Warsaw. As a German citizen, he was interned by the Russian authorities in Vyatka, and then transferred to Moscow. He taught in Polish high schools and was active in the Polish Scientific Circle. He published a book *On the influence of the spectral type of stars on determining the apex of the Sun (O wpływie typu widmowego gwiazd na wyznaczanie apeksu słońca)*. He returned to Poland in 1918, worked at the astronomical observatory in Warsaw, Warsaw Polytechnic (lecturing on advanced surveying and the error theory) and the Military Geographical Institute. Then he took a chair of astronomy at the University of Wilno (Vilnius), where he also lectured on analytic geometry, statistics and mathematics for naturalists (Domoradzki 2017; Rybka 1964). In 1932 he published a paper on a problem in number theory (Schinzel 1993).

2.4. Kharkov

The University of Kharkov was established in 1805 by a Polish aristocrat, Seweryn Potocki (1762–1829). In 1897 there were 3969 Poles living in Kharkov, 2.28% of its population. The distance from the front lines of the World War I allowed for the university activities to go on as usual, at least at the beginning of the war. **Jerzy (Yuri Cheslavovich)**

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[52] Like in the case of Leśniewski, the second volume was planned, but never appeared.

[53] The ordinary chair of mathematics was offered to Sierpiński by the Ministry of Religious Denominations and Public Education on February 27, 1919.

Neyman (1894–1981), born in Bendery, in the Bessarabia governorate, entered his second year of studies at the University of Kharkov in 1914/15. Rejected in the draft because of poor eyesight, he was preparing a paper on Lebesgue integral to enter a university-wide competition at the encouragement of his professor **Cezary (Cezar Karlovich) Russjan (1867–1934)**.[54] His 580-page work won and he received a monetary equivalent of the gold medal (the actual medal could not be awarded because of wartime restrictions on metals). In 1917 Neyman finished his course of studies and, on Russjan's recommendation, was granted a government stipend to prepare himself for an academic career. At the same time he started working in the Kharkov Institute of Technology as an assistant to **Antoni Bonifacy Przeborski (A. P. Psheborskii, 1871–1941)**,[55] in analytic geometry and introduction to analysis, and as a lecturer in elementary mathematics.

The wartime situation in the Russian empire was complicated by the outbreak of 2 revolutions: in February and October 1917, and by the Ukrainian-Soviet war. Ukrainian People's Republic of Soviets formed in 1917 in Kharkov fought Ukrainian People's (or National) Republic proclaimed in January 1918 and based in Kiev, which was aided by the Germans after the Brest-Litovsk peace treaty between Germany and Russia was signed on February 9, 1918. The fightings continued after the Germans withdrew. The University and Polytechnic Institute in Kharkov continued to operate under the Bolshevik rule (with some interruptions), opening their doors to many more people from underprivileged background. Neyman was assigned a task of teaching remedial classes in mathematics to these new students. In addition, he taught mathematics and physics in a newly opened Polish high school in Kharkov. He also spent a brief time in prison, arrested for bartering

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[54] Russjan got his doctorate in Odessa in 1900. For some time he held lectures at the Lwów Polytechnic and the Jagiellonian University. In 1907 he took the chair of mathematical analysis at the University of Kharkov. His main interests were differential equations and probability.

[55] Przeborski got his doctorate in 1902 at Moscow University. In 1908 he became an ordinary professor at the University of Kharkov. He also taught at Kharkov Polytechnic Institute, Women's Higher Courses and the Workers' University. His main interests were analytic functions, dfferential equations and variational calculus. In the independent Poland he was a professor of universities in Warsaw and Vilnius; he also taught mathematics and mechanics at the Warsaw Polytechnic.

matches for food in the black market. It was during the wartime that Neyman got interested in statistics (which later became the field of his highest achievements), through discussions he had with Sergei Natanovich Bernstein (1880–1968), newly promoted to professorship at the University of Kharkov.

In the years 1919–1920 Przeborski was the rector of the university, reorganized into the Academy of Theoretical Sciences. Neyman recalled that in the time of severe deprivations Przeborski arranged for the professors to obtain permission to chop trees in the nearby park for fuel. Due to a misunderstanding about legitimacy of the permission several professors were arrested, including Przeborski himself. He was released, later made the dean, and then the rector again. Eventually, Neyman and Przeborski left for Poland after the Polish-Soviet war (in 1921 and 1922, respectively). Russjan remained in Kharkov until his death in 1934[56] (Reid 1998; Kijas 2011).

2.5. Kiev

The Imperial St. Vladimir Kiev University, established in 1843, could be viewed as contination of the Krzemieniec Lyceum, since it started with the Lyceum's assets and its Polish faculty. It was a popular destination for Polish students; the poets Bolesław Leśmian (1877–1937) and Jarosław Iwaszkiewicz (1894–1980) studied there. Overall, there were 16,579 Poles in Kiev in 1897, 6.69% of its population. In 1919 the number went up to 36,800, or 6.77%. **Eustachy Żyliński (1889–1954)**, born to a Polish family in the Podolia district, graduated from the St. Vladimir University as a student of Dmitrii Alexandrovich Grave (1863–1939). He passed his exams for master's degree in 1914 in Kiev after taking a study trip to Göttingen, Cambridge and Marburg, then completing his exams and presenting a thesis "On the field of p-adic numbers". From 1912 to 1915 he worked at the St. Vladimir University. On April 16, 1916 he was drafted into the Russian army as a *Praporshchik* (ensign). As part of his officer's training, he completed several courses in engineering subjects and in radiotelegraphy in Kiev and Petersburg.

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[56] Some publications give his year of death as 1935. According to Savchuk and Kushlakova (2008) Russjan worked at the University of Kharkov until 1934 and remained a member of the Kharkov Mathematical Society until 1935.

On February 7, 1917, he was nominated to the rank of *Podporuchik* (Second Lieutenant). He became the Head of Radiotelegraphy of the South-Western Front, then he commanded an officers' class. He did not engage in combat; his main task was to teach electrotechnical subjects to the Staff of 103rd Front in Kamenets-Podolskii (Kamieniec Podolski; Kamianets-Podilskiy) and Berdichev.

On July 24, 1917, Polish I Corps was formed in Belarus from Polish soldiers serving on Northern and Western Fronts, under the command of General Józef Dowbór-Muśnicki (1867–1937). Żyliński reported to the commander of the Corps in November 1917. In the period from December 1917 to February 1919 he worked in the Polish University College, Ukrainian State University and Higher Private Polytechnic Institute in Kiev. He taught classes in analytic geometry, set theory, higher algebra and introduction to analysis. He got his habilitation at the Polish University College. He published one paper, "O zasadach logiki i matematyki" ("On the principles of logic and mathematics") in the *Reports of Polish Scientific Society in Kiev (Sprawozdania Polskiego Towarzystwa Naukowego w Kijowie)* in 1918. He also wrote 2 extensive works in the fields of algebra and logic during the period of war (both probably remained unpublished). On February 19, 1919, Żyliński went to Warsaw as an officer in the Polish army. He taught in the Officers' School of Communication. He remained in the military service until September 1919, still as a second lieutenant (*podporucznik*) in a radio-telegraph battalion. He was released to become an associate professor (*profesor nadzwyczajny*) of mathematics at Jan Kazimierz University in Lwów.[57] Earlier, he rejected an offer to take a chair of mathematics at the Kamianets-Podilsky State Ukrainian University (formed in 1918 under a law signed by Pavlo Skoropadskyi (1873–1945), Hetman of Ukraine). In Lwów he soon became the head of the Chair A. He initiated a revival of algebra in Lwów (Maligranda 2009; Domoradzki, Stawiska, Zarichnyi 2016).

Kazimierz Abramowicz (1888–1936), born in the Polish Kingdom, finished his course of studies in mathematics at St. Vladimir's University

[57] Żyliński's candidacy was supported by a mathematical committee, consisting of an astronomer Marcin Ernst (1869–1930), a physical chemist Roman Negrusz (1874–1926), a philosopher Kazimierz Twardowski (1866–1938) and a physicist Ignacy Zakrzewski (1860–1932).

in Kiev in 1911, receiving a gold medal for his work "On hypergeometric functions with one removable singular point". He worked under direction of Boris Yakovlevich Bukreev (1859–1962). In 1914 he passed his master's degree exams and went to Berlin and Göttingen for further studies. Because of the outbreak of the war, he returned to Kiev. In the academic years 1914/15 and 1915/16 he lectured at the Kiev Polytechnic Institute. He was delegated as a senior lecturer (*docent*) to the Perm branch of the Petrograd University for the year 1916/17.[58] As the branch became an independent university in 1917, Abramowicz was nominated a full professor (*profesor nadzwyczajny*) in the chair of mathematics. Because of the war operations he could not return to Perm in the fall of 1918, so he taught recitation classes in mathematics at the Polytechnic Institute in Kiev. He returned to Poland in June 1920 and started working at the newly established University of Poznań in 1921. His research concerned mainly automorphic functions (Maligranda 2016, Schinzel 1993).

Izabela Abramowicz (1889–1973)[59] was the first woman to receive the 1st degree diploma at the Faculty of Mathematics and Physics of the St. Vladimir University in Kiev and a gold medal for the thesis "On double integrals on algebraic surfaces", in 1911. Like her brother, she worked under direction of Boris Bukreev. She stayed at the university, by permission of the education minister, but without a stipend, to prepare herself for exams towards her master's degree. She also taught at three gymnasia in Kiev. In the years 1917–1920 she lectured on introduction to mathematics (as a senior lecturer [*docent*]) at the Polish University College in Kiev.[60] She joined the College when it expanded its course offer

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[58] The branch was established in 1916 as a result of evacuation of the Petrograd University deep into the territory of the Empire, to safeguard people and to alleviate provisional shortages. Mathematicians Yakov Davidovich Tamarkin (1888–1945), Alexander Alexandrovich Friedmann (1888–1925), Abram Samoilovich Besicovitch (1891–1970), Nikolai Maximovich Gjunter (1871–1941), Rodion Osievich Kuzmin (1891–1949) and Ivan Matveevich Vinogradov (1891–1983) taught there in the early years (Demidov 2015).

[59] Kazimierz Abramowicz's sister.

[60] The College was established at the initiative of Wacława Peretiatkowiczowa (1855–1939), a headmistress of two women gymnasia in Kiev. It started in 1916 as Higher Polish Learning Courses, initially allowed to offer only a program in humanities. It continued in 1917–1919 as the Polish University College. The faculty was recruited

to mathematics and sciences. She was one of two women among the faculty members; the other one was Antonina Dylewska (1883–1951), a mineralogist. In addition to her teaching activities, Abramowicz was also a member of the short-lived Polish Scientific Society in Kiev. The College faculty and students started to leave as the fightings continued. Even after the Great War had ended, Kiev changed hands, passing from Germans to Ukrainians to Bolsheviks to White Russians to Ukrainians and Poles to Bolsheviks again. Abramowicz was supposed to leave Kiev in 1920 with the retreating Polish army (her name was on the list of those approved for departure). Only in August 1923 did she arrive in Poznań; no record is known of her whereabouts and activities in 1920–1923. During the Second Republic and after World War II she taught mathematics in high schools in Poznań (Maligranda 2016).

2.6. Yuryev (Dorpat; Tartu)

The University of Dorpat continued the traditions of a Jesuit college established by the Polish king Stefan Batory in 1583 and attracted many Polish students.[61] Among distinguished graduates there were Tytus Chałubiński (1820–1889), a physician and promoter of tourism in Tatra mountains, and Wincenty Lutosławski (1863–1954), a philosopher. Jan Niecisław Ignacy Baudouin de Courtenay (1845–1929), a renowned linguist, was a professor there in the years 1883–1893. **Tadeusz Banachiewicz (1882–1954)**, a native of Warsaw,[62] took a position of

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from Poles teaching at Russian institutions of higher education as well as academics from the Polish Kingdom and Galicia who for various reasons found themselves in the Russian Empire. About 40 people overall taught there. Some of them, including Eustachy Żyliński, obtained their habilitation at the College. The students – mainly Polish nationals from the Kiev Governorate, along with some incomers from the Polish Kingdom or Galicia – were interested in getting higher education and preparing themselves for professional specialization or teaching in Polish schools in the Western Ukrainian territories. The majority of them – 572 out of 718 in the first semester – were women. The College's activities were financed mainly by the Society for Supporting Polish Culture and Learning in Ruthenia (Towarzystwo Popierania Polskiej Kultury i Nauki na Rusi) (Róziewicz, Zasztowt 1991).

[61] Until 1893 the language of instruction was German. Then the university was fully Russified. The city itself was renamed Yuryev.

[62] Banachiewicz studied at Warsaw, Kazan, Moscow and Göttingen. He was involved in the activities of the Society for Scientific Courses. He was primarily an

a junior assistant in September 1915 at the Astronomical Observatory there, moving from the University of Kazan. He also submitted his thesis "Three essays on refraction theory", for which he got habilitation and became a lecturer (private *docent*) at the Yuryev University. It was difficult for him to carry out his observations as planned, because some instruments were evacuated to places further inside the Russian Empire. However, his theoretical work (on orbit determination, involving high-precision solutions to Gauss' equation) was going well and brought him the master's degree in 1917, which in turn led to nomination for a senior lecturer (*docent*), winning the competition for a professor position in 1918 and the appointment as the director of the observatory. The university was being transferred to Voronezh and Banachiewicz got a nomination for a professor's position there, which he did not accept. He was allowed by the German occupying authorities[63] to go to Warsaw in September 1918. The end of war and proclamation of independent Poland found him there. From October 1918 to March 1919 he was a deputy professor of geodesy at the Warsaw Polytechnic. In 1919 he took a chair of astronomy at the Jagiellonian University, which he was offered in May 1918 (Flin, Panko 2011; Bujakiewicz-Korońska, Koroński 2016).

3. Poles in other countries

Leon Lichtenstein (1878–1933) held PhD degrees in engineering and mathematics (from Technische Hochschule Berlin-Charlottenburg and Friedrich-Wilhelm University, respectively) and was active both as a mathematician and an engineer in Berlin. From 1910 (*veniam legendi*) to 1919 he taught at the Technische Hochschule Berlin-Charlottenburg, lecturing on synthetic and descriptive geometry, graphic static, vector calculus, trigonometric series, integral equations, potential theory and other subjects. At the same time (from 1902 to 1920) he worked for Siemens & Halske (later renamed Siemens-Schuckert Werke), becoming

astronomer, but made a lasting contribution to mathematics by inventing the (non-associative) algebra of Cracovians. He is also credited with a proof of Schur's determinant formula.

[63] The independent Republic of Estonia was declared on February 24, 1918. The Germans withdrew from the territory and handed over control to the Estonian Provisional Government in November 1918.

a head of the electric laboratory in the factory of electric cables in 1906 and a mathematical expert in 1918.[64] The electrical industry was important for German economic growth.[65] The Siemens company was also active in the arms industry[66] and contributed to the war effort of the German Empire (it developed, among other things, a type of a rotary aircraft engine). The usefulness of his work was probably the reason why Lichtenstein, who was born in Warsaw and completed one-year "voluntary" service in the Russian army (in 1897), was able to obtain German citizenship in the first days of the war (Przeworska-Rolewicz 2005).

Chaim (Herman) Müntz (1884–1956), born in Łódź, obtained his doctorate in mathematics at the Friedrich-Wilhelm University in Berlin in 1910. He was unsuccessful in getting habilitation and academic position in Germany, so after a period of supporting himself with private lessons he became a teacher of mathematics in a boarding school called the Odenwaldschule near Heppenheim in southern Hessen.[67] He was given ample time to work on his mathematical research. In 1915 he left and took a position at another boarding school, also in Hessen (having only Hessian residency but no German citizenship he could not move freely), from which he was dismissed in 1917 as a "little Polish Jew". Müntz was able to write and publish 5 mathematical research papers while teaching. Also in 1915, he met and befriended the philosopher Martin Buber (1878–1965). He contributed (under the pseudonym of Herman Glenn) to a journal *Der Jude*, founded and co-edited by Buber (Ortiz, Pinkus 2005).

Mieczysław Biernacki (1891–1959): In the years 1909–1911 he studied chemistry at the Jagiellonian University. He was expelled for taking part in students' protests. Then he continued his studies at Sorbonne,

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[64] Some of Lichtenstein's engineering papers are mentioned here: High tension cable manufacture, present state and future. *London Electrician*, June 2, 1911; Testing high-tension cables. *Elek. Zeit.*, October 8, 1914.

[65] According to Eksteins (1989), by 1913 the value of German electrical production was twice that of Britain and almost ten times that of France, while Germany's exports in this area were the largest in the world, almost three times those of the United States.

[66] This activity resulted in January 1914 in the so-called Siemens Scandal involving bribes for supplying the Japanese navy.

[67] It was a renowned modern co-educational school, founded and run by the innovative educator Paul Geheeb (1870–1961).

switching to mathematics. When the war broke out, he voluntarily enlisted in the French army. He fought at the Western front, suffering gas poisoning and a severe wound. For his service he received the distinction of the Officer's Cross of the Legion of Honour. On June 4, 1917, the president of France issued a decree about forming an independent Polish army in France. Biernacki transferred to the Polish units. He returned to Poland with the Polish army under the command of General Józef Haller (1873–1960), also known as the "Blue Army". In 1928 Biernacki obtained a doctorate in Paris under the direction of Paul Montel (Montel 1962; Radziwiłłowicz 1997; Domoradzki 2013).

Juliusz Paweł Schauder (1899–1943): He graduated from Gymnasium VIII in Lwów in 1917, was drafted into the army and sent to the Italian front.[68] He was taken prisoner. While in the camp, he learned about a Polish army being formed in France under the command of Gen. Haller. On January 24, 1919, he reported to the local recruitment office and was enlisted into a company of ensigns in the rank of corporal. He returned to Poland with Haller's army and wore his blue uniform long after being discharged, because of material hardship (Derkowska 1990).

Stefan Straszewicz (1889–1983): He taught at the Society for Scientific Courses (fundamental notions of set theory, among other things). In summer 1913 he went to Zürich, thanks to the scholarship from the Mianowski Fund.[69] He got his PhD at the University of Zürich in 1914 under the direction of Ernst Zermelo (1871–1953) on the basis of the thesis "Beiträge zur Theorie der konvexen Punktmengen" (Research on the theory of convex sets). He continued his research in geometry and topology and translated into Polish the book *Stetigkeit und irrationale Zahlen* (Continuity and the irrational numbers) by Richard Dedekind (1831–1916). He belonged to the Union of Societies of Polish Youth for Independence (Unia Stowarzyszeń Polskiej Młodzieży Niepodległościowej), commonly called Filarecja. He returned to Poland in 1919. He fought in the Polish-Soviet war, then taught in Warsaw, first at the University and then at the Polytechnic (Piłatowicz 2006–2007).

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[68] The most important operations on the Italian front took place in the Isonzo valley. Ultimately the Italians prevailed and the armistice with Austro-Hungary was signed on November 3, 1918.

[69] According to Piłatowicz (2006–2007), Stefan Straszewicz's studies were financed by Zygmunt Straszewicz, his uncle.

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Henryk Gustaw Lauer (1890–1937): Born in Warsaw, he took part in the school strike in 1905. In the years 1908–1910 he studied mathematics at the Polytechnic (ETH) in Zürich, in 1910–1912 at the Sorbonne in Paris. In 1914 he returned to Zürich, where in 1918 he obtained a doctorate in mathematics at ETH under the direction of Adolf Hurwitz and Herman Weyl on the basis of the thesis "Sur la réduction des formes positives d'Hermite". He authored a few papers in mathematics. He was active in the Socialist Union of Student Youth and in the Social Democratic Party of Switzerland. In 1919 he returned to Poland, joined the Communist Workers Party of Poland (Komunistyczna Partia Robotnicza Polski) and devoted himself entirely to the party activity.

4. Conclusion

Few individual losses, students developing interests in mathematics as well as the arrival of a few promising or already established mathematicians (Kazimierz Abramowicz, Jerzy Neyman, Antoni Przeborski, Wiktor Staniewicz, Eustachy Żyliński) after the fall of the Tsarist Russia and the Bolshevik upheaval meant that Poland did not experience a generational gap in mathematics, unlike France. The so-called Bourbaki thesis (see Aubin, Goldstein 2014) claims that the occurrence of such a gap hindered modern development of mathematics. Another statement discussed in (Aubin, Goldstein 2014), Forman's thesis, claims that the war caused a collapse of traditional ways of thinking and hence accelerated progress in physics and mathematics, especially in Germany. We have not argued here whether or how either of these theses could be applied to Polish mathematics and mathematicians. But we should point out an audacious proposal made during the war, which was crucial for the direction that Polish mathematics took afterwards. We mean here the publication of the article "On needs of Polish mathematics" ("O potrzebach matematyki polskiej") by Zygmunt Janiszewski in 1917, answering an appeal of a new journal *Polish Science. Its Needs, Organization and Development* (*Nauka Polska. Jej Potrzeby, Organizacja i Rozwój*), published by Mianowski Fund. In that article Janiszewski announced his famous program of advancing Polish mathematics and it was rather radical of him to propose that it should be done by concentrating research on one discipline, possibly in one academic center, and establishing a specialized scientific journal devoted to this discipline. Yet to a large extent the program was carried out by the Polish School of Mathematics, continuing years after Janiszewski's premature death. Moreover, Janiszewski already practiced his program when the Polish university opened in Warsaw in place of the Russian one. In conducting lectures and seminars and developing research he was aided by like-minded colleagues such as Stefan Mazurkiewicz and Wacław Sierpiński. Topics in which he was interested managed to attract attention of several younger mathematicians. Janiszewski understood the importance not only of individual ideas, but also of research collaboration and institutional support. It took many intellectual and organizational efforts of Polish mathematicians before the Polish School of Mathematics emerged. The end of the Great War brought in the independent Polish state, in which the School thrived.

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In memory
of Polish botanists
who explored
Lithuanian flora

Botany at Stefan Batory University in Vilna (Wilno, Vilnius) (1919–1939)

Abstract

The university in Vilna (in Polish: Wilno, now: Vilnius, Lithuania), founded in 1579, by Stefan Batory (Stephen Báthory), King of Poland and Grand Duke of Lithuania, was a centre of Polish botany in 1780–1832 and 1919–1939.

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In the latter period the university functioned under the Polish name Uniwersytet Stefana Batorego (in English: Stefan Batory University). It comprised six departments connected with botany (General Botany, Pharmacognosy and Cultivation of Medicinal Plants, Plant Taxonomy, Botanical Garden, Garden of Medicinal Plants, and Natural History Museum).

There worked such distinguished scientists, as: Jakub Mowszowicz (1901–1983), phytogeographer and phytosociologist; Jan Muszyński (1884–1957), botanist and pharmacist; Bronisław Szakien (1890–1938), cytologist and mycologist; Piotr Wiśniewski (1881––1971), physiologist; and Józef Trzebiński (1867–1941), mycologist and phytopathologist. Ca. 300 publications (including ca. 100 scientific ones) were printed in the period investigated, dealing mainly with morphology and anatomy, cytology, plant physiology, floristics (floristic geography of plants), systematics (taxonomy) of vascular plants, mycology and phytopathology, ecology of plant communities (phytosociology), as well as ethnobotany, and history of botany. Stefan Batory University was also an important centre of teaching and popularization of botany in that region of Europe.

The aim of the article is to describe the history of botany at the Stefan Batory University in 1919–1939.

Keywords: *botanical research, history of botany, Lithuania, Poland, Polish botanists, the interwar period, twentieth century, Vilnius, Wilno, University in Vilna, Stefan Batory University, Jakub Mowszowicz, Jan Muszyński, Bronisław Szakien, Piotr Wiśniewski, Józef Trzebiński*

Botanika na Uniwersytecie Stefana Batorego w Wilnie (Vilna, Vilnius) (1919–1939)

Abstrakt

Uniwersytet w Wilnie (w języku angielskim: Vilna, obecnie: Vilnius w Republice Litewskiej), założony w 1579 r. przez Stefana Batorego, króla Polski i wielkiego księcia Litwy, był ośrodkiem polskiej botaniki w latach 1780–1832 oraz 1919–1939. W tym ostatnim okresie funkcjonował pod nazwą Uniwersytet Stefana Batorego (w języku angielskim: Stefan Batory University).

W latach 1919–1939 zorganizowano następujące zakłady związane z botaniką: Botaniki Ogólnej, Farmakognozji i Hodowli Roślin Lekarskich, Systematyki Roślin, Ogród Botaniczny, Ogród Roślin Lekarskich oraz Muzeum Przyrodnicze.

W ośrodku wileńskim pracowali wybitni uczeni, m.in. Jakub Mowszowicz (1901–1983), fitogeograf i fitosocjolog; Jan Muszyński (1884–1957), botanik i farmaceuta; Bronisław Szakien (1890–1938), cytolog i mykolog; Piotr Wiśniewski (1881–1971), fizjolog oraz Józef Trzebiński (1867–1941), mykolog i fitopatolog. Badacze roślin ogłosili drukiem ok. 300 publikacji (w tym ok. 100 naukowych) dotyczących głównie morfologii i anatomii, cytologii, fizjologii roślin, florystyki (florystycznej geografii roślin), systematyki (taksonomii) roślin naczyniowych, mykologii i fitopatologii, ekologii zbiorowisk roślinnych (fitosocjologii), a także etnobotaniki i historii botaniki. Uniwersytet Stefana Batorego był również ważnym ośrodkiem nauczania i popularyzacji botaniki w tym regionie Europy.

Celem artykułu jest opracowanie historii botaniki na Uniwersytecie Stefana Batorego w latach 1919–1939.

Słowa kluczowe: *badania botaniczne*, *historia botaniki*, *Litwa, okres międzywojenny, polscy botanicy, Polska*, *Wilno*, *Uniwersytet w Wilnie*, *Uniwersytet Stefana Batorego*, *Jakub Mowszowicz*, *Jan Muszyński*, *Bronisław Szakien*, *Piotr Wiśniewski*, *Józef Trzebiński*

1. Introduction

Vilna is a multi-national town situated in Central-Eastern Europe. The town has undergone turbulent political changes, and belonged to various countries: Lithuania, Polish-Lithuanian Commonwealth, Russia, Poland, the Soviet Union, Lithuania, the Soviet Union, Nazi Germany, the Soviet Union, and now – to Lithuania. The university in Vilna (Wilno, Vilnius), now Lithuanian Vilniaus Universitetas, founded in 1579 by Stefan Batory (Stephen Báthory), King of Poland and Grand Duke of Lithuania, was one of the first centres of Polish botany in 1780–1832. In 1780, the Chair of Natural History was established, in 1781[1] – the Botanical Garden, and in 1802 – the Chair of Botany. The employees

[1] Botanical Garden, created in 1781 as an additional unit of the Chair of Natural History, was opened in 1782.

of the university[2] conducted studies on vegetation of lands belonging today to the north-eastern Poland, western Belarus, and Lithuania.[3] In 1795, Poland lost independence, divided between Austria, Prussia, and Russia. The former Vilna University was closed on the orders of the Russian Tsar in 1832. Botany was still taught in Vilna at the Imperial Medical-Surgical Academy. After its closure in 1842, botany was cultivated for the next 77 years by a few amateurs.

After Poland had regained freedom in 1918, the university was reopened on 28 August 1919 under the name Uniwersytet Stefana Batorego (in English: Stefan Batory University). From June to October 1920, the University was closed, because of the Soviet occupation, and resumed its activities only after the victory of the Polish armed forces. The preparatory and organizational work related to the launch of botanical studies was started by Piotr Wiśniewski (1884–1971), former professor at the Warsaw University of Life Sciences.[4] Wiśniewski was one of the organizers of the Stefan Batory University. In the academic year 1919/1920, he was also the first dean of the Faculty of Mathematics and Natural Sciences, which included the science of plants.[5] The faculty began work on 19 September 1919, when the first meeting of the Faculty Council was held.

The short period between 1919 and 1939, when the University functioned, can be referred to as a heroic time – its organizers, and later its employees had to cope with a multitude of problems, starting from political, through personal to financial.[6] Nevertheless, the patriotic enthusiasm of scientists accompanying the resurrection of Polish science helped to overcome gradually the difficulties. On 15 December 1939, the University was closed by the authorities of the Republic of Lithuania (Vilna was transferred to Lithuania by the Soviet Union occupying the city). After the Second World War, as a result of changed borders the town was included in the Soviet Union, and from 1990 – in the Republic of Lithuania.

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[2] At first among others Frenchman Jean Emmanuel Gilibert (1741–1814), later the Poles: Rev. Stanisław Bonifacy Jundziłł (1761–1847), and Józef Jundziłł (1794–1877).

[3] Grębecka 1979; 1998.

[4] Siedlecki 1929, p. 66 (8).

[5] LCVA [see footnote 10], I Bb 784.

[6] Mienicki 1929, p. 125(11); Jaczewski 1978, pp. 39–40.

Fig. 1. Jakub Mowszowicz. University of Łódź Archives: personal files.

Until recently, the main source of knowledge about botany in the University was the papers in Polish by Jakub Mowszowicz (1901–1983) (Fig. 1), written mainly on the basis of his own memories (without archival source research). In some short articles in Lithuanian by Jolita Klimavičiūtė selected issues of botany in the University concerned, among many topics, the history of botany in Lithuania (including Vilna) in the interwar period.[7] In her monograph, the author discussed (only on 15 pages and again in Lithuanian) the above issues a bit more extensively but based on few archival sources.[8]

Despite the existence of the above-mentioned studies, the detailed history of botany at the Stefan Batory University in the interwar period has been still not sufficiently researched and described so far. The aim of this study is to write both the external (institutions, collections, in some extent also biographies) and the internal (branches, methods, published results) history of botany in the University on the basis of very numerous archival documents, and

[7] Klimavičiūtė 1997; 1998a; 1998b.

[8] Klimavičiūtė 2002, pp. 50–64.

printed works. Electronic copies of archival materials were obtained owing to the interdisciplinary research project that has been launched with the purpose to elaborate the history of science at the Stefan Batory University.[9] The archival materials are kept in Lietuvos Centrinis Valstybės Archyvas in Vilna.[10]

2. Botanical and other departments studying plants

Several research units involved in the studies of plants operated in the Stefan Batory University (Table 1). Scientific laboratories and teaching rooms of particular departments had been furnished gradually, after the accommodation and subsidies for repairs were acquired.

Table 1. Organizational units connected with botany in Stefan Batory University (SBU) in Vilna

Year of foundation	**Names**[11]	**Heads (years of life) – years of employment in SBU**
1919	Department of General Botany (Department of Botany I)	Piotr Wiśniewski (1884–1971) – 1919–1939
(1919 – choice of area) 1920	Botanical Garden	Piotr Wiśniewski – 1919–1923 Józef Trzebiński (1867–1941) – 1924–1937 Franciszek Ksawery Skupieński (1888–1962) – 1937–1939

[9] Research Project of the Ministry of Science and Higher Education in Poland – NPRH/912-Bibl. *Hinc itur ad astra* – executed by Nicolaus Copernicus University in Toruń in cooperation with Vilnius University.

[10] Lietuvos Centrinis Valstybės Archyvas [in English: Lithuanian Central State Archives in Vilnius is quoted in this paper as LCVA. Since all archival materials derive from only one file entitled 'F. 175 Akta Uniwersytetu Stefana Batorego w Wilnie' [F. 175 Files of the Stefan Batory University in Vilna] – the file title has been omitted in the following notes.

[11] The most frequently used name of departments (in the brackets other names used in particular years).

Table 1 cont.

1921	Department of Pharmacognosy and Cultivation of Medicinal Plants	Jan Muszyński (1884–1957) – 1921–1939
1923	Garden of Medicinal Plants	Jan Muszyński – 1923–1939
1924	Department of Plant Taxonomy (Department of Botany II, Department of Agricultural Botany, Department of Taxonomy and Plant Geography)	Józef Trzebiński – 1924–1937 Franciszek Ksawery Skupieński – 1937–1939
1928 (1931 – opening)	Natural History Museum	Jan Prüffer (1890–1959) – 1928–1939

2.1. Department of General Botany

In 1919, the Department of General Botany[12] was organized by Piotr Wiśniewski (Fig. 2), a plant physiologist, the first Dean of the Faculty of Mathematics and Natural Sciences (academic years 1919–1920 and 1926–1927), who also created the Botanical Garden (see below). Wiśniewski obtained his doctoral degree (1910) at the Lwów University (now: Lviv, Ukraine) on the basis of a thesis on experimental plant anatomy. Later, he visited Dutch and German botanical laboratories. After returning to his homeland, he held the post of Professor (1913–1919) in the Industrial-Agricultural Courses (from 1916 – Higher Agricultural School) in Warsaw. In 1919–1939, he was a Professor at the Vilna University, obtaining the title of Full Professor in 1920. Piotr Wiśniewski was a devoted academic teacher, and created the so-called Vilna botanical school.[13] Twelve assistant lecturers were appointed in the Department, one to several per year (Table 2). Very low salaries forced them to take additional jobs, usually in primary or secondary schools.[14]

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[12] Department operated under different names listed in Table 1.

[13] In 1944–1959, Wiśniewski was a Full Professor at the Maria Curie-Skłodowska University in Lublin. He died in 1971 in Lublin (Kurancowa 1970, Paszewski 1971, Sałata 1995).

[14] Köhler 2016 a, b, c, d.

Fig. 2. Piotr Wiśniewski. LCVA I Bb nr 784 (Piotr Wiśniewski).

The Department of General Botany was home to the studies in the fields of plant physiology, anatomy, cytology, floristics, phytosociology, and the history of botany.[15] It also housed botanical scientific collections (chiefly herbarium, see below), as well as teaching aids.[16]

Table 2. Assistants employed in the Department of General Botany SBU

Assistants (years of life)	Name of post and years of fulfillment	Later employment
Stefan Kownas (1898–1978)	Volunteer Assistant 1938–1939	after WWII Professor in the Higher Agricultural School in Szczecin[17]
Jakub Mowszowicz (1901–1983)	Volunteer Assistant 1936–1939, in 1936 Ph.D. in SBU	after WWII Professor in University of Łódź[18]

[15] LCVA I Ab 152, I Ab 391, I Bb 784.
[16] LCVA I Ab 314, I Ab 391.
[17] Köhler 2016a.
[18] Hereźniak, Czyżewska 2000.

Table 2 cont.

Anna Niekrasz (1896–1973)	Assistant 1929–1931, Senior Assistant 1 Jan 1931–15 Dec 1939	after WWII research worker in the Nicolaus Copernicus University in Toruń, and in the University of Łódź[19]
Maria Oszurkówna (Bagieńska) (?–?)	Assistant since 1 Nov 1925 during academic year 1926/27 (maybe until the end of academic year 1928/1929)	teacher[20]
Helena Peksza (née Korwin-Kurkowska) (1900–1971)	Assistant 1924–1928	teacher, after WWII research worker of the University of Łódź[21]
Anna Przewłocka (née Moksiewiczówna) (1897–?)	Assistant since 1 Jan 1921, Senior Assistant 1 Oct 1923–31 Dec 1925[22]	later fate unknown
Irena Renigerówna (?–?)	Assistant 1 Oct 1925–30 Sep 1929[23]	later fate unknown
Czesława Rudnicka (?–?)	Assistant 1 Dec 1922–30 Sep 1923[24]	later fate unknown
Antonina Sienicka (1898–1979)	In 1931 Ph.D. in Warsaw University, Assistant 1929–1938, Assistant Professor 1 Nov 1938–15 Dec 1939	after WWII research worker in the Nicolaus Copernicus University in Toruń, later Associate Professor in the Higher Agricultural School in Szczecin[25]

19 Köhler 2016b.
20 LCVA I Ab 391, VII B.
21 Mowszowicz 1976.
22 LCVA I Ab 56, VII B 54, VII B 57, VII B 58.
23 LCVA I Ab 391, F. 175 VII B 59.
24 LCVA VII B 56.
25 Köhler 2016c.

Table 2 cont.

Assistants (years of life)	Name of post and years of fulfillment	Later employment
Witold Sławiński (1888–1962)	Senior Assistant 15 Sep 1919–30 Sep 1923	after WWII Professor in the Maria Curie-Skłodowska University in Lublin, in the Warsaw University of Technology, and in the Medical Academy in Białystok[26]
Bronisław Szakien (1890–1938)	Assistant since 1 Jun 1921, Senior Assistant since 1 Oct 1925, Assistant Professor since 1 Oct 1935, Ph.D. – 1927, 'habilitation' in SBU – 1937[27]	
Kazimiera Wilczyńska (née Urbanowiczówna) (1897–1980)	Assistant since 1929/1930, Senior Assistant 1 Oct 1930–15 Dec 1939	after WWII worked, among others, in the Nicolaus Copernicus University in Toruń[28]

2.2. Department of Plant Taxonomy

In 1924, the Department of Plant Taxonomy[29] started its activities. It was established by a mycologist and phytopathologist Józef Trzebiński (1867–1941) (Fig. 3). In 1897, he obtained the degree of candidate in life sciences at the Imperial University of Warsaw. In 1903, he was conferred a doctoral degree, and in 1920 a post-doctoral degree ('habilitation') in botany in the field of phytopathology of the Jagiellonian University in Kraków. He visited many plant protection stations in western countries of Europe, and in Russia. In 1922–1937, Trzebiński was

[26] Gątkiewiczowa 1963.
[27] Köhler 2016d.
[28] LCVA 1 (I Bb) 1013, VII B 52.
[29] Department operated under different names – see Table 1.

Fig. 3. Józef Trzebiński in his laboratory. Archives of the Polish Academy of Sciences, file reference number: 105.

appointed a Professor in the Vilna University, and from 1923 – a Full Professor. He headed the Department of Plant Taxonomy and the Botanical Garden. In the academic year 1925–1926, he was the Dean of Faculty of Mathematics and Natural Sciences. In 1928, the Mycological Section of the Plant Protection Station in Vilna, managed also by Professor Trzebiński, began its work in association with the Department of Plant Taxonomy. He retired in 1937.[30]

[30] In 1937, Trzebiński was awarded the title of doctor *honoris causa* of the Major School of Rural Economy in Warsaw. He died in Vilna in 1941 (Köhler, Majewski 2016).

The successor of Trzebiński was Franciszek Ksawery Skupieński (1888–1962) – a mycologist, and a researcher of slime moulds. In 1920, he obtained the degree of *docteur des sciences naturelles* at the Sorbonne in Paris, in 1929 – a post-doctoral degree ('habilitation') at the Faculty of Mathematics and Natural Sciences of the University of Warsaw (UW). He worked at the UW (1920–1937) and combined it with employment in the Warsaw Polytechnic (1933–1937). From 1937 to 1939, Skupieński held a professorship in botany at the Vilna University.[31]

Ten assistant lecturers were appointed in the Department of Plant Taxonomy (Table 3). The research area of this unit covered principally mycology, phytopathology, floristics, and phytosociology.[32] It also gathered botanic scientific collections (chiefly herbarium, see below), as well as teaching aids.[33]

Table 3. Assistants employed in Department of Plant Taxonomy SBU

Assistants (years of life)	**Name of post and years of fulfillment**	**Later employment**
Zofia Fiedorowiczówna (1895–1983?)	Assistant since 15 May 1924 until academic year 1928/1929[34]	later fate unknown
Ryszard Kruszyński (1908–1940)	Assistant Volunteer since 1935/1936 until 1939, employed also in Plant Protection Station in Vilna	murdered by NKVD in Kharkiv[35]
Irena Michalska (née Malinowska) (1910–2000)	Assistant 1 Nov 1935–15 Dec 1939	after WWII Professor in the Institute of Animal Physiology and Nutrition, Polish Academy of Sciences, in Bydgoszcz[36]

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[31] From 1945 to 1960, Skupieński was a Professor in the University of Łódź. He died in Łódź in 1962 (Konarski 1998).

[32] LCVA I Ab 152, I Ab 391, I Bb 784.

[33] LCVA I Ab 314, I Ab 391, VII B 232.

[34] LCVA I Ab 391, VII B 57, VII B 58, VII B 59.

[35] LCVA I Ab 314, VII B 52, VII B 200, VII B 232; Majewski 1982; 2016, p. 120.

[36] LCVA A 14 no. 299, VII B 52, VII B 161, VII B 197, VII B 198, VII B 232.

Table 3 cont.

Andrzej Michalski (1904–1973)	Deputy Assistant since 1 Oct 1930, Senior Assistant 1 Sep 1936 (maybe already since 1935)–15 Dec 1939, he was also Inspector i.e. Chief Gardener of the SBU Botanical Garden) since 1936	after WWII research worker in the State Agricultural Scientific Institute – head of the Mycological Division in Bydgoszcz, head of the Bydgoszcz Division of Institute of Plant Protection, National Research Institute, and its Phytopathological Unit[37]
Irena Morawska-Boguszewska (?–?)	Assistant 1 Apr–31 Dec 1926, and 1930/1931–1932/1933 (maybe continuously), Senior Assistant 1933–1935, Ph.D. in SBU in 1932 or in 1933[38]	later fate unknown
Nadzieja Rojecka (1898–1986)	Assistant since 1 Oct 1931, Senior Assistant since 1 Sep 1935 probably until 15 Dec 1939, seasonal Assistant in the Plant Protection Station in Vilna in 1931–1935	after WWII research worker in the Nicolaus Copernicus University in Toruń, Assistant Professor in the State Agricultural Scientific Institute (Państwowy Instytut Naukowy Gospodarstwa Wiejskiego) in Puławy, and the Puławy Division of the Institute of Plant Protection, National Research Institute[39]

[37] Sokołowska-Rutkowska, Piszcz 1975.

[38] LCVA I Ab 314, 1(I A) B 955, VII B 52, VII B 59; Sokołowska-Rutkowska, Piszcz 1975.

[39] Sokołowska-Rutkowska 1988.

Table 3 cont.

Assistants (years of life)	Name of post and years of fulfillment	Later employment
Henryk Rylski (?–?)	Deputy Assistant 1924/1925–1926/27[40]	later fate unknown
Witold Sławiński	Assistant 1 Oct 1923–31 Dec 1925	see Table 2[41]
Irena Sokołowska-Rutkowska (1904–?)	Assistant since 15 Oct 1925 until the end of academic year 1929/1930	teacher of primary school in Dubno (now in Ukraine), after WWII lived in Warsaw[42]
Antoni Zieliński (?–?)	Deputy Assistant 1928/1929[43]	later fate unknown

2.3. Department of Pharmacognosy and Medicinal Plant Breeding

In the Department of Pharmacognosy and Medicinal Plant Breeding, organized in 1921 by a pharmacist and botanist Jan Muszyński (1884–1957) (Fig. 4), apart from other issues, was a place of study of useful plants, chiefly medicinal species, and ethnobotany.[44] Jan Muszyński graduated in 1917 from the Dorpat University (now: Tartu, Estonia) with a Master of Science degree (equivalent to a doctoral degree in the Austro-Hungarian Monarchy). From 1921 to 1939, he worked in the Vilna University, starting in 1923, as an associate Professor and, from 1937, as a full Professor of pharmacognosy and breeding of medicinal plants. Muszyński was the Director of the University Pharmaceutical Study from the academic year 1923–1924.[45] The Garden of Medicinal Plants was established in 1923, on approx. 30,000 square meters, situated near the Botanical Garden.[46]

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[40] LCVA VII B 52.

[41] Gątkiewiczowa 1963.

[42] LCVA I Ab 391, VII B 48, VII B 52, VII B 59.

[43] LCVA I Ab 391.

[44] LCVA I Ab 56, VII B 54.

[45] In 1942, Muszyński moved to Warsaw, where he was active in clandestine teaching. From 1945, he lived in Łódź, where he organised the Faculty of Pharmacy at the Łódź University (UL) of which he was the first Dean (from 1945 to 1951). He died in Łódź in 1957 (Rembieliński 1957).

[46] Rydzewski 1929, p. 405 (29).

Fig. 4. Jan Muszyński. The National Digital Archives (Warsaw, Poland), file reference number: 1-N-403.

Herbaria and other botanical collections

Each of the two botanical departments independently accumulated scientific collections, including herbaria. The Department of General Botany was the first to be organized. Probably for this reason, this Department received herbaria from the Vilna Medical Society and from the natural museum at the Vilna Public Library, where the herbaria had been kept earlier.[47] They included collections by botanists associated with the former University of Vilna: Willibald Besser (1784–1842), Edward Karol Eichwald (1795–1876), Józef Fiedorowicz (1777–1860), Stanisław Batys Górski (Gorski) (1802–1864), Jerzy Pabreż (1771–1849), Jan Fryderyk Wolfgang (1775–1859), or botanists with whom S. B. Górski exchanged dried plants, e.g. Wojciech Jastrzębowski (1799–1882).[48] The collections by botanists of the 19th–20th centuries, as Maria

[47] Mowszowicz 1957, p. 16.

[48] Köhler 1994.

Fig. 5. Bronisław Szakien. LCVA I Bb nr 637.

Burhardtówna, Vsevolod Izmailskiy, S. Lavrov, Wacław Hryniewski, January Kołodziejczyk and Józef Niekrasz, were acquired as deposits of the Society of Friends of Sciences based in Vilna.[49] The interwar period witnessed the arrival of collections accumulated by Piotr Wiśniewski, Konstanty Prószyński, Bronisław Szakien (Fig. 5), Jakub Mowszowicz, and students.[50] Unfortunately, archival sources do not contain precise data on these collections.

The beginning of the herbarium at the Department of Plant Taxonomy was a collection brought by Professor Józef Trzebiński from Puławy.[51] The collection consisted of samples of seeds, fruits, timbers and macromycetes.[52] In the interwar period, the herbarium of the Department was enlarged by collections accumulated by its employees as well as graduate students (Table 4), for example Nadzieja Rojecka (Fig. 6).

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[49] Mowszowicz 1966 pp. 106–107.
[50] LCVA VII B 232.
[51] LCVA VII B 52.
[52] Mowszowicz 1966 pp. 109, 111; Rydzewski 1929, p. 334.

Fig. 6. Nadzieja Rojecka. Archives of the Polish Academy of Sciences, Ananiasz Rojecki files, file reference number: III-230, j. 72.

Table 4. Collections donated to the Herbarium of the Department of Plant Taxonomy SBU

Year	Donator	Area	Donations
1922[53]	Józef Trzebiński	no data	samples of seeds, fruits, timbers and macromycetes
1924[54]	Józef Trzebiński	surroundings of Łowicz	flowering plants
	Witold Sławiński	Savoy Alps and Piedmont	flowering plants
	Zofia Fiedoro-wiczówna	the Dzisna district (now in Belarus)	zoocecidia

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[53] Mowszowicz 1966, pp. 109, 111; Rydzewski 1929, p. 334.

[54] LCVA I Ab 152.

Table 4 cont.

Year	Donator	Area	Donations
before 1929[55]	Witold Sławiński	the Zielone Jeziora (now: Žalieji ežerai, Lithuania) area	flowering plants (800 species)
	Irena Sokołowska	Międzyrzecz in the Rudnicka (now: Rūdninkai, Lithuania) Ancient Forest	flowering plants (400 species)
	Helena Krzyżanowska	surroundings of Nowe Święciany (now: Švenčionėliai, Lithuania)	flowering plants (300 species)
	Zofia Fiedoro-wiczówna	the Dzisna (now in Belarus) district	zoocecidia (120 species)
	Józef Trzebiński, Konstanty Prószyński	north-eastern Poland	fungi
1928–1929[56]	no data	no data	vascular plants, pathogenic fungi
	no data	no data	mountain plants
	no data	no data	relict plants
1929–1930[57]	Józef Trzebiński	lakes near Troki (now: Trakai, Lithuania)	plants from peat bogs and lakes
	no data	no data	lichens (400 species)

55 Rydzewski 1929, pp. 334–335.
56 LCVA I Ab 391.
57 LCVA VII B 52.

Table 4 cont.

1930–1931[58]	F. Majer	Tatry Mts.	no data
	M. Szmukler	surroundings of Włodzimierz Wołyński (now: Volodymyr-Volynskyi, Ukraine)	no data
	Janina Perepeczkówna	surroundings of Oszmiana (now: Ashymany, Belarus)	zoocecidia
	no data	no data	didactic collection of more important flowering plants and spore ones
	no data	no data	preserved specimens (wet and dry) (100 pieces) and microscopic preparations (several dozen)
1931–1932[59]	Janina Perepeczkówna	surroundings of Oszmiana	zoocecidia
	Nadzieja Rojecka	the Karaim cemetery of Troki	flowering plants
	Józef Trzebiński	Dalmatia	flowering plants
	no data	the University Botanical Garden	geobotanical collection of plants
1933–1934[60]	Ita Frydmanówna	surroundings of Zamość	flowering plants
	Ryszard Kruszyński	surroundings of Lida (now in Belarus)	parasitic fungi

[58] LCVA VII B 52.
[59] LCVA VII B 52; VII B 52.
[60] LCVA I Ab 314; VII B 48.

Table 4 cont.

Year	Donator	Area	Donations
	Jadwiga Matuszkiewiczówna	surroundings of Vilna	algae[61]
	Edward Ząbek	surroundings of Pińsk (now: Pinsk, Belarus)	flowering plants
1934–1935[62]	Lidia Anisimowiczówna	the Sokółka district	flowering plants
	Irena Rutkowska	Warszawa and Pomorskie voivodeships	zoocecidia
	Andrzej Michalski	Vilna, Nowe Troki and surroundings	lichens, parasitic fungi
1935–1936[63]	Feliks Krawiec	surroundings of Poznań	lichens (gift of the Poznań University)
	Zinaida Sinicynówna	surroundings of Nieśwież (now: Nyasvizh, Belarus)	collection of peat plants
	Maria Kostrowicka	surroundings of Brześć nad Bugiem and Kobryń (now: Brest and Kobryn, Belarus)	mosses
	Gienia Szulmanówna	surroundings of Dołhinów (now: Daūhinava, Belarus)	flowering plants
	Zuzanna Gołąbówna	surroundings of Druskienniki (now: Druskininkai, Lithuania)	flowering plants
	Andrzej Michalski	surroundings of Vilna	slime moulds
1938–1939[64]	no data	surroundings of Vilna	collection of meadow and pasture plants

61 LCVA I Ab 314.
62 LCVA VII B 52.
63 LCVA VII B 52.
64 LCVA VII B 198.

In 1931, the Natural History Museum of the Stefan Batory University came into being and housed mainly zoological, geological, palaeontological specimens, and only some herbaria, plant drawings, and photographs of plant life of the Vilna province.[65]

2.4. Botanical Garden

Botanical Garden, organized in the years 1919–1920, was presented in the paper *Historia Ogrodu Botanicznego Uniwersytetu Stefana Batorego w Wilnie (1919–1939)*.[66]

3. Research activities

3.1. General characteristics

The initial decades of the 20th century were characterized by an increased rate of development in the field of biological science, including botany. The development pertained to both laboratory studies in the areas of anatomy, cytology, and physiology of plants, as well as to the studies combined with field research – taxonomy, geography of plants (phytogeography), palaeobotany, and ecology (including phytosociology).[67] These developments were only scarcely reflected in Polish science.[68] The flora of Central and Eastern Europe, including the region of Vilna, and particularly the lower plants and fungi were still poorly known. The Vilna centre of botany was clearly associated with practice and served as a means to elevate the level of agriculture and medical care in the north-eastern borderlands of the then Poland. The studies into the history of botany in Vilna were the signs of interdisciplinary interests and of the past of the University. The Vilna botanists stayed in touch with other Polish research centres, e.g. with the Jagiellonian University operating continuously since 1364, as well as with younger universities, e.g. the University of Warsaw, and the Major School of Rural Economy in Warsaw. They participated in the work of the Polish Botanical Society, established in 1922, and organized its

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[65] LCVA I Ab 311, I Ab 314, I Ab 391; VII B 52; VII B 229; 1(IA) 919.

[66] Zemanek, Köhler 2016.

[67] Mägdefrau 2004, pp. 274–280, 328–331; Morton 1981, pp. 448–466; Zemanek 2011.

[68] Hryniewiecki 1949, pp. 120–126; Kosiek 1983, pp. 455–470; Paszewski 1973.

four national conventions in Vilna (5th Convention – 23–24 May 1926, 7th Convention – 8–10 June 1928, 13th Convention – 29 June–1 July 1934, and 18th Convention – 27–29 May 1939).[69] Foreign contacts included trips to conferences, and – in single cases – longer stays abroad, e.g. Witold Sławiński (France, Paris, 1923–1924),[70] Bronisław Szakien (Belgium, Leuven, 1925–1926 and 1935),[71] and Wacław Strażewicz (Hungary, including Budapest, and Austria, Vienna, 1936–1937).[72] Franciszek Ksawery Skupieński was a member of Société Botanique de France.[73]

3.2. More important publications

Initially, the rate of scientific work measured by the annual number of publications was low, but it accelerated during the 1930s. A total of approx. 300 works were published, including about 100 scientific papers dealing with several specialties (Diagram 1). A great number of these

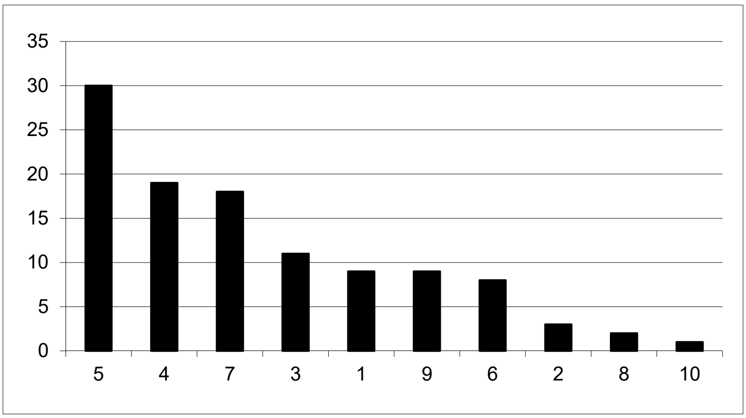

Diagram 1. Numbers of papers in particular specialties. 1 – morphology, anatomy and cecidiology, 2 – cytology, 3 – physiology, 4 – floristics and systematics of vascular plants, 5 – mycology and phytopathology, 6 – phytosociology, 7 – useful and medicinal plants, 8 – ethnobotany, 9 – history of botany, 10 – palaeobotany.

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[69] LCVA I Ab 314, I Ab 391, I Bb 59, VII B 52, VII B 198.
[70] LCVA VII B 57, 1 (I Bb) b 898.
[71] LCVA I Bb 637, VII B 265.
[72] LCVA 1 (I Bb) 52.
[73] Konarski 1998; WG 2015.

are rare prints now, available in single copies. The results of scientific studies were generally published in Polish, with brief summaries in German or French. Most of the papers were published in the local journal *Prace Towarzystwa Przyjaciół Nauk w Wilnie. Wydział Nauk Matematycznych i Przyrodniczych* and in the nationally circulated *Acta Societatis Botanicorum Poloniae*. Few papers were published in foreign languages, particularly in German, principally in the latter periodical, and even fewer in journals abroad. Some of the research subjects initiated by the botanists during their employment in the University were concluded only after the Second World War and published in other Polish journals.

3.2.1. Morphology and anatomy – zoocecidiological research

Additionally, there were studies conducted in cecidiology, a science on the border between botany, zoology, and phytopathology. Cecidia are abnormal outgrowths on various plant organs induced by foreign organisms such as insects or nematodes. The above subject matter, which had been studied for many years, had economic significance, because it revealed potential threat to cultivated plants.[74] The research of cecidia in the Vilna region was among the first such studies in Polish botany.[75] Examples are provided by the works of Wiśniewski's students – Zofja Fiedorowiczówna,[76] Olga Kotlarówna,[77] and Leia Wolpjanówna.[78]

3.2.2. Cytology

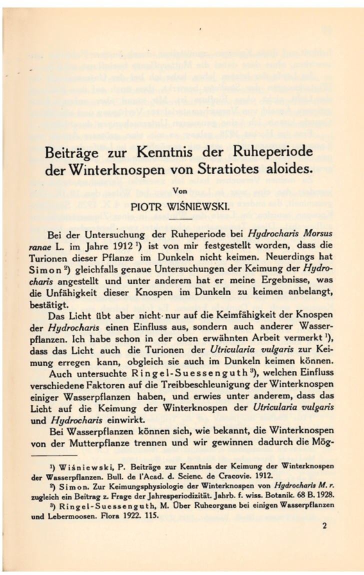
Bronisław Szakien was a researcher who specialized in cytology under the supervision of Victor Grégoire (1870–1938), a cytologist and caryologist at the Catholic University of Leuven (Université Catholique de Louvain) in Belgium, during his stays in 1925–1926 and 1935. The studies on the course of nuclear division in the prophase of meiosis in royal fern (*Osmunda regalis* L.) allowed B. Szakien to find details hitherto known only from the analysis of material obtained from animals.[79]

[74] Mägdefrau 2004, p. 200; Szweykowscy 2003, pp. 1015–1016.
[75] Mowszowicz 1973.
[76] Fiedorowiczówna 1931.
[77] Kotlarówna 1934.
[78] Wolpjanówna 1934.
[79] Wiśniewski 1938a, p. XXXIII.

These results were the basis for his doctoral dissertation at the University in 1927;[80] whereas, later studies on the meiotic division of nucleus in two species of horsetails (*Equisetum*) provided the basis of his post-doctoral dissertation ('habilitation') in 1937.[81]

3.2.3. Plant physiology

A plant physiologist Piotr Wiśniewski developed his own scientific school, primarily including the M.Sc. students of the Department of General Botany. He dealt with the dormancy period in plants and with the effects of various factors on the artificial regulation of its length. His studies on the germination of turions i.e. dormant buds in aquatic plants (e.g., water soldier *Stratiotes aloides* L.) formed in autumn, wintering at the bottoms of bodies of water, and surfacing in spring[82] (Fig. 7a, b), were highly acclaimed by specialists.[83] Many of Wiśniewski's ideas



Beiträge zur Kenntnis der Ruheperiode der Winterknospen von Stratiotes aloides.

Von

PIOTR WIŚNIEWSKI.

Bei der Untersuchung der Ruheperiode bei *Hydrocharis Morsus ranae* L. im Jahre 1912 [1]) ist von mir festgestellt worden, dass die Turionen dieser Pflanze im Dunkeln nicht keimen. Neuerdings hat Simon [2]) gleichfalls genaue Untersuchungen der Keimung der *Hydrocharis* angestellt und unter anderem hat er meine Ergebnisse, was die Unfähigkeit dieser Knospen im Dunkeln zu keimen anbelangt, bestätigt.

Das Licht übt aber nicht nur auf die Keimfähigkeit der Knospen der *Hydrocharis* einen Einfluss aus, sondern auch anderer Wasserpflanzen. Ich habe schon in der oben erwähnten Arbeit vermerkt [1]), dass das Licht auch die Turionen der *Utricularia vulgaris* zur Keimung erregen kann, obgleich sie auch im Dunkeln keimen können.

Auch untersuchte Ringel-Suessenguth [3]), welchen Einfluss verschiedene Faktoren auf die Treibbeschleunigung der Winterknospen einiger Wasserpflanzen haben, und erwies unter anderem, dass das Licht auf die Keimung der Winterknospen der *Utricularia vulgaris* und *Hydrocharis* einwirkt.

Bei Wasserpflanzen können sich, wie bekannt, die Winterknospen von der Mutterpflanze trennen und wir gewinnen dadurch die Mög-

[1]) Wiśniewski, P. Beiträge zur Kenntnis der Keimung der Winterknospen der Wasserpflanzen. Bull. de l'Acad. d. Scienc. de Cracovie. 1912.

[2]) Simon. Zur Keimungsphysiologie der Winterknospen von *Hydrocharis M. r.* zugleich ein Beitrag z. Frage der Jahresperiodizität. Jahrb. f. wiss. Botanik. 68 B. 1928.

[3]) Ringel-Suessenguth, M. Über Ruheorgane bei einigen Wasserpflanzen und Lebermoosen. Flora 1922. 115.

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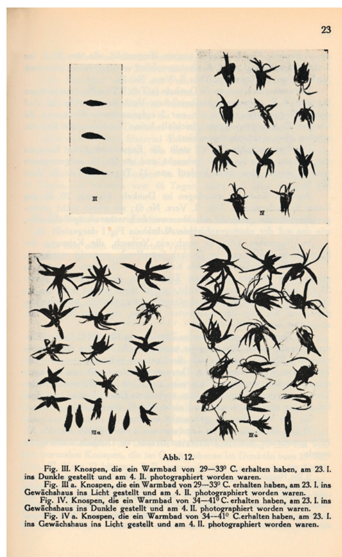

Fig. 7. *Beiträge zur Kenntnis der Ruheperiode der Winterknospen von Stratiotes aloides* (Wiśniewski, 1930): a. title-page, b. pictures.

[80] Szakien 1927.

[81] Szakien 1936; 1937a.

[82] Wiśniewski 1930.

[83] Kurancowa 1970, p. 66.

were implemented by his students in their M.Sc. theses, e.g., by Marja Oszurkówna,[84] and Nojma Goldmanówna.[85]

3.2.4. Floristics (floristic geography of plants) and the taxonomy of vascular plants

The employees of the University undertook the studies of flora (i.e. species occurring in a given area) and vegetation (i.e. plant communities – the works on the latter topic were classified in the field of phytosociology, see below), concentrating particularly on the nearest territory of the Vilna region.[86] Several dozens of papers of various grades, from short floristic notes to more voluminous monographs of tens of pages (regional floras), were published. For example, Witold Sławiński published a monograph on the flora and vegetation of the Zielone Jeziora (now: Žalieji ežerai, Lithuania) region near Vilna[87] (Fig. 8a, b, c). Jakub Mowszowicz, phytogeographer and phytosociologist, was one of the most active researcher of vegetation cover, working for 15 years without pay as an Assistant Lecturer-Volunteer, while earning his living as a teacher.[88] His doctoral dissertation, on the Ponary Mountains (now: Paneriai in Vilnius) and its neighbourhood,[89] defended in 1936, was a floristic-phytosociological monograph containing lists of both vascular plants, as well as mosses, liverworts, fungi, and lichens. Summing

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[84] Oszurkówna 1929.

[85] Goldmanówna 1931; 1933.

[86] Mowszowicz 1959, p. 5.

[87] Sławiński 1924.

[88] Jakub Mowszowicz, who was of Jewish origin, had dramatic fates during the II World War. Initially, he worked at the Lithuanian University, but after Vilna was occupied by the German Nazi army, he was moved to the Vilna ghetto in July 1941 (Mowszowicz 1968, p. 245). In 1943, after all his family was murdered in front of his eyes, when digging a grave for himself as ordered by a Nazi, he succeeded to escape. He survived until the end of the German occupation, hidden by female colleagues from the University. Many years later, he wrote about it in the following words: "[…] Thanks to the endeavors of a noble lady [...] Anna Niekrasz, after long searches and vicissitudes, I have found shelter and care in our common colleague from the University, Weronika Milewska, where I can survive the last ten months of the dark night of Nazi occupation. I wish to pay here my great homage to my Polish colleagues – who came to the rescue of their co-worker irrespective of his origin" (Mowszowicz 1976a).

[89] Mowszowicz 1937a; 1938a.

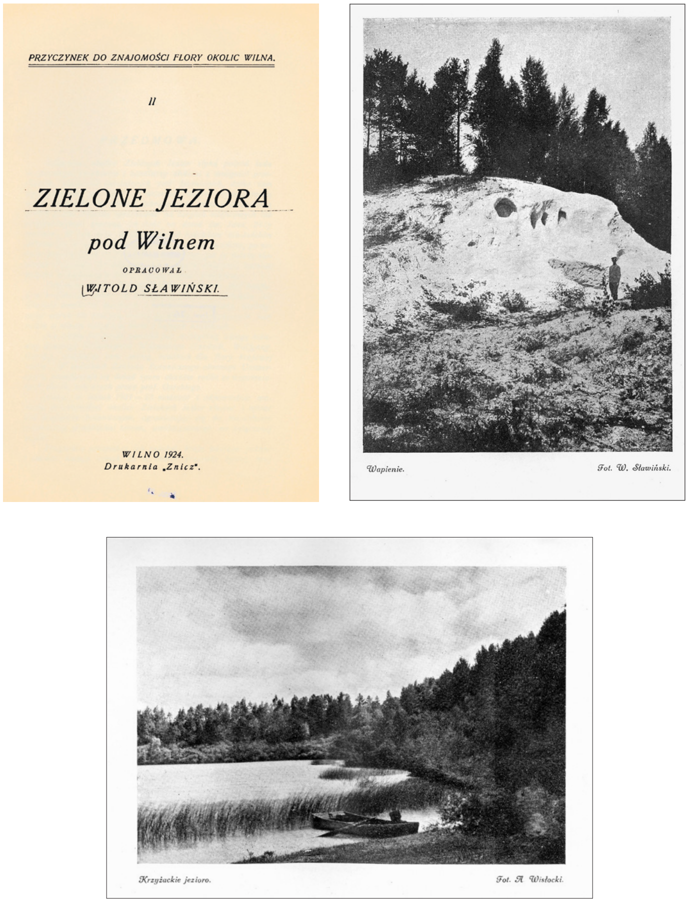

Fig. 8. *Zielone Jeziora pod Wilnem* (Sławiski, 1924): a. title-page, b. photo after the page 20, c. photo after the page 178.

up the research to date conducted in the Vilna region, it contained a list of vascular plants together with the list of all known localities.[90] Unfortunately, the publication, dated in 1938, covered only a part of the edited text, the rest of it was lost during war.[91] Many years later,

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[90] Mowszowicz 1938c.

[91] J. Mowszowicz wrote about it in the 3rd part of *Conspectus*..., 1959 (see below), on p. 5, whereas on p. 96 provides the following reference to this work: *Wykaz roślin naczyniowych Wileńszczyzny z wyszczególnieniem dotychczas podanych stanowisk (Pteridophyta, Gymnospermae, Angiospermae*, *Monocotyledones, Dicotyledones do Rosaceae)* ['List of vascu-

J. Mowszowicz restored this work in a extended version in the form of a three-part monograph, which constituted the basis for his post-doctoral dissertation ('habilitation') at the Łódź University.[92] It is one of the more important syntheses pertaining to the studied area, and it still serves as a reference point to any floristic-phytogeographic considerations.

3.2.5. Mycology and phytopathology

A record in mycology and phytopathology was among the greatest achievements of the Vilna centre. Phytopathology, a science of diseases in plants, has a wider scope than mycology, but in the Vilna centre, the work concentrated mainly on fungal diseases. Several dozens of studies of diversified volumes were published, and some of them were pioneer works in this part of Europe. One of the leading mycologists was Józef Trzebiński, who was regarded as one of the joint creators of Polish phytopathology.[93] The classic item in the list of Trzebiński's publications was his textbook *Choroby roślin: Ogólna fitopatologia* (Diseases in plants: General phytopathology)[94] (Fig. 9a, b). Konstanty Prószyński, another experienced mycologist, was the author of the list of higher fungi of the *Hymenomycetes* order, classified within the *Basidiomycetes*, collected in the neighbourhood of the town of Troki (now: Trakai, Lithuania) (with the descriptions of eight new species)[95] (Fig. 10a, b, c). Unfortunately, the work of his life, devoted to fungi, was not published, and disappeared in unknown circumstances.[96] Piotr Wiśniewski described it in the following words:

> He prepared and left in manuscript an exhaustive work, containing detailed descriptions in Latin of the numerous fungal species he found, including many credited as new species. The manuscript was accompanied by an atlas of these fungi, which contained several thousands of colour tables

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lar plants of Vilna and its vicinity including all known localities (*Pteridophyta, Gymnospermae, Angiospermae, Monocotyledones, Dicotyledones* – till *Rosaceae*)], published by the Department of General Botany, Vilna 1938, pp. 1–160.

[92] Mowszowicz 1957; 1958; 1959.

[93] Majewski 2016, pp. 120, 251; Köhler, Majewski 2016.

[94] Trzebiński 1930a.

[95] Prószyński 1931.

[96] Some of the drawings he made were recently found in collections of Vilnius University (Rukšėnienė, Jonynaitė 2006).

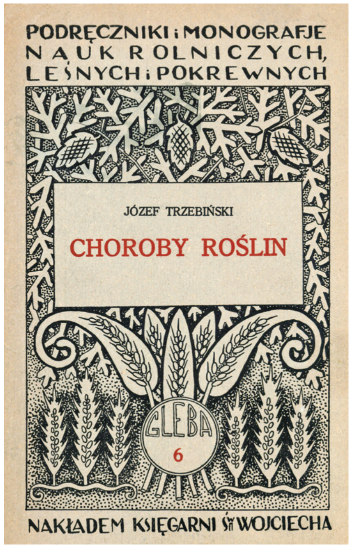

Fig. 9. *Choroby roślin: Ogólna fitopatologia* (Diseases in plants: General phytopathology) (Trzebiński, 1930a): a. cover, b. page 123.

> drawn with by the talented author. […] For many years he strived to publish a major work […] and a tragedy of his life that accelerated […] his death was the impossibility to publish it because of great cost of such a publication.[97]

Bronisław Szakien studied parasitic rust fungi.[98] Ryszard Kruszyński published, among other works, a critical review of plant diseases observed in north-eastern Poland.[99] Andrzej Michalski was the author of, *inter alia*, an article on parasitic fungi found in the area of Vilna-Troki,[100] and on lichens,[101] as well as the first-ever Polish study of lichenicolous fungi.[102] Franciszek Skupieński conducted studies on the ecology of several species of myxomycota (slime moulds) jointly with Andrzej Michalski and Irena (Malinowska-) Michalska.[103]

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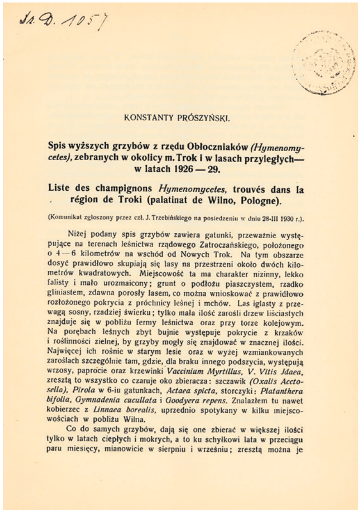
[97] Wiśniewski 1938b.
[98] Szakien 1929; 1935; 1937b.
[99] Kruszyński 1938; Majewski 2016, p. 120.
[100] Michalski 1936.
[101] Michalski 1935.
[102] Michalski 1937.
[103] Michalska, Skupieński 1938; Michalski, Skupieński 1939.



KONSTANTY PRÓSZYŃSKI.

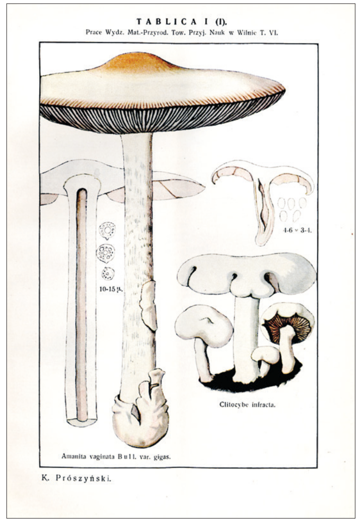
Spis wyższych grzybów z rzędu Obłoczniaków *(Hymenomycetes)*, zebranych w okolicy m. Trok i w lasach przyległych— w latach 1926 — 29.

Liste des champignons *Hymenomycetes*, trouvés dans la région de Troki (palatinat de Wilno, Pologne).

(Komunikat zgłoszony przez czł. J. Trzebińskiego na posiedzeniu w dniu 28-III 1930 r.).

Niżej podany spis grzybów zawiera gatunki, przeważnie występujące na terenach leśnictwa rządowego Zatroczańskiego, położonego o 4 — 6 kilometrów na wschód od Nowych Trok. Na tym obszarze dosyć prawidłowo skupiają się lasy na przestrzeni około dwóch kilometrów kwadratowych. Miejscowość ta ma charakter nizinny, lekko falisty i mało urozmaicony; grunt o podłożu piaszczystem, rzadko gliniastem, zdawna porosły lasem, co można wnioskować z prawidłowo rozłożonego pokrycia z próchnicy leśnej i mchów. Las iglasty z przewagą sosny, rzadziej świerku; tylko mała ilość zarośli drzew liściastych znajduje się w pobliżu fermy leśnictwa oraz przy torze kolejowym. Na porębach leśnych zbyt bujnie występuje pokrycie z krzaków i roślinności zielnej, by grzyby mogły się znajdować w znacznej ilości. Najwięcej ich rośnie w starym lesie oraz w wyżej wzmiankowanych zaroślach szczególnie tam, gdzie, dla braku innego podszycia, występują wrzosy, paprocie oraz krzewinki *Vaccinium Myrtillus, V. Vitis Jdaea*, zresztą to wszystko co czaruje oko zbieracza: szczawik *(Oxalis Acctosella)*, *Pirola* w 6-iu gatunkach, *Actaea spicta*, storczyki: *Platanthera bifolia, Gymnadenia cucullata* i *Goodyera repens*. Znalazłem tu nawet kobierzec z *Linnaea borealis*, uprzednio spotykany w kilku miejscowościach w pobliżu Wilna.

Co do samych grzybów, dają się one zbierać w większej ilości tylko w latach ciepłych i mokrych, a to ku schyłkowi lata w przeciągu paru miesięcy, mianowicie w sierpniu i wrześniu; zresztą można je



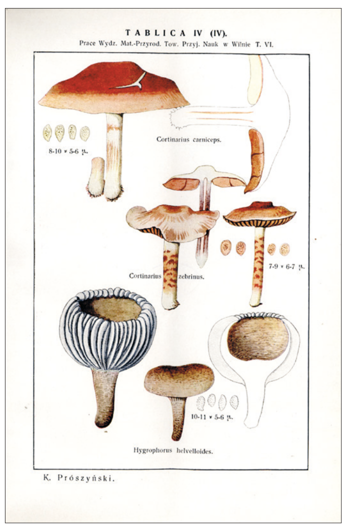


Fig. 10. *Fig. 10. Spis wyższych grzybów z rzędu Obłoczniaków (Hymenomycetes)* […]. *Liste des champignons Hymenomycetes* […] (Prószyński 1931): a. title-page, b. tab. I, c. tab. IV.

3.2.6. Ecology of plant communities (phytosociology)

The dynamic development of ecology was also reflected in the activities of Vilna botanists. The majority of studies was devoted to the ecology of plant communities (synecology) and employed the methodology of the French-Swiss phytosociological school of Josias Braun-Blanquet (1884–1980), introduced into Poland in 1923 by botanists from Kraków.[104] Trzebiński[105] wrote the chapter on the vegetation of the Vilna region, included in the collective work *Wilno i Ziemia Wileńska* (Vilna and the Vilna region).[106] His voluminous work *Flora jezior Trockich* (The flora of Troki lakes) also included a description of plant communities, prepared for printing in the series *Krajobrazy roślinne Polski* (Vegetational landscapes of Poland), published by Zygmunt Wóycicki, was unfortunately destroyed during the Second World War in the printing house of the Warsaw Scientific Society.[107] The other important phytosociological studies which should be mentioned were the works by Irena Sokołowska-Rutkowska on the Rudnicka (now: Rūdninkai, Lithuania) Ancient Forest,[108] Zinaida Sinicynówna on peatlands near Nieśwież (now: Nyasvizh, Belarus),[109] as well as the study by Sergiusz Macuk concerning the area of Koziany, a small town in the Brasław district (now: Kazyany near Braslaū, Belarus).[110]

3.2.7. Studies on useful plants

The studies on useful plants, chiefly medicinal, were conducted by Jan Muszyński and his disciple Wacław Strażewicz. They played a great role in the development of Polish herbal medicine, as well as on the acclimatization of species of foreign origin. Their studies were included in the range of pharmacognosy and were only partly related to botany. These two authors also published a monograph on the history and use in Poland of an oil-providing plant – soya bean (*Glycine max*), originating from Asia.[111] They also conducted breeding work in the Garden of Medicinal Plants, e.g. they bred a new variety of soya bean of high oil content that they named 'Vilna soya bean', which later was commercially traded.

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[104] Zemanek *et al.* 2010, pp. 76–77.

[105] Trzebiński, working jointly with Edward Strumpf, was the author of the Polish translation of the classic work by the pioneer of ecology and phytosociology Eugenius Warming *Plant communities* (1900), whose editions were published in many languages.

[106] Trzebiński 1930b.

[107] Köhler, Majewski 2016, p. 53.

[108] Sokołowska-Rutkowska 1933.

[109] Sinicynówna 1936.

[110] Macuk 1938.

[111] Muszyński, Strażewicz 1933.

3.2.8. Ethnobotany

One of the important currents in the interests of Jan Muszyński was the folk knowledge of plants. He searched for active compounds in club-mosses, which he studied not only experimentally but also in the field where gathered the knowledge of their use in folk medicine. In 1927, he published an article that is now considered to be one of the most interesting ethnobotanical accounts related to the Vilna region.[112] It documented plants sold during St. John's fair held annually on the 24 June near the St. Andrew Church in Vilna (the article listed more than a hundred species, with their vernacular names and information on their uses).[113]

3.2.9. History of botany

After the Second World War, Witold Sławiński and Jakub Mowszowicz became historians of botany, acting simultaneously in other areas of botany. As researchers of the past, they dealt, e.g., with the history of the earlier University of Vilna, as well as with the biographies of botanists working there in the 18th and 19th centuries. The 1922 publication by Sławiński compiled the bibliography of their publications.[114] Also published were two of his studies devoted to the life and activities of Jean Emmanuel Gilibert, a French botanist and the first Professor in Natural History in the earlier University of Vilna.[115] After the Second World War publications included a book on Stanisław Bonifacy Jundziłł, who was a pioneer of the Polish studies in floristics.[116] After the war, Jakub Mowszowicz published many papers on the history of botany in the Stefan Batory University, which were referenced in this publication.

4. Popularization of science

Spreading the knowledge of natural science and of its practical aspects in agriculture was an important area of activity among the botanists, who published popular scientific books, articles in the press, delivered

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112 Petkevičius et al. 2014, p. 60.

113 Muszyński 1927. J. Mowszowicz (*Conspectus* III, p. 96) cited also a wider version of this work (J.Muszyński, *Wileńskie zioła ludowe*, Warsaw 1927, pp. 1–64).

114 Sławiński 1922.

115 Sławiński 1925–1926; 1926.

116 Sławiński 1947.

public lectures, and organized conferences, as well as various courses. Many years later, Maria Ławrynowicz, Professor of the Łódź University, gave the following account of Professor J. Mowszowicz's motives: "[…] Professor J. Mowszowicz treated the popularization of science as his duty as a citizen, and his mission as a scientist in the public arena".[117]

More than half of the printed output of the botanists (nearly 200 publications) were popular papers, communications and books. Józef Trzebiński, the author of many articles, was among the leading popularisers of phytopathology and mycology.[118] Jan Muszyński published more than 100 short articles and communications aiming, among other purposes, at disseminating the knowledge of pharmacy, herbal medicine and the cultivation of medicinal plants. Jakub Mowszowicz wrote two books addressed primarily to students and teachers,[119] and two botanical guides with keys for plant determination.[120]

5. Teaching botany

The botanists were widely involved in teaching activities. They ran courses for both the students of the Faculty of Mathematics and Natural Sciences, where botanical departments were situated, as well as – to a lesser extent – for the students of the Pharmaceutical Study, initially operating at this Faculty, and in 1921 transformed into the Pharmaceutical Section of the Faculty of Medicine.[121] Additionally, they also worked for the Agricultural Study, established in 1924.[122] In particular years, Professor P. Wiśniewski delivered several courses of lectures in general botany, taxonomy, morphology, anatomy and cytology, the physiology of plants as well as the biology of reproduction. He was considered to be an excellent lecturer: "The lectures delivered by Professor P. Wiśniewski were precise, clear, well-structured, and very carefully prepared. They were magnificently supplemented by numerous tables, illustrations, and the use of epidiascope".[123]

117 Ławrynowicz 2003, pp. 87–88.
118 E.g. Trzebiński 1925.
119 Mowszowicz 1937b; 1938b.
120 Mowszowicz 1939a; 1939b.
121 Trzebiński 1929, p. 404 (28).
122 Rydzewski 1929, p. 288 (16).
123 Mowszowicz 1966, p. 104.

Fig. 11. South-African plant *Haemanthus albiflos* Jacq. – picture by Konstanty Prószyński. Description of the picture: *Fam.: Amaryllidaceae R. Br. Haemanthus albiflos* Jacquin *Evasis Horti bot. Vilnensis. Fl. Septembri m. usque in mediam hiemem. Terra Capensis* (Reproduced by permission of the Vilniaus universiteto botanikos sodas).

In various academic years, Professor J. Trzebiński lectured principally in general botany, morphology, and the taxonomy, plant physiology, phytogeography, ecology, and phytopathology.[124] Assistant Lecturers conducted laboratory classes, courses in the Botanical Garden, and floristic trips in the Vilna area (smaller groups were travelling in the Garden-owned cart drawn by a pair of horses). K. Prószyński, a talented artist, painted many images of plants, both wild and cultivated in the Garden (Fig. 11) (there are at least 559 such paintings), and of fungi (several hundreds), which were used as illustrations during lectures and

[124] The curriculum was planned for particular years of studies: Geography of plants (Geo-botany), Genetic and floristic geography of plants – 1928/29, 1930/31, Geobotany (Geography of plants) – 1927/28, 1937/38, as above in part I. Genetic and floristic geography – 1936/37, as above in part II. Genetic geobotany (fossil plants) and floristic botany – 1932/33, Geography of plants (Geobotany), Ecological and phytosociological geography of plants – 1929/30, 1931/32, Geobotany (Geography of plants) part I. Ecology and phytosociology – 1933/34–1935/36 (based on the list of lectures).

practical studies. The number of participants of botanical courses was great. During the activity of the University, it totalled more than 2000 people (estimated with great approximation). The number of persons specializing in botany and working on theses for a degree of Master of Science was relatively low – from one to ten people in an academic year – but still it gave the sum of several dozens of such theses throughout the period of the University's activities.

6. Conclusions

The Stefan Batory University was established in 1919 and operated until 1939. Its botanical activity may be summarized in the following points:

1. The organizational basis for botany in Vilna was created. Two botanical departments were organized and one devoted to pharmacognosy and the breeding of medicinal plants, along with the Botanical Garden, the Garden of Medicinal Plants, and the Museum of Natural History.
2. The years 1919–1939 were characterized by an increasing pace of development of botany studies in the world. This development was reflected more or less at the leading Polish universities, in Kraków, Lwów, Warsaw and Poznań. Stefan Batory University, newly renovated, located in the Eastern borderlands of the state, belonged to more modest botanical centers. What prevailed was the reception of ideas from other Polish and foreign universities, among others in Belgium and France.
3. Nevertheless, research activity in Vilna included several specialties of modern botany of that time pertaining to both laboratory studies in the areas of anatomy, cytology and physiology, as well as to the studies combined with field research, i.e. floristics, taxonomy, geography of vascular plants (phytogeography), ecology (including phytosociology), mycology, phytopathology, and others. Additionally, interdisciplinary studies in ethnobotany were conducted as well as in the history of botany.
4. In this centre worked distinguished botanists, as: Jakub Mowszowicz, phytogeographer and phytosociologist; Jan Muszyński, botanist and pharmacist; Bronisław Szakien, cytologist and mycologist; Piotr Wiśniewski, physiologist; and Józef Trzebiński, mycologist and phytopathologist.

5. Publication of ca. 300 papers (including about 100 scientific ones) of which several dozens were of good scientific quality. They were published chiefly in Polish, some of them with brief summaries in foreign languages, i.e. German and French. Publications on morphological and anatomical research of cecidia and phytopathology were among the first such studies in this part of Europe. The works on the vegetation of the Vilna region are important, because they include historical data on biological diversity.
6. Stefan Batory University was a dynamic centre of teaching botany, of popularization of natural science and of phytopathology. Several dozens of people specialized in botany and obtained M.Sc. degrees. At least three scientists obtained the degree of doctors, and one – a post-doctoral degree ('habilitation').
7. The botany centre of Vilna showed a clear tendency towards practice and served to implement the idea of elevating the level of agriculture and medicine in the eastern fringes of Poland at that time.

7. Closing remarks

After Vilna was incorporated into the Soviet Union as the capital of the Lithuanian Soviet Socialist Republic, at the end of the Second World War, many of the former employees of the University were deported to Poland, which had now new borders. Newly established universities needed their knowledge, experience, and courage. Some of them took part in the organization of botanical, biological, and pharmaceutical departments in the University of Łódź,[125] in the Maria Curie-Skłodowska University in Lublin,[126] Higher School of Agriculture in Szczecin,[127] and in other institutions. Some botanists continued the research that had been started in Vilna.

As a closing remark, it is worth quoting the words of Wanda Grębecka, a historian of botany:

> In the countries where the continuity of work was repeatedly interrupted by changeable geopolitical conditions, the ability to transfer studies and subjects into new

[125] Mowszowicz 1968.
[126] Paszewski 1971, pp. V–VI; Sałata 1995, p. 111.
[127] Köhler 2016a.

> circumstances, as well as starting scientific life anew without interrupting the continuity of studies was a valuable capability, which determined success […].[128]

8. Acknowledgments

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[128] Grębecka 2003, pp. 69–70.

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Some remarks and documents concerning the emigration of Polish mathematicians during the 1930s and early 1940s

Abstract

The history of the sufferings and the emigration of mathematicians under Nazi influence would be very incomplete without considering the perhaps most vibrant and at the same time most victimized European mathematical school of the 1930s, namely the Polish one. Polish mathematical emigration contributed – similarly to German-speaking emigration – considerably to the development of mathematics in the host countries, particularly in the United States.

The paper contributes to the discussion with some archival documents from two specific sources, which have so far found

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relatively little attention among historians of mathematics. These are the files of the Society for the Protection of Science and Learning (SPSL) at the Bodleian Library in Oxford, UK, and the files related to the Asylum Fellowship Plan organized by the Astronomer at Harvard University Harlow Shapley, now in possession of the Harvard University Archives.

Keywords: *emigration of Polish mathematicians*, *occupation by Nazi Germany*, *problems of academic job market*, *Society for the Protection of Science and Learning (SPSL)*, *Shapley's Asylum Fellowship Plan*

Kilka uwag i dokumentów dotyczących emigracji polskich matematyków w latach 30. i 40. XX wieku

Abstrakt

Historia cierpień i emigracji matematyków wywołanych przez nazistów jest bardzo niekompletna, jeśli nie weźmiemy pod uwagę szkoły polskiej, chyba najbardziej tętniącej życiem, a zarazem najbardziej prześladowanej europejskiej szkoły matematycznej lat trzydziestych XX wieku. Polska emigracja matematyczna przyczyniła się – podobnie jak emigracja niemieckojęzyczna – do rozwoju matematyki w krajach przyjmujących, szczególnie w Stanach Zjednoczonych.

Artykuł jest przyczynkiem do dyskusji na temat niektórych dokumentów archiwalnych z dwóch konkretnych źródeł, które jak dotąd stosunkowo mało interesowały historyków matematyki. Są to akta Society for the Protection of Science and Learning (SPSL) w Bodleian Library w Oxfordzie w Wielkiej Brytanii oraz akta związane z Asylum Fellowship Plan organizowanym przez Harlowa Shapleya, astronoma z Harvard University, obecnie w posiadaniu Harvard University Archives.

Słowa kluczowe: *emigracja polskich matematyków*, *okupacja przez hitlerowskie Niemcy*, *problemy akademickiego rynku pracy*, *Society for the Protection of Science and Learning (SPSL)*, *Shapley's Asylum Fellowship Plan*

1. Introduction, problems and sources

In my book *Mathematicians Fleeing from Nazi Germany* of 2009 I investigated the enforced emigration of German-speaking mathematicians from Nazi Germany and Nazi dominated European territory between 1933 and 1945. In my definition all mathematicians who were trained mathematically and/or socialized or assimilated in a German-speaking mathematical environment, including Austria and related territories, and Switzerland were "German-speaking". I did not differentiate between mathematicians born in Germany or elsewhere, and thus I included in my book Hungarian born Johann von Neumann, Polish born Leon Lichtenstein, and others whose mother tongue was not German but who published mostly in German. In a later publication (2012) I investigated the "migration of mathematicians to and from Czechoslovakia, caused by National Socialism." But again, I focused on German-speaking mathematicians, because there were many of them working even after WWI in various universities and schools in Bohemia and Moravia, particularly in Prague and Brno. The same was not the case in Poland, even in those parts which formerly (before WWI) had been under Prussian or Habsburgian rule, although many Polish mathematicians at the time (Tarski, Walfisz, Birnbaum, Rosenblatt, Schauder etc.) had a good command of German and published partly in this language. I have not felt fit until now to include into my research mathematician-refugees either from Czechoslovakia, Poland, France, Italy or any other country in Europe, who taught, published and communicated mostly in non-German languages. I still feel this should be reserved primarily to historians from those countries with better linguistic and cultural competences for that task. Any attempt from my side cannot, by necessity, completely succeed in analyzing the particular circumstances of emigration in the countries of origin, conditions of acculturation in the host countries, and the scientific impact of the refugees. This in fact requires an analysis of correspondences with compatriots and of mathematical publications in languages not familiar to me. For the Polish case we have encouraging recent investigations in this direction by historians such as Ciesielska, Duda, and Maligranda.[1]

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[1] Ciesielska, Maligranda 2014; Maligranda 2011 and Duda 2012. The latter publication comprises many more refugees than those from Nazi oppression, but – maybe for lack of space – it basically refrains from giving historical analysis.

However, systematic studies of the political, economical and scientific reasons for the emigration of Polish mathematicians, the sensitive issue of academic anti-Semitism and of the reception of refugees in the host countries are still missing. The autobiographies of emigrants from Poland such as Mark Kac (1985),[2] and Stanisław Ulam (1976), and by the mathematicians surviving occupation such as Hugo Steinhaus (2015––2016, vol. 1; vol. 2),[3] and Kazimierz Kuratowski (1980), are still important and not fully explored sources.[4] Further sources are biographies of Polish émigré-mathematicians such as of J. Neyman (Reid 1982), A. Tarski (Burdman Feferman, Feferman 2004), and A. Rosenblatt (Ciesielska, Maligranda 2014).

If I now nevertheless and in full recognition of my limits use the occasion of the invitation[5] to speak in Kraków in June 2018 to reflect about the fate of some "non-German-speaking" Polish mathematicians between 1933 and 1945, it is mostly for two reasons.

Firstly, I want to contribute to this discussion with some archival documents from two specific sources, which I found "accidentally" when searching for my main group of refugees, the German-speaking mathematicians. These two sources have so far found relatively little attention among historians of mathematics. I speak about the files of two organizations that supported scholars fleeing from the Nazis after 1933: the files of the *Society for the Protection of Science and Learning* (SPSL, original name Academic Assistance Council) at the Bodleian Library in Oxford, UK, and the files related to the Asylum Fellowship Plan organized by the Astronomer at Harvard University (Cambridge, USA) Harlow

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[2] Kac was actually a Russian-speaking Ukrainian born Jew who was socialized in Poland when, as a result of WWI, his hometown Kremenetz had become Polish Krzemieniec and Lwów (Kac's place of mathematical studies) had become Polish as well. Kac was thus a Polish-speaking mathematician in a similar sense in which Johann von Neumann was a German-speaking one.

[3] Jewish Steinhaus went into hiding after the German occupation of Lwów in 1941.

[4] Kuratowski's book appeared under conditions of communist censorship, which reduces its openness about questions such as anti-Semitism and Polish-Soviet relations. However, the book is particularly valuable for photographic material. As communicated to me by D. Ciesielska there is some evidence that Kuratowski was of Jewish ancestry and hid this from the German occupiers.

[5] My talk was on 4 June 2018 in front of the Polish Academy of Arts and Sciences and had the same title as this article.

Shapley (1885–1972). As to the SPSL I can partly build on previous research by Robin E. Rider (1984) and by my colleague in Kristiansand, Rolf Nossum (2012).

Secondly, during my previous studies I developed an intense feeling that the history of the sufferings and the emigration of mathematicians under Nazi influence is very incomplete without considering perhaps the most vibrant and at the same time the most victimized European mathematical school of the 1930s, that is the Polish one. As a German historian I feel a particular responsibility to consider this question at least to some extent. Restricting my discussion to refugees implies of course that I cannot really give a picture of the sufferings of the Polish mathematicians. After all, refugees fleeing from the Nazis were "privileged" victims, compared to the ones who were killed in Germany or under occupation. The Polish probably suffered most, due to the Nazi policies to annihilate the Polish intelligentsia, not just the Jews among them. During my research I counted 17 German-speaking mathematicians who were murdered or driven to suicide by the Nazis (or in fewer cases by Stalinism), just to mention the well-known F. Hausdorff, R. Remak und A. Tauber among them. But there were many more killed from the relatively smaller Polish community, among them S. Kaczmarz, J. Marcinkiewicz, S. Saks, J. Schauder, M. Wajsberg. I found 35 Polish mathematicians murdered; my Polish colleagues probably know more.[6] In my paper I will focus on the surviving refugees and keep respectful silence in view of the irretrievable loss of life.

Finally, for this introduction I want to stress that Polish mathematical emigration contributed – similarly to German-speaking emigration – considerably to the development of mathematics in the host countries, particularly in the United States. As one example that stands for many, I quote here from the 1989 article on Antoni Zygmund's mathematical school by his American students Coifman and Strichartz:

> It is important to realize the following unique features of this school. When Zygmund came to Chicago, the "trend"

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[6] Steinhaus in his diaries of January 1945 already counted 26 murdered, two among them killed by the Soviets (Kempisty, who committed suicide when jailed by the Russians in Wilno, and Marcinkiewicz). For Marcinkiewicz see Maligranda 2011. See also the list in Kuratowski 1980 (pp. 81/82) which comprises 22 murdered.

> in mathematics was very much influenced by the Bourbaki school and other forces that championed a rather abstract and algebraic approach for all of mathematics. Zygmund's approach toward his mathematics was very concrete. He felt that it was most important to extend the more classical results in Fourier analysis to other settings... He realized that fundamental questions of calculus and analysis were still not well understood. In a sense, he was "bucking the modern trends" (Coifman, Strichartz 1989, p. 347).

2. Remarks and scattered evidence about reasons for the emigration of Polish mathematicians in the 1930s and early 1940s

Emigration of European mathematicians during the 1930s and early 1940s cannot be reduced to forced emigration under direct Nazi pressure in Germany or in German-occupied territories. Not only was there – if to a smaller degree – forced emigration from other dictatorial regimes such as Italian Fascism and Soviet Stalinism. Emigration was also conditioned by fear of coming events such as occupation, by the precarious academic job market in many countries and by political conditions such as anti-Semitism even outside Nazi Germany. As a general tendency, conditions for emigration worsened during the late 1930s not least due the fact that the earlier refugees had filled available positions abroad. To take just one example: Mark Kac, born in 1914, the future prominent American probabilist, felt in 1937 in Lwów that he was disadvantaged compared to German-speaking Jewish mathematicians:

> I was young, not even a Ph.D., with only two or three published notes, while my "competitors" from Hitler's Germany were, in many cases, world-renowned (Kac 1985, p. 41).

The five major centers of mathematical research in Poland between the two World Wars in approximate order of importance were in Lwów,[7] Warsaw, Wilno, Kraków and Poznań. All five towns were Polish at the

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[7] The most detailed historical description of any Polish mathematical center is the one by Duda on Lwów (2014).

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time. Lwów and Wilno were ceded to the Soviet Union after World War II, with surviving members of the Lwów school being relocated to the Western part of Poland, in particular to formerly German Wrocław. All five centers came under German or Soviet occupation during WWII. When occupation began, it was almost too late for emigration. Antoni Zygmund in Wilno (1940 to the US), Jan Łukasiewicz in Warsaw (1944 to Nazi Germany), Leon Chwistek in Lwów (1941 to the Soviet Union) were notable exceptions.

Various major social and political conditions for Polish mathematicians in the 1920s and 1930s come up repeatedly in historical accounts, for instance in the biographies mentioned above, among them a lack of academic jobs, and anti-Semitism as an additional obstacle. Moreover, there was a rather slow international recognition of Polish mathematics in the West, influenced by political and ideological prejudices against Eastern European scholars and partly by the language barrier.

To begin with political and ideological prejudices. Within the American Rockefeller Fellowship program of the 1920s, Polish mathematicians fared in the end quite well. There were 10 Polish fellows out of a total of 130 going to mathematical centers in the West, usually for a year.[8] But there were signs of discrimination against Polish mathematics within the American Rockefeller philanthropy, which had been advised by leading American and German mathematicians. Among other things they did not realize the growth and the importance of the new centers in Lwów and Warsaw, and Rockefeller philanthropy did not consider support for them as institutions.

Even more difficult were Polish-German mathematical relations in particular. There were thematic restrictions in German mathematics in fields such as topology, functional analysis, and stochastics, which were partly the result of traditions, partly reinforced by the political alienation between the two nations after World War I. Anti-Polish resentments supported German partial abstention from functional analysis, and from some parts of research in the foundations of

[8] The lists are in Siegmund-Schultze 2001. On the relation of Polish mathematicians to the Rockefeller philanthropy I gave a separate talk in Kraków on 4 June 2018 before the colloquium of the Institute of Mathematics of the Jagiellonian University. The title was "Rockefeller and the Internationalization of Mathematics between the two World Wars [with some emphasis on Poland]." I cannot go into details here.

mathematics. This – in the end – backfired against the Germans. There was decidedly less reserve against Polish mathematics in Vienna in the 1920s, where a completely different political and philosophical atmosphere encouraged, for example, the collaboration of logical positivists and mathematicians, and supported contacts with Polish scientists such as A. Tarski and W. Sierpiński.[9] The Vienna topologist, Karl Menger, writes in his memoirs:

> In Germany, in the 1920's, Abraham Fraenkel was familiar with Polish set theory but was less versed in Polish logic; the logicians in Göttingen were not yet fully familiar with the results obtained in Warsaw; nor had the relations of the Polish logicians with Heinrich Scholz and his group yet developed. The majority of Germans were intensely hostile to the restored Polish nation because of the loss, in the peace of Versailles, of the territories inhabited by Poles, especially the so-called Polish Corridor which joined Warsaw to the sea while separating Berlin from Königsberg, the city of Kant. Even many German intellectuals had an idiosyncratic aversion to Poles, which the latter, mindful of one hundred and fifty years of oppression by Prussia, reciprocated (Menger 1994, pp. 144–145).

In fields with stronger traditions in Germany, such as number theory, Germans such as Edmund Landau (1877–1938), himself persecuted by the Nazis after 1933, supported Polish colleagues (for instance his former student A. Walfisz, and S. Lubelski). Landau apparently even supported financially their new journal *Acta Arithmetica* (founded in 1935).[10]

This leads to the social and political conditions within Poland in the 1920s and 1930s. There was certainly a lack of job opportunities especially for the kind of mathematicians trained by the very theoretical and abstract Polish schools of set theory, logic, number theory, and functional analysis. Kuratowski says in his autobiography that in 1937 there were

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[9] I mentioned these issues in Siegmund-Schultze 2001, p. 15.
[10] Krätzel, Lamm 2013, p. 47, and below in section 3 on Lubelski.

> in Poland 23 chairs of mathematics and 27 positions for auxiliary staff. That was not enough to employ even half of our research mathematicians (Kuratowski 1980, p. 77).

In another place in his book he admits a certain "one-sidedness of Polish mathematics" and an "almost complete omission of applications" (Kuratowski 1980, p. 75). Kuratowski also mentions that "there were not many adepts in number theory in Poland at the time" (*ibid.*, p. 56). The leading figures of the new Polish number theoretic journal *Acta Arithmetica*, Arnold Walfisz and Salomon Lubelski (about the latter – see below), both lacked material support, partly due to the marginality of their field in Poland. In order to survive, Walfisz had to take a job in the insurance industry,[11] as did the future émigrés Mark Kac (see below) and W. Birnbaum (Woyczyński 2001). Only Walfisz succeeded with emigration (1936 to Tiflis in the Soviet Union), Lubelski perished under the Nazis. About the functional analyst Juliusz Schauder, Mark Kac said in 1985:

> Even for docents[12] of great scientific renown the chances of obtaining a professorship in a reasonable length of time were extremely small. They were nil for docents who were Jewish (the name of the mathematician Juliusz Schauder, an internationally famous Lwów docent who never became a professor, comes to mind) (Kac 1985, p. 29).

Schauder himself wrote to his French friend Jean Leray on 8 July 1936 in German:

> I really do not know what future has in store for me. With my age I should have become professor for long, but in my country [Vaterland] the situation for myself is totally hopeless. […] I would really need one or two years for myself in order to do mathematics (Leray 1980, p. 431, translation from German).

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[11] Krätzel, Lamm 2013, pp. 45–48.

[12] "Passing habilitation and earning the coveted title of «docent» allowed one to lecture without pay and to get onto a list of candidates when a professorial opening materialized because of a retirement or a death" (Kac 1985, p. 28).

Of course, academic anti-Semitism in Poland and other European countries outside Nazi-Germany cannot be compared to state-enforced anti-Semitism as under the Nazis. Kac remarks in this connection:

> Polish anti-Semitism had always been largely religious. Racial overtones, though possibly ever present, became noticeable only after Hitler came to power in Germany (Kac 1985, p. 28).

As we have seen above, Polish mathematicians could not count much on help for their job prospects from German colleagues even before Hitler came to power in 1933.

Anti-Semitism as a state doctrine in Germany created a totally different situation that went far beyond traditional academic anti-Semitism, which existed in other countries as well. In particular, it did not matter to the Nazis if a Jewish scholar had converted to Christianity, they persecuted what they defined as "Non-Aryans." When in early June 1934 Walfisz tried to win the German mathematician Helmut Hasse for the editorial board of *Acta Arithmetica*, Hasse inquired anxiously with his superiors in the ministry whether he was allowed to accept the invitation. In his letter he added the remark:

> [a]s to the factual matter I remark that the three managing editors are Polish mathematicians, two of them (Walfisz and Dickstein) presumably non-Aryans.[13]

This led to an official ban for Hasse to collaborate with Walfisz. At the same time the Germans could not hide their admiration for the recent Polish progress, particularly in functional analysis, where German mathematics was rather under-developed. Two years after the incident with Walfisz the same number theorist Hasse wrote in an internal report about his attendance of the International Congress of mathematicians in Oslo 1936:

> At a social gathering it was stressed, relative to the Polish mathematician S[tefan] Banach (Lemberg), who was in

[13] Siegmund-Schultze 2002, pp. 339/340. In the end, Dickstein did not become editor, however Lubelski did. The latter was also "non-Aryan" in the Nazi understanding. See Krätzel, Lamm 2013, p. 45.

> attendance at the Congress, how admirable it was that Poland had developed such a broad and strong mathematical school of such a particular orientation in so short a time.

The recipient of Hasse's partial report, Walther Lietzmann in Göttingen, the leader of the German delegation to Oslo, added in his comprehensive report to the Nazi authorities:

> We should improve our contacts with Poland which is still largely oriented towards France relative to science. However, certain problems may arise, since many Polish mathematicians seem to be nonAryans.[14]

Banach was no Jew but he was not spared oppression under German occupation.[15] In view of the ruthless extermination policies of the Nazis, which were not only directed against Jews but against the Polish intelligentsia as a whole, and given the pre-history of nationalistic conflicts as described above, the Polish, as a rule, could not rely on help from German mathematicians. A notable exception was the logician Jan Łukasiewicz who came to Germany late in 1944, on the initiative of Heinrich Scholz from Münster. In realistic fear of the approaching Russians, Łukasiewicz stressed his anti-Semitism and his anti-Soviet position in order to be acceptable for the Nazis (Schmidt am Busch, Wehmeier 2005, p. 125).[16]

3. Some new documents from SPSL and the Shapley Committee, concerning the emigration of Polish mathematicians

In this final section I will quote a few documents mostly from the files of the SPSL and the Shapley Committee, which have been introduced

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[14] Both quotes in Siegmund-Schultze 2002, p. 344.

[15] Banach survived in German occupied Lemberg (Lwów) but died soon thereafter in 1945, possibly also due to the previous hardships – see Kałuża 1996, pp. 84–91; Duda 2014, pp. 69–73.

[16] Although it took certainly courage on the part of Scholz to support the Pole Łukasiewicz, he was not in the position to help Polish mathematicians of Jewish ancestry in the same manner. Attempts of some biographers of Scholz seem inappropriate to gloss over the anti-Semitism and anti-slavic conceit of the Nazis, whom Kac rightly describes as the "German barbarians" (Kac 1985, p. 18).

in the first section. The documents concern above all the following mathematicians who either emigrated from Poland (before or during occupation), stayed there under occupation or tried in vain to leave the country: S. Kempisty, W. Kozakiewicz, K. Kuratowski, S. Lubelski, J. Marcinkiewicz, J. Neyman (Reid 1982), J. Rudnicki, A. Tarski (Burdman Feferman, Feferman 2004), and A. Zygmund. Several of these mathematicians lost their lives under occupation. Well-known mathematicians and philosophers from England and the United States such as G.H. Hardy, B. Russell, H. Weyl, J.H.C. Whitehead, and J.H. Woodgers were involved as recipients or authors of several of these documents. I give only very sparse commentary of my own.[17]

Finally, for the purpose of documentation and possible future research I add an anonymous report from the Shapley Papers on a mysterious Polish mathematician "H.Z.", who escaped through Germany and Switzerland to France.

3.1. Records from about 1940 in the Harlow-Shapley Papers at Harvard University on the Polish number theorist Salomon Lubelski

As mentioned above, Salomon Lubelski (1902 – probably 1941) together with Arnold Walfisz was in 1935 the co-founder of the Polish number theoretic journal *Acta Arithmetica*. He never got a full professorship and was finally murdered in the Nazi concentration camp Majdanek, probably in 1941. There is little biographical information in the literature beyond (Anon 1958). Lubelski apparently contacted the Shapley Committee shortly before or during occupation to inquire about chances for emigration. The Shapley Committee has one page of biographical information on record (possibly collected by Hermann Weyl) and an additional page with an undated typewritten letter with H. Weyl as the author. The prominent German mathematician and mathematical physicist Hermann Weyl (1885–1955) had been a refugee himself, arriving at the Institute for Advanced Study in Princeton in 1933. He was

[17] Some remarks in the letters, for instance about academic anti-Semitism in Poland and about the relation between Poland and Lithuania, or the refugee policies in Switzerland certainly need a detailed and nuanced investigation.

instrumental in helping other mathematicians to reach the United States. He was also well-educated and active in number theory, although it was not his primary research area.

a) Page with biographical information on Lubelski

(Harvard University Archives, Harlow Shapley Refugee Files HUG 4773.10, box 6B, folder L):

S. Lubelski: Address – Leszno 77, Warsaw, Poland
Born Feb. 9, 1902, Warsaw – Polish, Jewish, unmarried, Ph.D. 1927
Positions held ?
Editor of the Acta Arithmetica
References: A.A. Albert, E. T. Bell, Prof. Claude Chevalley (1938–1939, Fine Hall, Princeton, N.J.)
Publications – 23 papers, mostly on the theory of numbers, of substantial content. List can be supplied if required.

b) Hermann Weyl's letter (undated, signature typewritten)

(Harvard University Archives, Harlow Shapley Refugee Files HUG 4773.10, box 6B, folder L):

Re Lubelski:
Editor of the Acta Arithmetica. 23 papers, mostly on theory of numbers, of substantial content. Obviously long in opposition to the other Polish mathematicians, as evidenced by the following characteristic quotations from his letters:

"Der selige und liebenswürdige Professor Landau hat – um eine mathematische Arbeitsstelle unter anderem auch für mich zu schaffen – aktiv geholfen, die Zeitschrift Acta Arithmetica in's Leben zu rufen."

"Die Zeitschrift Acta Arithm. ist aber ein Dorn im Auge der hiesigen Mathematiker, die geglaubt haben, mich durch Aushungern der Mathematik entreissen zu können."

["The dear departed, very kind Professor Landau has – in order to create a mathematical job also for me – actively supported the foundation of the journal Acta Arithmetica [...] The journal Acta Arithm. is, however, a thorn in the side of the mathematicians here, who tried snatching me from mathematics by starvation."]

Not known to me personally, nor is his work known to me in detail. He seems to overestimate his own importance, but he is certainly a mathematician of high standing.
H. Weyl

3.2. Records in the files of SPSL from 1937/38 concerning a possible position abroad for the logician Alfred Tarski

Alfred Tarski (1901–1983) was one of the leading logicians of the 20th century. He happened to be on a lecture tour to the United States when Poland was invaded in 1939. He stayed there for the rest of his career but did not succeed in securing immigration for his family before the end of the War. There is a full-length biography on Tarski (Burdman Feferman, Feferman 2004) that strongly emphasizes academic anti-Semitism in Poland in the late 1930s as a reason for Tarski's lack of academic progress then, maybe downplaying a bit the general calamity of the academic job market. A letter in favor of Tarski written in 1937 to SPSL by the English biologist and philosopher of science Joseph Henry Woodger (1894–1981) emphasizes the same point. A handwritten letter to SPSL from 1938 by the famous logician and philosopher Bertrand Russell (1872–1970) has already been published in facsimile in Nossum 2012. The letter compares Tarski favorably with the German logician Rudolf Carnap (1891–1970) who – not being Jewish but a leftist – had already reached American soil at the time.

Joseph Henry Woodger to SPSL (Adams) on 22 November 1937 (typewritten, signed):
(Bodleian Library Oxford, SPSL 285, fol. 330/331)

Dear Mr. Adams,

In answer to your letter of 19th of November, it would be superfluous, if not impertinent, for me to say anything about Dr Tarski's scientific qualifications in addition to the testimonials I recently sent to you. His is one of the five or six leading names in modern logic and certain branches of mathematics. He has created the subject known as "Semantic" almost entirely alone. He has a long list of publications of outstanding importance already to his credit. Let me try to give you some notion of the wide significance of Dr Tarski's work. One of the most urgent needs of the present day is to make people language-conscious. This need is particularly urgent in relation to morals, politics and economics, and in every sphere of life which involves value-judgments. Because we live in an age in which all these topics are in a ferment and a great deal depends on the way in which beliefs regarding them are formulated. If people were more language-conscious – more aware of the properties of language as a tool or instrument – they would be far less at the mercy of propaganda than they are at present. They would be able to use the instrument of language more critically and intelligently, and would be on their guard against attempts by others to use it to enslave them. We are constantly being reminded of the truth of the old saying that "The pen is mightier than the sword" because it is only by the systematic misuse of the pen that people can be persuaded to use the sword. Now in my opinion the chief significance of Dr Tarski's work is to be found in the fact that he is creating the means by which it will be possible to make people – through its ultimate influence on education – language conscious. It is a great misfortune that at present we have no one of Dr Tarski's calibre working on his subject in England.

The tragedy of Dr Tarski's position lies in the fact that, owing to the antisemitism in Poland, and in spite of his genius, which is freely acknowledged among his Polish colleagues, he has no prospects of becoming a professor in a Polish university. His small remuneration as docent is quite insufficient to support a wife and child. He is therefore compelled to devote a large part of his time to school teaching. This, coupled with the constant uncertainty about the future which the tendency for antisemitism to increase brings with it, places him in an atmosphere in which it is extremely difficult for him to exercise his great talent. In recent years his circumstances have not enabled him to achieve the continuity which his work requires.

His case is thus somewhat similar to that of Dr. Karl Popper whom you helped. But Popper's gifts are not so great as Tarski's; the antisemitism in his case was perhaps less intense and less organized; and he had no children. Tarski's problems would be solved and he would be freed to continue his work if a suitable appointment could be found for him outside Poland. He could occupy any teaching appointment in either logic or mathematics. In this connexion I ought to mention that although Dr Tarski speaks French and German fluently and correctly he has not yet mastered spoken English. But he reads this language easily and is at present taking lessons in the spoken language.

Mr Bertrand Russell, when I spoke to him recently, told me that he would be pleased to do anything he could to help to obtain an appointment for Dr Tarski.

Yours sincerely,
J. H. Woodger

Bertrand Russell to SPSL 10 January 1938 (handwritten)[18]
(Bodleian Library Oxford, SPSL 285, fol. 333/333v)

Dear Sir

I have read a good deal of Tarski's work, and have met him in the company of other logicians. I have no doubt that he is the ablest man of his generation in what may be called "scientific philosophy" (by which I do not mean the philosophy of science). There is one man who has perhaps a wider reputation (Carnap), but I do not consider him so original, so clear, or so well-balanced.

I am not able to judge as to Tarski's prospects of getting a permanent post in England, but I should certainly do everything in my power to enable him to get one. I think it of great importance that he should be enabled to do his work.

Yours truly Bertrand Russell

3.3. SPSL-documents from 1940 concerning the situation under occupation of Polish mathematicians such as Kazimierz Kuratowski, Antoni Zygmund, Stefan Kempisty, Józef Marcinkiewicz, Juliusz Rudnicki and Wacław Kozakiewicz

Kazimierz Kuratowski (1896–1980), who would become president of the Polish Mathematical Society during the first eight years after WWII, described his situation under occupation, for instance his participation in the "underground universities" in (Kuratowski 1980).

Wacław Kozakiewicz (1911–1959) was born in Złotniki near Kielce. He was a graduate of Warsaw University. From 1938 on a scholarship

[18] This letter was published before in facsimile in Nossum 2012, p. 97.

in France, he was caught in a difficult situation when the war broke out. From 1944 in Canada, he became a professor at the French university in Montreal in 1959 (Duda 2012, p. 100).

The early Polish immigrant and mathematical statistician Jerzy Neyman (1894–1981) tried in various ways to help compatriots to positions abroad. There exists a full-length biography of Neyman (Reid 1982).

According to Nossum (2012, p. 98), Juliusz Rudnicki (1881–1948) survived in Wilno during the war, working as a secondary school teacher. Stefan Kempisty (1892–1940) committed suicide in prison in 1940 during Soviet occupation which began in June 1940 and ended in 1941 when the Germans invaded Lithuania (Józwik, Maligranda, Terepeta 2017). While Zygmund reached the US in 1940 and founded an influential analytical school there (see the quote by his students given above), his student Józef Marcinkiewicz (1910–1940) was killed as a Polish military officer in Soviet captivity (Maligranda 2011).

A. Zygmund (Wilno) to G.H. Hardy (England) 3 November 1939 (typewritten, copy):
(Bodleian Library Oxford, SPSL 286, fol. 372)

Dear Hardy,

When some time ago I wrote to you about Salz [sic, possibly Stanisław Saks], I did not expect that I should soon write to you a similar letter on behalf of myself.

Roughly speaking the situation is as follows. Wilno is at present at least in Lithuania. The country is very small and already possesses one university in Kovno. The present university in Wilno, that is the staff, will be liquidated very soon, and the Lithuanian University in Kovno will be transferred to Wilno. As a matter of fact, I doubt whether we shall obtain anything from the Lithuanian Government. In these circumstances, when all the high schools [meaning universities or higher schools] in the German part of Poland are closed and the Lwow [sic] University (now in U.S.S.R.) will probably be made Ukrainian this means that unfortunately I have lost any possibility to find work here. Would you help me a little?

I know that at present it is a hopeless task to look for a place in England. Besides, I owe you so much, both personally and mathematically (when you helped me to obtain the Rockefeller Fellowship) that I do not want your relation to me to be burdened too much by my troubles. Of course if you heard of any possibility in any country, I should be extremely obliged (I don't mean, naturally of any stabilised position, still less of any professorship. I should be glad to have any junior position which would permit me to exist and to work, until my return to Poland would be possible). I must however confess that I am rather pessimistic about such a possibility. A dislike of foreigners, especially of people coming from the East of Europe, is so widespread, that only in exceptional cases I might hope for anything.

There is however, another possibility – The Princeton Institute for Advanced Study. A few years ago, during his stay in Wayans [sic], Lefschetz told me that if I wanted to get there, let us say for a year, he would support me (If I understood him well this is a sort of stipend for mathematical workers). Being afraid of a possible refusal I did not apply to the Institute. The situation is different now, and I shall have to try this way, whatever the result. During a year's time, much may happen that could enable me to return to more normal conditions. Simultaneously with this letter I shall send a letter to Lefschetz and perhaps to Bochner. I feel however that if my application is not backed by people of high mathematical standing, I may apply in vain. And it is here you could help me, provided the thing could at all be realized. If for any reason it would not be possible for you to support me, I should appreciate it if you would let me know.

A. Zygmund

G.H. Hardy to Mr. Thomsen (SPSL), 19 November [1939], Trinity College Cambridge
(handwritten, SPSL 286, fol. 374)

Dear Mr. Thomsen,

I enclose a letter which I have received from Prof. A. Zygmund of Wilno Univ. It is a new type of case, & I hardly suppose the SPSL is likely to be able to do anything (if the general "refugee" position is much the same as it was before the war). But Zygmund is a thoroughly first rate man.

The irony of it is that Wilno is (I believe) a mainly Lithuanian city, which Poland just collared by violence against the terms of the peace settlement. And I suppose that giving it back to Poland is one of our 'war aims'!

I enclose a cheque for £ 25

I am yours sincerely

GH Hardy

May I have Z's letter back? I have written to Princeton about him.

J. Neyman (Berkeley) to Esther Simpson (SPSL) 3 January 1940 (typewritten, copy):
(Bodleian Library Oxford, SPSL 281, fol. 254/254)

Dear Madam,

Many thanks for yours of the 15th December. It is most encouraging to hear that the activities of your society now include Polish scholars. I appreciate the difficulty in getting information about those who are displaced. Consequently, I take the liberty of giving you such information that I happen to have.

1. The information obtained directly from Professor A. Zygmund of the University Wilno.

The Polish University of Wilno was definitely closed by the Lithuanian authorities on December 15th and all the Polish staff of the same dismissed. As far as I can judge, the most outstanding scholars in that University were Professor A. Zygmund, Docent Józef Marcinkiewicz, and Professor S. Kempisty, all three mathematicians. However, this judgment may easily be biased because of my lack of information on sciences other than mathematics.

I know that Professor Zygmund is still in Wilno and letters to him could be addressed: Mathematical Institute, University of Vilnius, Vilnius, Lithuania. I presume that Professor Kempisty is in Wilno also. I have no doubt that Professor Zygmund will be able to supply you with a complete list of the former staff at the University of Wilno now dismissed.

According to unconfirmed reports, Doc. Marcinkiewicz, who was mobilized and joined the Polish Army, is now interned in Hungary. Maybe you will at least be able to ascertain his address.

2. A couple of months ago I received a letter from Dr. Waclaw [sic for Wacław] Kozakiewicz, an extremely able mathematician of the younger generation, working on the theory of probability.

At the time when the war broke out he was on a Polish fellowship in France. There was some doubt whether his health would permit him to join the army and, having no means, he was in great distress. He gave his address as 19 rue Verdi, Chez Madame Detloff, Nice (A.M.), France, but my letters to that address and two telegrams, one of which was with a prepaid reply, are still without answer.

3. Professor Zygmund writes that, according to reports from persons who recently managed to escape from Warsaw, by crossing illegally into Lithuania, the University and the Engineering School of Warsaw are closed and even [sic] the German authorities have [even] shipped the equipment of several laboratories to Germany. So, for example, all the equipment of the Physical Institute was entirely removed from Warsaw. Presumably the scholars in Warsaw, Krakow [sic], and Poznan [sic] are in greatest need of help, but it is difficult to see how this help could be extended to them.

Thanking you again,
Sincerely yours J. Neymann [sic, typewritten]

Letter by Stefan Kempisty (handwriting) and Juliusz Rudnicki to SPSL on 27 January 1940 about their dismissal in Wilno
(Bodleian Library Oxford, SPSL 281, fol. 253, Handwritten by Kempisty)

Wilno, 27.1.1940

Dear Sirs,

The Polish University in Wilno is now liquidated and its staff dismissed. If you know of any vacant job suitable for us, we should be greatly obliged if you let we [sic] know of it.

Stefan Kempisty
Julius Rudnicki [sic, signature added]
Prof. of the former Polish University of Wilno

Nancy Searl (SPSL) to the English topologist J.H.C. Whitehead, 30 January, 1940 on the situation of Polish refugees (typewritten copy, excerpt):
(Bodleian Library Oxford, SPSL 281, fol. 263)

We have no particulars as yet about Dr. Kuratowski, and we should be glad to have any further particulars about him which you may obtain. The Polish refugees are so scattered and communication with them is so difficult, that we are glad to have any information that may come by indirect means.

Antoni Zygmund (1900–1992) in a questionnaire for SPSL from 1940 on his dismissal in Wilno (excerpts)
(Bodleian Library Oxford, SPSL 286, fol. 364–366)

Rank: Ordinary Professor of the university of Wilno
Grounds of dismissal: closing of the present Polish university in Wilno
Date of Notification: 20 November 1939
Religion, catholic: yes
Countries you prefer to go: English speaking countries
Countries you are not willing to go to: U.S.S.R.
If not state reasons: Experiences with the Russian authorities during the Soviet occupation of Wilno

3.4. Report in the files of the Harlow-Shapley Papers at Harvard University after 1943 about the emigration of a yet unidentified Polish mathematician H.Z. through Germany, Switzerland to France

(Harvard University Archives, Harlow Shapley Refugee Files HUG 4773.10, box 6E, folder: reports, anonymous, typewritten)

A few days ago, Mr. H. Z. left Switzerland to go to Paris. He is 28 years old of Polish nationality and a very talented mathematician. Already in 1939, when Hitler invaded Poland, Mr. Z. had to flee from the Gestapo. For some months he lived hidden always changing his name and flying [sic] from town to town, from village to village. Three times he was almost caught and it seems to him like a miracle that he could escape. But one of his comrades was not so lucky. He was killed by the Gestapo and that was perhaps the last chance for Mr. Z. He took the identification of his dead friend and applied for a job in a German aircraft-factory. But already a few weeks later his false name was discovered and he was to be arrested. Once more he could escape in the very last moment and this time his only chance was to reach central Germany. On his way he bought some false identification papers and reported to the authorities in Munich. From there he was sent to a factory near the Swiss frontier, where he had to work as a meat-packer. After a few days he made the acquaintance of some Polish war prisoners who worked in the same village. They decided to take a chance and to try to flee to Switzerland. When they started, they were 12, but only 3 of them managed to escape. 5 were shot by the Gestapo and the rest of them wounded and captured. That is how Mr. Z. came to Switzerland. But here he dared not report to the authorities because they would have delivered him at once to the Germans. So he lived hidden for more than one year and only in 1943, when also Swiss authorities began to believe in an Allied victory, did Mr. Z. announce himself at a police station. He hoped that it would now be possible for him to finish his studies and he applied to our Committee. We declared ourselves willing to grant him the necessary funds, but he was interned in a labor-camp. That was of course another aspect of Switzerland, which he had once believed to be a free country. During his internment in the labor-camp, Mr. Z. wrote a paper on a scientific subject and sent it to a well-known professor of Mathematics at the Geneva University. After having read this paper, the professor did his utmost to liberate our talented young friend from labor service. But Swiss authorities are tough. No wonder that Mr. Z. took the first chance to leave Switzerland and went to France. There he will not be interned and the finishing of his studies is only a financial question. With the help of our organization we will give him a chance to finish his studies and build up a new life earning existence for himself, his wife and his baby.

4. Acknowledgments

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Science in Central and Eastern Europe

Nauka w Europie Środkowej i Wschodniej

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Local knowledge and amateur participation. Shevchenko Scientific Society, 1892–1914

Abstract

This article discusses the possibilities which amateur participation offered to the young Shevchenko Scientific Society – limited to the description of the activities of this Society in the years 1892–1914.

The Society intended to develop rapidly into an academy of sciences in the Ukrainian language, but lacked the necessary resources. The existing network of Ukrainian associations in Eastern Galicia, which contributed to the development of scientific exchange, was helpful in achieving that status.

Before looking into the details of research agendas, the possibilities to use concepts of citizen science are measured for the context of the late 19th and the early 20th century.

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The relation between 'scientists' and 'amateurs' is problematized on the basis of biographical examples of engaged scientists and activists, especially Volodymyr Hnatiuk from the Ethnographic Commission and Stanislav Dnistrians'kyĭ from the Statistical Commission.

In order to understand the specific relations of Hnatiuk to his network of folklore collectors, their projects, aims and possibilities, Hnatiuk's research is contrasted with the statistical surveys initiated by Dnistrians'kyĭ.

Based on their archival documentation and published sources, these research projects are analyzed together with the different circumstances between the poles of "national science" and "local knowledge".

The article suggests that Ukrainian amateur researchers contributed intensely to the nation- and region-building in the multinational Empire.

Keywords: *Shevchenko Scientific Society (Naukove tovarystvo imeni Shevchenka), citizen science, scientific community building, networks, Volodymyr Hnatiuk*, *Stanislav Dnistrians'kyĭ*, *folklore*, *Habsburg census*, *Ukrainian science, 1892–1914*

Lokalna wiedza i uczestnictwo amatorów. Towarzystwo Naukowe im. Szewczenki w latach 1892–1914

Abstrakt

W artykule omówiono, jakie możliwości oferowały amatorskie badania młodemu Towarzystwu Naukowemu im. Szewczenki działającemu w Galicji Wschodniej; ograniczono się do opisu działalności tego Towarzystwa w latach 1892–1914.

Towarzystwo zamierzało się szybko rozwinąć w ukraińską akademię nauk, ale brakowało mu do tego niezbędnych zasobów. Pomocą służyła istniejąca już sieć ukraińskich stowarzyszeń, która przyczyniła się do rozwoju wymiany naukowej.

Przed rozpatrzeniem szczegółów programów badawczych przeanalizowano możliwości wykorzystania koncepcji nauki obywatelskiej w kontekście końca XIX i początku XX wieku. Przedstawiono relację między „naukowcami" a „amatorami" na podstawie przykładów z biografii zaangażowanych naukowców i działaczy, zwłaszcza Wołodymyra Hnatiuka działającego w Komisji Etnograficznej i Stanisława Dniestrianskiego działającego w Komisji Statystycznej.

Aby zrozumieć specyficzne relacje łączące Hnatiuka z jego siecią kolekcjonerów folkloru, ich projektami, celami oraz możliwościami, badania Hnatiuka zostały porównane z badaniami statystycznymi zainicjowanymi przez Stanisława Dniestrianskiego.

W oparciu o dokumentację archiwalną i opublikowane źródła przeanalizowano niektóre projekty badawcze z perspektywy z jednej strony „nauki narodowej", a z drugiej „wiedzy lokalnej".

Artykuł sugeruje, że ukraińscy badacze amatorzy intensywnie przyczynili się do budowania świadomości narodowej i regionalnej w wielonarodowym imperium.

Słowa kluczowe: *Towarzystwo Naukowe im. Szewczenki*, *nauka obywatelska*, *budowanie społeczności naukowej*, *sieci badawcze*, *Wołodymyr Hnatiuk*, *Stanisław Dniestrianski*, *folklor*, *spis habsburski*, *nauka ukraińska*, *1892–1914*

1. Introduction

Shevchenko Scientific Society (Naukove tovarystvo imeni Shevchenka, further on NTSh), operating in East-Galician Lemberg (Pol.: Lwów, Ukr.: L'viv), pursued the goal of a rapid development into an official academy of science since its reform in 1892. While the previous literary organization contributed little to its formulated goal of supporting Ukrainian science, the young and energic historian Mykhaĭlo Hrushevs'kyĭ (1866–1934) pushed the society towards its own research agendas.[1] Scientific work was to be organized in sections and subordinated commissions, members – selected through criteria of scientific excellence, the collection of folklore texts and ethnographic objects for the library and a museum – which was yet to be built – and new periodicals

[1] Hrushevs'kyĭ came to Lemberg in 1894, became an editor of NTSh's journal in 1895 and its chairman in 1897. This paper cannot take on the actual reformation under Oleksandr Barvinskyĭ and Hrushevs'kyĭ's various initiatives to transform NTSh. For a detailed account of NTSh's academization and the development of specialized journals, cf. Zaĭtseva 2006, pp. 40–180.

were to contribute to the mobilization of Ukrainian academics. This reform therefore included transformation from an elitist to a more inclusive concept in terms of accepting new members, as the former restrictive rules were considered an obstacle for productive work.[2] An explosion in the numbers of contributing scholars did not appear all of a sudden, as it became visible when the first journals started off.[3] Due to low funding during the 1890s and an increasing number of time- and cost-absorbing projects, leading members sought to delegate tasks to unpaid laypersons as well as profit from their knowledge. In order to discuss these inclusive transformations, the latest concepts of citizen science provide instructive suggestions.

Citizen science as a formative trend of the 21st century knowledge production leads to new questions about the relation between the science and the public as well as the formation of scientific communities. Unpaid collaborators provide scientists with new possibilities, in both contemporary and historical practices, even though the scale of participants varies and communication as well as data processing work differently. Furthermore, the educational level narrowed the possible participation down to certain groups in Eastern Galicia. Latest discussions are drawing the attention closer to the intersection of science and citizenship,[4] emphasizing the democratic potential of participation. This also concerns educational purposes, envisioning a type of schooling where "citizens actively collaborate in relation to accounting for future generations".[5] For 19th and early 20th century histories of science, these ideas can serve as a heuristic stimulus,[6] which helps to understand the formation of scholarly networks, the spread of scientific and non--scientific knowledge and motivations of groups to interact. There is already evidence for links between layperson participation in science and citizenship during the 19th century.[7] This perspective has proven fruitful e.g. regarding collective actions against possible natural catastrophes. In Deborah Coen's case of earthquake observers in the Habsburg monar-

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[2] Barvins'kyĭ 1892; Kupchyns'kyĭ 2013.
[3] Zaĭtseva 2006, pp. 133–136.
[4] Leach, Scoons, Wynne (eds.) 2005.
[5] Mueller, Tippins, Bryan 2012, p. 10.
[6] Shuttleworth 2017, p. 48.
[7] Vetter 2012, p. 136.

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chy, she emphasized a multinational network managed by the imperial Earthquake Commission in the service of the state.[8] NTSh, on the other hand, explicitly claimed to work for the greater good of the nation.[9] Even though NTSh was not in direct opposition to the state, but profited tremendously from the relative freedom and financial support,[10] it is difficult to speak of citizen science in a narrow, state-related sense. Indeed, the Habsburg Empire defined all of its subjects as citizens (*Staatsangehörige*),[11] but this is not the dominant affiliation in the occurring network, defined through and working on language, culture and nation. While this paper will focus on Eastern Galicia, NTSh was especially designed to transcend state borders and initiate a dialogue between Ukrainian intellectual communities. Consequently, amateurs and experts from the Russian Empire contributed to the research as well.[12] This point does not advocate for mutual exclusivity of the underlying identity concepts,[13] but for the integrative power of 'national science', showing interest in local affairs and providing their affiliates with access to a scholarly community. After all, even in a not directly state-related context, amateur participation was a relevant tool to build, shape and sustain a community.[14] Hence, this article calls for a broad understanding of citizen science, which applies to the situation of multinational empires as well as national states and separate communities defined through language or other characteristics.

The peculiarities of the non-dominant Ukrainian nation in Galicia and its scientific community also contain problems for the understanding of the term '(professional) scientist'. McCray suggests to

> use the terms 'professional scientist' and 'amateur scientist' with the recognition that the boundaries between and

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[8] Coen 2013, pp. 141–162. For Maoist China, Fa-ti Fan has proposed a concept of citizen science shaped by "ideology, institutions, and functions of a state", that "reflected the intimate relationship between science and the macropolitics of modern state and society" (Fan 2012, p. 150). While (national) ideology was an important point for NTSh-leadership, it did not necessarily have to concern the participants.

[9] Franko 1906; Kupchyns'kyĭ 2013.

[10] Rohde 2019.

[11] For a discussion of this term cf. Gammerl 2010, pp. 73–76.

[12] Cf. Surman [Forthcoming].

[13] Judson 2006; Zahra 2010; Hrytsak 2008–2009.

[14] Coen 2013, especially pp. 69–101.

> the identities of these groups were indistinct and that they sometimes overlapped.[15]

This point on astronomy in the US during the 1950s has to be even more viable for NTSh during the given period, as 'getting paid' for scientific activity does not seem to be a suitable category for their members. The majority was not able to make a living predominantly by pursuing science, including many of the most distinguished scholars. They worked as teachers, lawyers or parliamentarians, a few also as clerics. The legal scholar Volodymyr Okhrymovych (1870–1931) served as head of Dnister insurance company, Ivan Franko's (1856–1916) main income was generated through literary and journalistic work. However, they did publish in their respective field of interest in multiple languages with international acknowledgement, as well as held doctorates. Since the number of chairs and other positions in the Ukrainian language at universities was rather limited, in the case of Franko also because of his former political activities, professional scientists in the narrow sense were rather an exception than the norm.[16] NTSh provided several paid positions, such as a librarian, a secretary or a custodian of the museum, but these earnings were not enough to make a living.[17] This specific situation of a scholarship in a subaltern community can help to question the binary conceptions of 'scientist' and 'amateur', as I will further argue based on the source material.

In order to discuss the relations of NTSh and amateurs in the given period, I will raise the following questions: Who were these amateurs and what benefits for the NTSh could be achieved through long-term relationships? How could amateurs contribute to scholarly nation- and

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[15] McCray 2006, p. 637; Coen (2013) also argues for her case that "[t]he line between expert and amateur was remarkably fluid in nineteenth-century seismology" (p. 9).

[16] For a detailed account on jobs and education of NTSh members 1910, cf. *Khronïka Naukovoho tovarystva imeny Shevchenka* (further: *Khronika*) Nr. 45, 1911, pp. 16–30.

[17] Hnatiuk had several occupations within the NTSh, by which he could make a living, but he often complained about his difficult financial situation. This perspective does, of course, marginalize internationalist scientists as Ivan Puluĭ or Ivan Horbachevs'kyĭ, who taught in German and Czech in Prague. Those are, however, notable exceptions.

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region-building?[18] The first part of this paper investigates the problematic classification of amateurs in the given context, the renegotiations of this status in the long-term and the participation as a tool to get involved with the scholarly community. I shall illustrate these points using the scholarly biography of Volodymyr Hnatiuk (1871–1926), who began to work on folklore already as a pupil and became a respected authority in the field later on. As secretary of NTSh's ethnographic commission, he sustained a network of collectors of ethnographica and folklore, classified, edited and commented them for publication. In the respective case studies, I argue that local amateur researchers were especially relevant for studies in national borderlands. As shown by Patrice Dabrowski, the 'discovery' of the Carpathians as Galician borderlands was crucial for several identity building processes.[19] Especially Polish and Ukrainian national movements contributed to the transformation of this landscape into a contested frontier, even after the collapse of the Habsburg Empire.[20] The presented examples feature examples of the Hutsuls and the Boykos. Nowadays, both of these are identified as Ukrainian ethnic sub-groups. The Hutsuls are a distinct group of highlanders, the Hutsul region or *hutsul'shchyna* is located in the south-eastern part of the Carpathians. Of all Galician ethnic minorities, the Hutsuls were the most intensely studied and well-known also outside of Galicia, even as literary figures.[21] Boykos, located in the Boyko region (*boikivshchyna*) in the middle Carpathians, were much less studied, until NTSh conducted an intense research expedition in 1904.[22] Moreover, the territories outside of Galicia were an important point of reference. Transcarpathia, a term coined during the interwar period, was often referred to as "Hungarian Rus" in contemporary Ukrainian discourse, as these territories inhabited by Ruthenians were located in the Hungarian part of the Empire. The politics of Magyarization or (from the Ukrainian

[18] For the nexus of the respective disciplines and negotiation of identities cf. Baycroft, Hopkin (eds.) 2012; Göderle 2016; Stauter-Halsted 2001, pp. 97–114.

[19] Dabrowski 2005.

[20] Dabrowski 2008; 2013.

[21] Makarska 2010.

[22] Cf. below, section 2.3. A comparable example under different circumstances is the politicization of Alpine regions and the corresponding support for local research in Switzerland. Coen 2013, p. 78.

perspective) denationalization fostered the wish to scientifically prove the ethnographic and linguistic belonging to the Ukrainian nation.[23]

The ethnographic research, usually understood as a highly successful enterprise, will then be contrasted with a largely forgotten crowd-sourcing project inspired by the Cisleithanian census. The census and its highly disputed category *Umgangssprache* (language of daily use), used since 1880, provoked intense debates; not only in the well-researched Czech case,[24] but also in the Ukrainian political and scholarly community. Most likely inspired by Bohemian private counting initiatives in 1900/1901,[25] the statistical commission of NTSh initiated a similar project, combined with more complicated questions on interethnic relations and local politicization. This should allow a trustworthy revision of language and (implicitly understood) national statistics. While approximately 210 volunteers completed 290 questionnaires, there is not a single publication based directly on that material.

2. "Long winter evenings" – *liubyteli* and experts

> I obtained the affection for folk poetry at home. My maternal grandfather […] knew a tremendous mass of folk stories and gladly retold them to me, when I was around him. His wife Mariia, my grandmother, knew a large amount of songs. […] Furthermore, the doors of our hut were almost never locked for the different people, which were sitting for a long time with us, like in a village casino, especially on Sundays, holidays and long winter evenings, and entertained with different stories, which I heard and learned with pleasure […].[26]

Volodymyr Hnatiuk used this childhood memory to explain his admiration for popular culture, which he did not associate with work, but

[23] Tomashivs'kyĭ 1903.

[24] For an overview of the matter and the most relevant literature, cf. Hirschhausen 2012.

[25] Czech activities in this matter were obviously received: *Svoboda* XIV, Nr. 37, 15th September 1910, p. 2; Nr. 40, 6th October 1910, p. 3.

[26] Hnatiuk 1916b, p. III.

with family, sociability and conviviality. Thereby, he referred to emotional attachments to his field of research, varying drastically from the ideal of observers in Western Europe, who are prototypically "associated with a self-effacing, sober, emotionally disengaged expert".[27] He devoted a considerable part of his life to collecting and editing folklore for publications, evolving from a child enjoying learning local tales to an internationally recognized scholar in the field of folkloristics. He became integrated in NTSh still during his studies, also receiving financial grants for his individual research. As secretary of the ethnographic commission of the NTSh and a co-editor of *Etnohrafichnyĭ zbirnyk* (Ethnographic Collection; further on *EZ*), he was responsible for expanding and maintaining a loose network of volunteering supporters. His imagination of these collectors explicitly reflected his feelings towards popular culture, as he addressed his first newspaper-request for transmittals to the "*liubyteliiv* of our folk literature".[28] The Ukrainian term *liubytel'* denotes 'fancier' or 'enthusiast' as well as 'non-professional' in a positive connotation (in contrast to the more neutral 'amator', amateur).[29] While the call appears to accentuate the former meaning, collecting folklore is presented both as a contribution to science and as a pleasant pastime during vacations on the countryside.[30] Here, the call emphasizes the issue of locality, which seems essential, as some collectors worked almost exclusively in their hometowns.[31] In this consideration, the emotionality regarding the subject of research is shared with the laypersons. I will come back to this point when taking a closer look at the question of how *liubyteli* were imagined.

During the early years of academization NTSh still lacked qualified contributors to its journals. The Ukrainian *intelihentsia*[32] was small –

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[27] Coen 2013, p. 86. As some colleagues emphasize, emotionality seems to be a problem in contemporary Ukrainian scholarship (Petrenko 2016, p. 636); it would be a promising field for further investigations to trace this aspect historically.

[28] Hnatiuk 1898.

[29] N. N.2 1973, p. 562.

[30] Hnatiuk 1898.

[31] Cf. the case of Ivan Voloshyns'kyĭ below, in section 3.2.

[32] Vernacular Ukrainian term for the intelligentsia, usually meaning persons with higher education, who were affiliated with the national movement and especially its societies. However, the denoted group may vary in respective contexts. NTSh, for example, considers the 'village intelihentsia' as a distinct group. Cf. below, section 2.2.

publishing in the organs of NTSh required linguistic and ideological compatibility. For example, (former) socialist scholars such as Ivan Franko and Mykhaĭlo Drahomanov (1841–1895) were not asked to contribute to the first issue of the *Zapysky Naukovoho tovarystva imeny Shevchenka* (further: *Zapysky*), the central periodical of the society.[33] Mykhaĭlo Hrushevs'kyĭ was appointed professor of the University of Lemberg in 1894. He took over the editorship of the *Zapysky* and functioned as president of NTSh from 1897 to 1913. To fill the increasing number of periodicals under his auspices, he increasingly turned to retired or young academics, as the number of contributors willing to publish ideologically-fitting papers in Ukrainian did not meet his expectations. For example, his students Stepan Tomashivs'kyĭ (1875–1930) and Stepan Rudnyts'kyĭ (1877–1937) published literature reviews they wrote in class.[34] While Franko became a steady contributor during the second half of the 1890s and started editing the major journals in cooperation with Hrushevs'kyĭ and Hnatiuk, the constantly increasing number of journals called not only for promotion of young researchers, but also for the inclusion of laypersons providing expertise on certain topics. The interest of NTSh in regional political and ethnographic affairs inspired the editors to request that kind of contributions.

A prominent way to distinct between amateur and professional scientist is graduation from related courses of study at a university, which is responsible for introducing standards. Folkloristics, Ethnology and Anthropology were not yet institutionalized in the Habsburg monarchy, as it was still being discussed what directions should be taken by these young disciplines.[35] Consequently, scientific societies such as Towarzystwo Ludoznawcze (Ethnographic Society) in Lemberg, Verein für Österreichische Volkskunde (Society for Ethnography) in Vienna and NTSh were crucial for setting, maintaining and popularizing

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[33] Zaĭtseva 2006, p. 131.

[34] *Ibid.*, p. 136.

[35] It was not until 1910 that ethnography and ethnology were institutionalized in Lemberg; even earlier than in Vienna (Tarnavs'kyĭ 2013). The world exhibition in Vienna, crown land exhibitions like the Galician Land Exhibition 1894 and several imperial publication projects, spear-headed by the well-known *Kronprinzenwerk* (*Die österreichisch-ungarische Monarchie in Wort und Bild*), supported the interest of imperial as well as national groups and societies in ethnography (Grieshofer 1998; Karpenko 2016; Scharr, Barth-Scalmani 2011).

standards in the related disciplines and for the communities they addressed. These standards, however, varied according to the respective community and location. The imperial *Volkskunde* intended to mediate national differences by emphasizing similarities between national groups and subgroups.[36] The respective disciplines in NTSh, on the other hand, were eager to scientifically justify a separate Ukrainian identity.[37] Consequently, conceptions of 'professional' and 'amateur' have to be historicized individually for a respective scientific community,[38] as those categories were co-produced.

After the reform of 1892, the standing rules of NTSh formulated the goal "to foster and develop science and art in the Ukrainian-Ruthenian language, preserve and collect any monuments, antiques and scientific objects of Ukraina-Rus", as well as the collection of related material in and outside of Galicia, which was considered necessary to achieve this goal.[39] As the following examples will show, there were generally two types of collectors, but with some intermediary positions.

2.1. Volodymyr Hnatiuk

Volodymyr Hnatiuk grew up in the small village of Velesniv/Weleśniów. When he was still a grammar school pupil, he collected songs and prepared them for publication in the Russophile journal *Novyi Halychanyn*.[40] The Russophiles are no coherent national identity, but rather a conservative orientation of Galician-Ruthenians "looking at the existing Russian state as a potential protector and savior".[41] While this episode illustrates the fact that national affiliations were not as clear and stable as often referred to, Hnatiuk reported later on that he did not consider the Russophile orientation as being as stigmatized as it was in the beginning of the 20th century.[42] While the Russophile movement lost more and more ground to the Ukrainian national movement since the last

[36] Stachel 2002.

[37] Cf. below, section 2.1.

[38] Vetter 2011, pp. 129–131.

[39] Kupchyns'kyĭ 2013, pp. 60–61, quotation p. 60.

[40] Hnatiuk, Volodymyr, *Avtobiohrafia* 1900 r. IR NBUV, f. 170, N520, ark. 1.

[41] Zayarnyuk 2010, p. 118. For a detailed account of the Russophile movement in Galicia, cf. Wendland 2001.

[42] Hnatiuk 1916b, p. III.

decade of the 19th century, the Russophiles remained a relevant political factor with support in several rural regions.[43] After completing his middle school education at the Gymnasium in Stanislaviv/Stanisławów, Hnatiuk enrolled at the University of Lemberg, where he studied classical and Slavic philology, including language history and phonetics, as well as Ukrainian history, between 1894 and 1898.[44] One of his professors was Antoni Kalina, the head of Towarzystwo Ludoznawcze at that time. This society was founded in 1895 and, even though the working language was Polish, it also included Ukrainian members. In his first semester, Hnatiuk handed Kalina a notebook containing some of his collected folk songs. Thereupon Kalina introduced him to Franko, who was a co-founder of the Towarzystwo. This connection turned out to be fruitful very soon, as Franko published materials and essays by Hnatiuk in his ethnographic and literary Journal *Zhytie i slovo*. Furthermore, Franko invited him to be one of the first members of the newly founded ethnographic commission of NTSh.[45] Hnatiuk took an unpaid two-month internship at NTSh, and then continued to work as a secretary for a minimal salary. When his salary was raised and he accumulated more positions, he served as an editor of several journals. Thereby, he was enabled to make a living, even though a poor one, without continuing his career as a schoolteacher that would have forced him to work outside of Lemberg. He considered this an altruistic decision justified by his love for science, popular culture and nation, as emphasized by his biographers.[46]

Galician-Ukrainian studies of folklore were influenced by the 1874–1875 two-volume folk song compilation that Mychaĭlo Drahomanov and Volodymyr Antonovych prepared during their work in the South-Western branch of Imperial Russian Geographical Society in Kyiv.[47] Through the work of Ivan Franko, who intensively corresponded with Drahomanov and edited the journals *Zoria* and *Zhytie i slovo*, which were dedicated to literature, ethnography and folklore, their comparative approach to folklore was disseminated among the Galician-Ukrainian *intelihentsia* already during the 1880s.[48] Franko's and Hnatiuk's idea of editing *EZ*

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[43] Zayarnyuk 2010.
[44] Dashkevych (ed.) 1998, pp. 12, 17–18.
[45] Mushynka 2008.
[46] Ivaniuta 2002; Mushynka 2012; Sheremeta 2003.
[47] Hnatiuk 1916a, p. 14; Dragomanov, Antonovich 1874–1875.
[48] Kolessa 2005, pp. 115–116.

adapted this method, as they conceptualized it as coherent collections of certain text genres. By organizing the submitted materials directly Hnatiuk's work reflected this idea, as the documents in his archive at the Institute of Art Studies, Folklore and Ethnology in Kyiv clearly show. He usually received texts in notebooks or other forms of paper bundles. He cut them out to rearrange them in the designated order for the volume he was editing.[49] Also the arrangements of texts in a certain volume were developed in a manner perceived as scientific and according to international standards and concepts.[50]

During the first half of the 19th century, Polish ethnographers considered Ruthenian folklore as an essential part of their own, referring to the Great Polish cultural identity. This inspired not only Ruthenians in the middle of the 19th century, who profited from existing structures of Polish folkloristics at that time,[51] but also Hrushevs'kyĭ. Introducing the first issue of *EZ*, he regretted the lack of any steady institution responsible for the preservation of Ukrainian folklore during the last decades, since "ethnographic materials did not find themselves a shelter, got lost completely or were released on god's world in alien publishing houses".[52] This point reflects his conception of the problems of the Ukrainian nation, as "the Polish nation-building strategy resembled the Great Russian one, for it also depended on restricting Ukrainian culture to a pre-national level of development and strove to exploit the Ukrainian ethnos as raw material for the construction of Polish national culture."[53] His position was certainly not shared by all of Galician-Ukrainian *intelihentsia*, but for Hrushevs'kyĭ's followers, such as his young students like Hnatiuk, it was definitely influential, as Hrushevs'kyĭ's inclusion of ethnography in historiography inflicted a new dynamic in the study of popular culture.[54]

In 1895, Hnatiuk carried out his first research journey to the Ruthenian territories in Hungary, inspired and supported by Franko and Hrushevs'kyĭ. Until 1903, five more trips followed. He is not only

[49] Cf. the compilations of *kolomyĭky* in IMFE, f. 28-3, spr. 424; spr. 425.
[50] Nakhlik, Sheremeta 2016, pp. 367–368.
[51] Schwitin 2013.
[52] *EZ* I, 1895, peredmova (unpag.).
[53] Plokhy 2005, p. 89.
[54] Kuzelja 1930, pp. 173–174, 178–179.

considered the first researcher of the region's folk prose; the overall quality of his recordings is considered to be "professional, that is to say, scientific, with complete preservation of the particularities of the vernacular, articulation and accents as well as lexical and morphological forms".[55] These features were not treated with the same accuracy in the amateur recordings produced until then. He soon became an expert on the region, did not restrict his activities to folklore collections, as he conducted also ethnographic observations, worked on establishing a steady scholarly dialogue and inspired many of his colleagues, such as Franko, to study the region.[56]

The third volume of *EZ*, which appeared in 1897, was the first volume of Hnatiuk's "ethnographic materials from Hungarian Rus", comprising exclusively his own collections. The influence of Hrushevs'kyĭ and Franko is apparent from the dedication, describing them as "devotees of Ukrainian-Ruthenian science, friends and teachers of the youth".[57] He carefully edited the language, gave parallels for stories and motifs in other languages as well as adding translations of dialect words otherwise not understandable to his contemporaries.[58] At the same time, he published several scientific papers, even though he was still a student. From 1896 he became a regular contributor to *Zapysky*, beginning with reviews. In the two years that followed, he placed two extensive papers in the journal. Other papers regarding his excursions to Transcarpathia were published in Franko's *Zhytie i slovo*.[59] His scholarly articles as well as his protest against the "denationalization" of Ruthenians in Hungary were intensely recognized in and outside of the Galician-Ukrainian community.[60] Since 1900, Hnatiuk had been more and more included in the process of editing NTSh's journals. After his last field trip in 1903, he suffered from severe health conditions, holding him from further expeditions. Therefore, his compilations of folklore depended on material he had collected earlier and especially on texts submitted by others.[61]

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[55] Romanenchuk 1981, p. 6.
[56] Pan'kevych 1926.
[57] *EZ* III, 1897, p. [III].
[58] *EZ* III, 1897, pp. XIV–XX.
[59] Mushynka 2012, pp. 50–52; Mushynka 2008.
[60] Nakhlik, Sheremeta 2016, p. 361.
[61] Hnatiuk 1916b, p. VI.

Hnatiuk was constantly anxious to improve. Franko was an important mentor, but he also turned to Fedir Vovk (1847–1918), a well-known Ukrainian anthropologist and ethnologist working in Paris. Hnatiuk was curious about the expert's opinion in order to improve, especially regarding his first compilations. He developed a relationship of trust with Vovk, asking him for advice in any possible matter. He complained about not having access to a lot of specialist literature on folklore in Lemberg, and those works he found were not providing the methodological advices for professional documentation of the collected folklore he wished for. While being able to read in German and any Slavic language but Bulgarian, his linguistic repertoire lacked French and English. Beginning to teach himself French, he slowly was able to read publications Vovk recommended to him.[62]

Hnatiuk's early work shows an independent style of working, even though influenced by prominent Ukrainian humanities scholars and general issues of Ukrainian science. After his first excursions, he had to be considered an expert in terms of folkloristic field work. He achieved a reputation as a scientist through his publications in *Zapysky*, while his further work on preparing folklore compilations confirms this picture. While one might argue that this was no scientific task, categorizing and arranging the texts included interpretive steps based on linguistic and folkloristic knowledge. Hnatiuk was setting standards himself and provided the most systematic compilations of the respective genres.

2.2 "Village intelihentsia"

Finally, who are these local activists incorporated into NTSh's research? Most of the calls for submissions were directly addressed to the "village intelihentsia", namely teachers and clerics. Himka categorizes educated groups in the countryside not having to earn their income through agricultural work as (village) "notables".[63] NTSh's conception goes a step further, limiting the group by their level of education mainly to teachers and clerics. Due to the prerequisites of the Habsburg monarchy's educational system, women were not part of this group and do not appear as an active part in the corpus of sources this paper is based

[62] Naulko, Rudenko, Franko (eds.) 2001, pp. 12, 16.
[63] Himka 1988, p. 106.

on. For Habsburg ethnography in general, it was quite common to encourage teachers and clerics to collect all kinds of material and information during the given period.[64] Himka describes a Josephinist habit of the clerics, meaning a dedication for community work outside the church.[65] This was highly valued by the national movement, even though an unavoidable association with any of the prominent or less prominent concepts of identity was not implied. The specific situation of Galicia was shaped by a relatively high illiteracy rate, exceedingly so in the countryside. In the Ukrainian case, the Lemberg-based enlightenment society Prosvita tried to counter it through an extensive introduction of reading halls during the 1890s. The coordination was further improved through branches on the level of districts or respectively towns.[66] Usually village priests and teachers managed the reading halls during their leisure time. The halls received the daily press and collected literary works as well as popular scientific publications and some – even though not complete – publications of NTSh.[67] The so-called "village intelihentsia" can therefore be considered to be literate and well-educated, but is also characterized by good reachability, connection to the ideals of popular enlightenment and, due to the respective activities, was well-informed about local conditions. Clerics also had to perform other worldly tasks, which brought them in close contact to the local population and their situation, as they were e.g. asked to confirm poverty of a person or family in order for pupils or students to apply for respective grants.[68] As these points illustrate, their position and their potential to support research was based on their location and the knowledge resulting from it.

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[64] Kaindl 1903, p. 94.

[65] Himka 1988, p. 120.

[66] Prosvita began its work already in 1868, worked on several educational and popular scientific matters and formulated even scientific aims in the first statute. The establishment of the branches slowly began in the 1870s, while the first five reading halls were introduced in 1891. In 1900, there were 924 and in 1908 already 2048 in Galicia. Lozyns'kyĭ 1908. For a detailed account on reading halls in Galicia cf. Struve 2005.

[67] Cf. fn. 167.

[68] Dashkevych (ed.) 1998, pp. 12, 17–18. Priests were also requested to help without an existing connection to Prosvita, as Hnatiuk's reports on his travels to Transcarpathia prove, but it was not always that easy for him to find support. *EZ* III, 1897, p. XX.

According to Jürgen Renn, local knowledge may "primarily serve to solve problems of human survival, such as food production, communication, healing, building and mobility", as well as producing identity. Clear distinctions are not always possible, for example when it comes to language following "purposes of communication" as well as "constitute and preserve cultural identity".[69] While folklore fulfilled primarily cultural needs, it was not only of interest to scientists analyzing features of local identities, but also linguistic matters. For ethnographic studies, observations on fishing, farming and other forms of food production were highly relevant.[70] But knowledge about local circumstances was also relevant for research processes. First, it was obviously crucial to find out who was able to sing folk songs or tell stories and anecdotes in a favorable way, i.e. most authentic for the local understanding. Researchers visiting from outside, of course, had to find out about suitable narrators and persuade them to invest their personal leisure time into slowly dictating the traditions – a skill they had to learn at first, according to Hnatiuk.[71] The easiest way to do that was by mediation of local activists. In case of more distant excursions, letters of recommendation, written by a recognized member of the intellectual community, were helpful to gain the trust of others. Local *intelihentsia* also supported researchers frequently by other means, such as accommodation, information about interesting events (funfairs, weddings) or about craftsmen. Derived from NTSh's perception, it is therefore plausible to speak of local experts.[72]

The process of notation posed other problems for the respective groups. Getting the nuances of spoken texts in local dialects – like intonations varying from the standard – while only hearing it once certainly required the researcher to have a certain level of qualification. Hnatiuk himself pointed out that he would only be able to understand local dialects in Transcarpathia because he had learned them beforehand.[73]

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[69] Renn 2012, p. 330.

[70] Dashkevych (ed.) 1998, pp. 31–36.

[71] *Lïteraturno-Naukovyĭ Vistnyk* 1899, T. 8, kn. 12, pp. 176–178.

[72] Olga Linkiewicz (2016) describes the same necessity for local knowledge for Polish ethnologists in the interwar period, whereby the aspect of language seems even more relevant.

[73] Dashkevych (ed.) 1998, pp. 31–36.

A local would not have this problem, but, if not connected to the scholarly community, rather had to learn the praxis of notation. This does concern especially the relation between local particularities and contemporary standard language. As Hnatiuk emphasized, the "school orthography"[74] had to be used, not etymological spelling, which was used by Hungarian Ruthenians and Galician Russophiles. This may have at least constituted an obstacle for older people not affiliated with the school sector. For example, as the statistical questionnaires of 1910 show, 'old' (etymological) spelling was still used by some priests.[75] There is also a letter of a pupil of the 8th grade, productive collector of song texts, who wished to note also the corresponding melody, but did not know how.[76] In general, melodies sent in were rather exceptional;[77] they were rather recorded by professional ethnographers with a phonograph, such as Osyp Rozdol's'kyĭ (1872–1945) and Filaret Kolessa (1871–1947).[78]

Peter Finke emphasizes a clear distinction between "citizen science light", which essentially means the collection of data, dependent on evaluation and analysis of professional scientists, and "citizen science proper". The latter describes a tradition bearing all central characteristics of science, but operating independently from institutionalized science.[79] That differentiation is certainly inspired by issues of a lack of recognition of lay scientists in the 21st century, whereas a strict understanding is problematic for the context of NTSh in late 19th and early 20th century. On the one hand, NTSh itself and its various projects can be considered as an alternative to science institutionalized by the state, mostly pursued in other languages, in the case of Galicia predominantly Polish. On the other hand, NTSh literally was *the* institution for science in the Ukrainian language during the given period, furnished

74 *Lïteraturno-Naukovyĭ Vistnyk* 1899, T. 8, kn. 12, p. 176.

75 Cf. e.g. statistical data of Orelets', TsDIAL, f. 309, op. 1, spr. 2550, ark. 15; statistical data of Borshchovychi, *ibid.*, ark. 19.

76 Letter to the ethnographic commission by I. Valiuk, 1913, IMFE, f. 29-2, spr. 4.

77 But not excluded, cf. description of the former archive of the ethnographic commission, IMFE, f. 29-5, spr. 478, ark. 80; Material collected by Ivan Saviak, IMFE, f. 29-3, spr. 247.

78 Dovhaliuk 2016; Kolessa 1907.

79 Finke 2014, pp. 36–46. Finke has important objections against the predominance of top-down citizen science as promoted nowadays, as it would degrade the "underestimated knowledge of laypersons".

with explicit interest in contribution by laypersons. In this more narrow sense, however, Finke's intervention inspires the question how local experts and amateur participators could contribute independently from NTSh ideology. The very conception of publications such as *EZ* was a careful edition of collected folklore, arranged in a comparative manner, usually not accompanied by sophisticated analysis and individual interpretations.[80] It rather meant an invitation to promote local popular culture, a concept which obviously sparked interest. Everyone capable of doing so could hand in the material he found in his environment. As long as quality criteria and the previously announced forms of text genres were matched, every participant had the chance of getting published. In this sense, it is plausible to speak of democratization.

Additionally, the commissioned work created a possibility for local experts to share their knowledge. One of Hnatiuk's supporters in Transcarpathia was Yuriy Zhatkovych, a priest in Stroine. He was asked by Hnatiuk to prepare an ethnographic work on the region. The first part appeared in the second volume of *EZ*, while the rest remained unpublished for a long time. The same model is valid for Ivan Franko's acquaintance Mykhailo Zubryts'kyĭ, whose affiliation with NTSh became much more intense. Both Zhatkovych and Zubryts'kyĭ were elected members of the ethnographic commission.[81] They wrote on topics of their local expertise and in this way helped to solve the issue of scarcity of authors in the NTSh journals. Such contacts in the countryside were also highly valued information for other researchers interested in the regions, Ukrainians as well as non-Ukrainians.[82] Networking therefore has to be considered one of the most important skills for field work and collections in the given period.

2.3. Mykhailo Zubryts'kyĭ

Mykhailo Zubryts'kyĭ (1856–1919) is an outstanding example for the significant role that Greek-Catholic clergy could play for local intellectual life. In the reception of his work, as Frank Sysyn recently summed

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[80] Hnatiuk usually presented analytic papers in LNV, *Zapysky* or in *Zhytie i slovo*. Exceptions are to be found in the later issues of *EZ*. For a bibliography of his printed works cf. Mushynka 1987.

[81] *EZ* II, 1896, pp. 1–36; Myshanych 1992, p. 33; Mazurok 2010, pp. 126–132.

[82] Dashkevych (ed.) 1998, pp. 19–20, 53–54.

up, it is always referred as "the interconnectedness of God's work and national work".[83] Zubryts'kyĭ was a friend of Franko ever since their common time at Drohobych/Drohobycz Gymnasium and got in touch with him again when he was serving in the military in Lemberg.[84] When he studied to become a priest, he certainly disagreed with the large number of supporters of the Russophile movement around his fellow students, as he problematized in his autobiography.[85]

Zubryts'kyĭ began publishing primarily short miscellanea in *Zoria* in the second half of the 1880s, when Franko was still collaborating with the journal. Their cooperation continued, when Franko began to issue his *Zhytie i slovo* in 1894, featuring not only short submissions, but also longer ethnographic descriptions of Zubryts'kyĭ's home village, Kindrativ/Kondratów. From 1898, he regularly published in NTSh's periodicals and other volumes, predominantly in the *Zapysky*, *EZ* and *Materials on Ukrainian-Ruthenian Ethnology* (further *MURE*).[86] Many of these works were commented editions of his ethnographic collections, but he also provided historical and economical accounts of Mshanets', the village where he worked as a priest since 1889 and served as head of the local Prosvita reading hall.[87] However, his interactions with NTSh were not limited to publications or his rare visits to Lemberg. As an acquaintance of Franko, he was asked to support several excursions to the region. Not only did he host Franko and Hnatiuk on several occasions,[88] but also the renowned expedition led by Fedir Vovk in 1904. During this undertaking, a lot of material on the region was gathered. Vovk concentrated on anthropometric measuring and photographs. Furthermore, ethnographic objects were collected for museums in Vienna, Lemberg and Petersburg. Songs were written down by the expedition members and one of them, Zenon Kuzelia (1882–1952), observed a wedding he described in detail in *MURE* 1908.[89] Zubryts'kyĭ did not only show the researchers around, but mediated with the locals, referred them to

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83 Sysyn 2012, p. 85.
84 Hrytsak 2006, p. 257; Sysyn 2013.
85 Zubryts'kyĭ 2016.
86 Zubryts'kyĭ (ed.) 2013; Iakymovych 2006, p. 146.
87 Sysyn 2013, p. 25; Zubryts'kyĭ (ed.) 2016, pp. 299–314.
88 Kyrchiv 2008, p. 377; Dashkevych (ed.) 1998, p. 60.
89 Kuzelia 1908.

priests of nearby villages and assisted them by other means. As this point shows, he did not play a passive role in this project, but contributed actively to the successful conduction of the research.[90] In 1904, he was made a 'real member' of NTSh, a degree awarded for scientific achievements and commitment to the society. This point has to be considered of explicit importance, which becomes evident in comparison with Kuzelia, who received the same honor only after completing his doctorate, while Zubryts'kyĭ did not have one.[91]

What came first, the chicken or the egg? While the scientists' interest in the region was, of course, stimulated by ethnographic peculiarities of the Boykos, it has to be doubted that the popularity of Mshanets' – certainly the most often researched Boyko-village by NTSh – was influenced only by this aspect. Zubryts'kyĭ's hospitality, communicated by Franko and therefore accessible to his colleagues, would have contributed to that condition as well. In this regard, the village priest played a major role with sustainable influence on the perception of his village and region, in scholarly as well as popular discourse. But was he considered equal to the Lemberg-based members?

In an essay Zubryts'kyĭ published in 1905 in LNV, he discussed the benefits of science for the public. He argued for the practical use of ethnographic information instead of pursuing science for the sake of itself.[92] As Franko argued in his reaction, even though Zubryts'kyĭ was a representative of NTSh, he was not conveying the position of the NTSh leaders. Franko pointed to the statutes of the society, which did not include any didactic mission, and openly considered the priest's views as provincial.[93] Still, as he pointed out in LNV in 1906, he highly valued Zubryts'kyĭ's work. Even though he did not consider the anecdotes Zubryts'kyĭ included in his research as scientifically valuable, they would provide a lively picture of his subject.[94] The idea of Ukrainian science articulated by Franko was addressed to an international scientific community, which should recognize Ukrainians as a cultural nation. These issues of proving intellectual maturity and national

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[90] Franko 1905b; Vovk 1908.

[91] Sysyn 2013; Patsaĭ 2013.

[92] Sysyn 2013, pp. 16–17.

[93] Franko 1905a; Sysyn 2013, p. 17.

[94] Franko 1906.

individuality were dominant topoi for NTSh until 1918,[95] also reflected in Hrushevs'kyĭ's aforementioned conception of academization of NTSh. While it is true that NTSh gave Zubryts'kyĭ the possibility to articulate opposing thoughts, they were followed by severe criticism of the journal's editor literally on the next page. Of course, the polemic should be relativized by the fact that NTSh was under severe attack from Ukrainian politicians for not engaging in popular education; that is the overall context of these articles. But again, Zubryts'kyĭ had many possibilities to support his fields of interest and articulate his positions. He handed in the material he considered as valuable, prepared at his own discretion and therefore even published anecdotes Franko regarded as irrelevant for science.

NTSh gave Zubryts'kyĭ the opportunity to place his region on the mental map of Ukraine or respectively (Eastern) Galicia in the different conceptions he addressed. Based on his support for Vovk's anthropometric research, he could also secure the presence of Boykos in broad works on Ukrainian anthropology, as well as place ethnographic objects as exhibits in museums in Lemberg, Vienna and St. Petersburg. Those objects were not only sold by NTSh: the Museum für Volkskunde in Vienna asked Zubryts'kyĭ directly for models of local buildings, which was mediated by Franko.[96] In summary, Zubryts'kyĭ functioned as an intermediary between several spaces – village, region, nation, and empire. He was able to generate advantages for himself as well as for the place and the group he intended to represent.

3. Collecting folklore

When Hrushevs'kyĭ asked Vovk, who had been working and studying in Paris for more than a decade by that time, about his thoughts on the first issue of *EZ*,[97] Vovk praised the journal as such, but severely criticized the attached research program. This program included a large amount of questions regarding folklore, ethnography, local law traditions and anthropology. The questions were very general and covered all possible

[95] Rohde 2019.
[96] Rechnung Zubryts'kyĭ, Archiv Museum für Volkskunde, Vienna.
[97] For his biography cf. Franko 2000.

fields of interest.[98] In Vovk's consideration, it was obviously very close to the program he had designed for the South-Western Branch of the Russian Geographic Society, issued in 1873. Compared to the state-of-the-art recent West European and US works, it would have been completely outdated. Furthermore, the questions would not fulfill their task of stimulating researchers. For communication between laypersons and scientists, language is often considered to be the key point; the scientific language of the experts had to be "translated" into something their network could understand.[99] When it comes to ethnography, the problem is less about terms, but more about how to communicate what information is of interest for the researchers, as Vovk demonstrated by the example of a general question on wedding rituals:

> They [the amateurs, M.R.] will describe the wedding for you, but only so that you know that at the wedding there are bridesmaids, bride's male attendants and a *karavai* [*korovai* in contemporary Ukrainian; bride cake, M.R.] – and that's it, but nobody will tell you what you really need, because it does not come to their mind that you would need it. I am sufficiently aware of that issue […].[100]

Accordingly, a "need for mediation"[101] with non-professional researchers was postulated. Vovk suggested much more detailed questionnaires for specific matters, while matters of ethnographic collections should have been separated from ethnological and anthropological works. Soon, this also manifested itself in terms of the periodicals issued by the commission. The *MURE* were designated to the scientific study of anthropology, while the *EZ* was to remain responsible for all kinds of folklore.[102] In the first volume of *MURE*, issued in 1899, Vovk proposed a research program for collecting information on "Arts of life" (Vovk's term in parenthesis; Ukrainian: *pobutova tekhnyka*) featuring specialized sets of questions on hunting, fishing, cattle breeding,

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98 *EZ* I, 1895, pp. 1–16 (separate pagination).

99 Coen 2013, pp. 11–12.

100 N. N.3 2001, p. 111.

101 Coen 2013, p. 101.

102 To Vovk's disappointment, distinctions between these subjects were handled rather fluidly in NTSh. Cf. N. N.3 2001, pp. 111–112.

grain cultivation, metallurgy or tanning.[103] While Vovk initially planned to intensify this concept with further detailed programs, such as the one for weddings he intended to publish already for the first volume, misunderstandings between him and Hrushevs'kyĭ as well as financial limitations of the journal obstructed a more intense development of this kind of questionnaires.[104] The program for weddings was circulated between interested scholars anyway and contributed to the special issue on wedding ceremonies in part two of *MURE* 1908.[105] In the decade that followed, other specialized programs were designed, such as for collecting *pysanky* (traditionally designed Easter eggs), songs – including melodies – or dialectal peculiarities.[106]

Vovk had strong objections to the integration of amateurs into anthropological research, he argued for example that anthropometric data would be outright worthless taken by a layperson, since that task would have to be fulfilled by a trained specialist.[107] He also dissuaded Hnatiuk from conducting anthropological research, as the latter was not trained in this field; if he would like to do so, Vovk suggested to visit him in Paris for a semester in order to learn the latest scientific methods.[108] While amateurs were not categorically excluded from contributing ethnological or anthropological treatises or submitting related records, the emphasis of further programs was on folklore. Ivan Franko and Volodymyr Hnatiuk became responsible for the collecting, administering and publishing folklore; the latter, as the commission's secretary, was also responsible for organizational tasks.[109] Together they were responsible for calls and questionnaires on folklore as well as the conceptualization of further volumes of *EZ*.

3.1 Questionnaires and specialization

The Chronicles (*Khronika Naukovoho tovarystva imeny Shevchenka*) and Literary-scientific herold (*Lïteraturno-Naukovyĭ Vistnyk*), a monthly

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[103] Vovk 1899.

[104] *Ibid.*, pp. 152–161.

[105] *Materiialy do ukraïns'ko-rus'koï etnol'ogiï* 10, 1908, part 2, pp. 1–150.

[106] For a full account of such programs published and/or distributed by NTSh, cf. Hnatiuk 1916a, pp. 18–19.

[107] N. N.3 2001, p. 104.

[108] Dashkevych (ed.) 1998, p. 34.

[109] Sapeliak 2000, pp. 24–34.

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revue, respectively their offprints, were a tool of communicating with the network of collectors apart from individual correspondence. The ethnographic commission not only posed special requests when new projects for *EZ* were planned, but also reacted to frequent questions in a collective manner. The editors introduced their concept for *EZ* already in their first call for submissions in LNV, in 1899. The upcoming volumes would be comprised of systematic arrangements of certain text genres, so that every volume would constitute a self-contained whole. The same text would also not be republished; thus contributors could orient themselves and would not conduct unnecessary work. By communicating their publishing plan the commission stimulated its supporters in advance to hand in texts of desired genres in time.

As it is apparent from Hnatiuk's editing, the methodology of recording was of essence. Every transcript sent in should contain at least the information who wrote it down, where, when and from whom.[110] Even more than before, the editors emphasized that it would be necessary to write every text on a separate paper, only on one side. Every word should be written down as the narrator said it, without leaving anything out or adding anything; all specific features of dialects should be written down as accurately as possible, without changing anything into literary language.[111] These aspects correspond to Hnatiuk's principles for the materials to have a scientific value. The descriptions, examples and instructions provided in the calls therefore also had a pedagogic value for prospective 'village scientists'.

In 1899, Franko and Hnatiuk asked for collections of folk legends, giving detailed descriptions of their most common topics with the request to turn to villagers talking about these.[112] When the commission intended to edit three volumes of *kolomyĭky* – two rhymed verses, integrated in folk music and folk dances – a pertinent call was issued. Obviously, the commission knew its network well already, since it turned directly to the network with the request to submit notebooks with earlier collections by parents and grandparents of contemporary activists.[113] Further specialized calls requested local tales as well as historical

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[110] *Lïteraturno-Naukovyĭ Vistnyk* 1899, T. 8, kn. 12, p. 178.
[111] *Lïteraturno-Naukovyĭ Vistnyk* 1902, T. 17, kn. 1, p. 78.
[112] *Lïteraturno-Naukovyĭ Vistnyk* 1899, T. 8, kn. 12, pp. 176–178.
[113] *Lïteraturno-Naukovyĭ Vistnyk* 1904, T. 26, kn. 6, p. 196.

documents on robbers of the 16th to 18th century, funeral customs or local narrations on place names and historical figures.[114] For the latter case, Hnatiuk prepared an extensive list of examples, which derived from his own collection, to illustrate what kind of material he was looking for in this more abstract request. At the same time, he provided patterns to show how these narrations should be written down.

To motivate prospective contributors, the commission continued to appeal to the *liubyteli's* altruism. Calls included the emphasis on free time of the village *intelihentsia* and peasants during long winters, but this formulation did not originate in any pejorative attitude about people living in the countryside; it was rather related to Hnatiuk's romantic ideas and childhood memories. In 1904, a reward system was successfully installed. If materials were published, the contributors would get a free copy of the related issue of *EZ*; if they contributed a lot of texts, they might receive other issues also.[115] Apparently, this was a successful measure, since a summary in 1907 stated there was much more material than could be published due to financial reasons.[116] Collecting seemed to be a never-ending story for Hnatiuk. During World War I, collections were lost and destroyed, so that Hnatiuk turned to the younger generation, which would have the "duty to save those collections with numerous new notations" of folklore.[117] Even more than before, responsibility for cultural heritage was delegated. Some remaining letters of collectors prove not only their love for popular culture, but also that the questionnaires stimulated their interest and motivation.[118] The calls had the possibility to influence the network with special requests, reconfiguring it according to changing needs and educating participants with scientifically approved patterns.

3.2 Contributors, their interests and perspectives

Due to the huge number of volunteers who contributed to the collection of the ethnographic commission, it is not possible to give detailed accounts in this article. Of the vast material, mostly preserved in IMFE,

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114 *Khronika* 1907, Nr. 31, pp. 28–31; Nr. 32, pp. 16–25; 1909, Nr. 39, pp. 37–39.
115 *Khronika* 1904, Nr. 20, p. 17.
116 *Khronika* 1907, Nr. 29, pp. 16–17.
117 Hnatiuk 1916a, p. 17.
118 Sapeliak 2000, p. 49.

only a fraction has been published; many texts do not feature the name of their collectors.[119] Accordingly, I will discuss the general base of interaction between the ethnographic commission and the volunteers, before considering examples of two of the most outstanding contributors.

In the school year 1909–1910, a historical-ethnographic circle was formed at Ukrainian-language Lemberg Academic Gymnasium. It was designed to attract pupils in the various disciplines and for one thing covered specialist lectures, by teachers of the school as well as by NTSh ethnographer and musicologist Filaret Kolessa. During spring and summer break, the pupils were instructed to collect objects for the National Museum and folklore for NTSh library.[120] In November 1910, the circle sent a collection of 2,400 texts, foremost *kolomyĭky*, to NTSh. In return, they asked to refer to the circle if the material was published and to receive a collection of NTSh's recent publications.[121] Several other contributors also asked for certain issues of a periodical as a reward, even before this system was officially announced. This way, NTSh supported their contributors directly by following their interests, as they sometimes did not have the possibilities to buy them on their own.[122] Yet another contributor hoped to be rewarded with other monographs published by NTSh.[123]

This is an example of an inspired teaching method and at the same time it underlines the mutual cooperation between Ukrainian institutions in Lemberg. Since participation was optional, it was either the subject that attracted pupils or the motivation by the teacher Aleksii Sushko, so that the pupils devoted their holiday time to this research. Ivan Pankevych (1887–1956) provides a suitable long-term example of a person successfully promoting young scholars. He became acquainted with NTSh already as a pupil of a Polish gymnasium in Lemberg. When he was preparing a talk on Transcarpathia, where he was born,

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119 Documented in the inventory of the former archive of Ethnographic Commission, IMFE, f. 29-5, spr. 476. Hanna Sokil (2011) delivered the most detailed account on Ukrainian folklore in Galicia and covers more of the lesser-known names.

120 Zvit dyrektsyii ts. k. akademichnoii himnazyii u L'vovi za shkil'nyĭ rik 1909/1910, pp. 51–52.

121 TsDIAL, f. 309, op. 1, spr. 747, Letter to the executive board, 1910.

122 Sapeliak 2000, pp. 47–49.

123 IMFE, f. 29-2, spr. 3, Letter to the ethnographic commission by T. Kuzyshyn, 1911.

he regularly worked in the library of NTSh. There he also met several of its affiliates. The most influential acquaintance, as he wrote in his autobiography, was Volodymyr Hnatiuk. He wrote down folklore in the village where he was born, as well as in the surrounding area. The first pieces were published already in 1907, in *EZ*. He stayed in contact with NTSh during his studies in Lemberg and Vienna, and was made a real member in 1923. In the interwar period, he worked as an organizer of a national intellectual movement in Uzhhorod (then in Czechoslovakia), editing the journal Scientific Collection of Prosvita-Society in Uzhhorod (1923–1938). His works on dialectology and ethnography made him one of the most important researchers of the region. Early participation and motivation by and with NTSh sparked his interest into pursuing a scholarly career, following the footsteps of Hnatiuk.[124] However, collectors without scientific ambitions were also dedicated to this task.

Bohdan Zaklyns'kyĭ (1886–1946) was born in Stanislaviv/Stanisławów (today: Ivano-Frankivs'k) in 1886. His family was well-known for its involvement in the Ukrainian national movement and their regional pedagogical work. His father, Roman, was a gymnasium teacher and also conducted ethnographic research. Bohdan Zaklyns'kyĭ studied at the teacher seminar in the city and graduated as an elementary school teacher in 1909. Between 1909 and 1913, he taught in several villages of Kosiv district.[125] His motivation to collect originated in the almost endless amount of folklore. As he explained, after weddings in a village, people would "sing *kolomyĭky* all night long, and all are new every time". There would simply be not enough specialists for this task, therefore "everyone who just has the possibility should take part in that, such as village priests, teachers, students, and even more enlightened villagers."[126] His national romantic motivation is also reflected by the notebooks he handed in, sometimes with a big portrait of the national poet Shevchenko on the front, sometimes with portraits of many other Ukrainian writers or an included quotation of the national poet as a prefixed motto.[127]

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124 Mushynka 2001, pp. 79–90.

125 Kas'kiv 1998.

126 IMFE, f. 28-3, spr. 178, ark. 3, Oral literature, collected by Bohdan Zaklyns'kyĭ.

127 IMFE, f. 29-3, spr. 85a, Zaklyns'kyĭ, B., Etnohrafiia; IMFE, f. 29-3, spr. 82–84, Baiky, etc. written down by Bohdan Zaklyns'kyĭ; IMFE, f. 29-3, spr. 79, Folk songs, written down by B. Zaklyns'kyĭ and others.

He also inspired his family members as well as other acquaintances to leave him collections to submit them to NTSh, including material his father and others collected in 1874.[128] Some of the latter material was also published.[129] The personal notes he handed in started around 1904. When Zaklyns'kyĭ was still a student, he sent his first collections to Ivan Franko. He received a long answer, which he did not expect from the well-known scholar. Franko instructed him on how he could find old ethnographic documents and what kind of material would be interesting for NTSh. He was highly stimulated by this advice and handed in a chant from 1738. Hnatiuk also supported the young collector with hints on how he might conduct research in Transcarpathia. Zaklyns'kyĭ was also very interested in the published calls for contributions, as he reacted to many of them, for example to the *EZ* volume on bandits and historical tales. Even during World War I, when he served in the *Sich* Riflemen, he collected soldier songs on Hnatiuk's suggestion.[130] After the war, when NTSh as well as other aspects of the region's intellectual life greatly transformed, he kept his connection to the ethnographic commission.[131]

Zaklyns'kyĭ did not limit himself to folklore. He donated ethnographic and archaeological objects to the museum and many books and manuscripts to the library on several occasions since 1907.[132] In 1913, he submitted a collection of drawings by his pupils, on another occasion – archaeological and ethnographical notes on Halych, as well as reported having found a coin during an excavation, which he had already sent to the museum.[133] In 1918, he contributed a short paper on nutrition in Kosiv district to *MURE*. In that description he took up on Hnatiuk's and Franko's position against generalization of ethnographic regions, arguing that eating habits would even vary between two close villages. Most of his publications, including mostly popular-scientific

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[128] IMFE, f. 29-3, spr. 205, Rostyslav Zaklyns'kyĭ's notebook, 1904; spr. 246–248, Materials by Ivan Saviak; IMFE, f. 28-3, spr. 178, ark. 27; Folk songs, collected by Zaklyns'kyĭ R., Zaklyns'kyĭ B., IMFE, f. 29-3, spr. 78; IMFE, f. 29-3, spr. 79.

[129] *EZ* XXXV, 1914, p. IV.

[130] IMFE, f. 29-3, spr. 245, War songs, collected by Bohdan Zaklyns'kyĭ.

[131] Kas'kiv 1998, pp. 78–83.

[132] *Khronika* 1907, Nr. 32, p. 2; 1908, Nr. 34, pp. 17; 1911, Nr. 45, p. 59.

[133] IMFE, f. 28-5, spr. 566, Letter by Bohdan Zaklyns'kyĭ, 1913; IMFE, f. 28-5, spr. 177, Archaeological and ethnographical notes by Bohdan Zaklyns'kyĭ, 1904.

and pedagogical works, appeared independently from NTSh.[134] While he was an expert for his region as well as for collecting, he did not have ambitions for a scientific career. This commitment was not one-sided. He was regularly rewarded with issues of NTSh periodicals and elected member of the ethnographic commission in October 1912. His father Roman and his brother Rostyslav were appointed ordinary members in 1914, too.[135]

As opposed to this well remembered family, Ivan Voloshyns'kyĭ (?–?)[136] is a barely known collector, who was situated in Daleshiv/Daleszova,[137] a village in the district Horodenka. His collection, which was submitted to the ethnographic commission, is now preserved in the Institute for Art Studies, Folklore and Ethnology of Ukrainian National Academy of Sciences (IMFE) and consists of 20 notebooks.[138] Eight of them contain folk songs, five tales, four *baiky* (Ukrainian fables), two *kolomyĭky* and other songs and one contains legends. His first record dates back to 1893, when he exclusively collected folk songs.[139] In 1897, he started to collect legends and since 1909, he focused on tales and *baiky* until 1913.[140] Hnatiuk published two papers by Voloshyns'kyĭ in *MURE* 1919. He followed Hnatiuk's wish and collected material on funeral customs and weddings between 1911 and 1913.[141] This short history of collections clearly shows how the calls and questionnaires stimulated a dedicated researcher.

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[134] Kas'kiv 1998, pp. 80, pp. 172–179; cf. below for the position of Franko and Hnatiuk.

[135] *Khronika* 1907, Nr. 32, p. 26; 1912, Nr. 52, p. 17; 1914, Nr. 57, p. 24.

[136] Biographical data is also not secured. The online Encyclopedia of Modern Ukraine considers him a cleric and accounts for his death after 1917. However, there is no reliable evidence given for that. The cleric in Daleshiv/Daleszowa was Nykolaĭ Voloshyns'kyĭ, while no other person of this last name comes up in the respective directory of the Greek-Catholic diocese in Stanislaviv, to which the village was assigned. Cheremshyns'kyi 2006; *Shematyzm vseho klyra hreko-katolytskoy Eparkhiï Stanyslavivskoï na rik Bozhyĭ 1913*, p. 33–34.

[137] Today, the village is named Daleshove, Horodenkivs' kyi raion, Ivano-Frankivs'ka oblast'.

[138] IMFE, f. 28-3, spr. 103–113; f. 29-3, spr. 206–214, Notebooks by Ivan Voloshyns'kyĭ.

[139] IMFE, f. 28-3, spr. 103–105.

[140] IMFE, f. 28-3, spr. 109–112; f. 29-3, spr. 206.

[141] *Materiialy do ukraïns'koï etnol'ogiï* 1919, 19–20, p. 194.

His records are characterized by great attention to detail. Usually he placed all accents according to the local articulation,[142] in opposite to Zaklyns'kyĭ. Voloshyns'kyĭ also provided translations of dialectal terms, added references to similar folklore and most of his texts are well dated.[143] Hence, they are a productive source, while only one collection of songs and *kolomyĭky*, consisting of a cut up notebook and other loose paper, lacks detailed information. He also gave an overview of the motives of some *baiky* in case something would not be understandable since, as he emphasized, he would write down every story exactly as dictated.[144]

With only a few exceptions,[145] all of Voloshyns'kyĭ's recordings originate from the village as well as neighboring places. His notebooks contained a few recordings prepared by his relatives,[146] but the majority of them were written down by himself. Zaklyns'kyĭ was a much more mobile researcher, as he recorded material in different regions in Galicia as well as in Volhynia and Transcarpathia.[147] Many texts of them were cut out due to Hnatiuk's method of arranging the material for publication. The sole fact that 449 of his records were incorporated into the two volumes of *EZ* on demonology – that is roughly a third of the volume, published in 1912,[148] documents the outstanding quantity and quality of his contributions.

Upon Hnatiuk's request, Voloshyns'kyĭ also collected ethnographic objects from his home village for the ethnographic museum of NTSh. For his support, he was able to receive literature as well as money. Hnatiuk also ordered relevant ethnographic books as a reward for Voloshyns'kyĭ and Zaklyns'kyĭ in 1914, which should have been bought antiquarian if not available otherwise.[149] Voloshyns'kyĭ's bad health condition might have been responsible for the interruptions and the end of this extraordinary cooperation,[150] but his biography is mostly unknown.

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142 E.g. in the notebooks IMFE, f. 29-3, spr. 206–209.

143 IMFE, f. 29-3, spr. 213, ark. 6; Sokil 2009, p. 155.

144 IMFE, f. 29-3, spr. 207, ark. 1 zv.

145 IMFE, f. 28-3, spr. 213, ark. 2–7, Ethnographic material from Volhynia, collected by Bohdan Zaklyns'kyĭ.

146 Sokil 2009, p. 155.

147 IMFE, f. 28-3, spr. 272; IMFE, f. 28-3, spr. 178.

148 *EZ* XXXIV, 1912, p. III.

149 Dashkevych (ed.) 1998, pp. 276–277.

150 Hnatiuk 1916b, p. IX.

Voloshyns'kyĭ delivered extensive material from the ethnographic region of Pokuttya, situated in the south-eastern border region of Galicia. His work is highly relevant, as he managed to preserve fluent texts as well as objects from the region.[151] The same conclusion is true for Zaklyns'kyĭ, as he did the same for Kosiv (Pol. Kosów) district or respectively *hutsul'shchyna* and Mykhailo Zubryts'kyĭ for *boikivshchyna*. The nexus between folklore – and other aspects of ethnography and anthropology – and region-building was shown foremost in terms of organization and cooperation. *Kolomyĭky* are a suitable example to substantiate these observations with the content of research. They literally are an "invented tradition",[152] extraordinary popular and a relevant feature of identity in Western Ukrainian lands, based on historical customs. Regionality is an important aspect of analysis in Ukrainian folkloristics, ethnography and their history, because research was usually influenced by these regional categories and relying on ethnic subgroups localized there. When it comes to the definition of regional identity, Franko and Hnatiuk never ceased to emphasize that overall generalization would be harmful, as it would conceal all the independent phenomena of subregions, villages or even parts of them.[153] This position sums up why they spent all the energy on collecting and publishing material from small and otherwise rather unknown villages.

4. A statistical survey and the Cisleithanian census

Citizen science does not always have to be a highly successful long-term operation, as the following example shows. During January 1911, the statistical commission of NTSh surveyed localities in 40 Galician districts. The documents preserved in the Central Historical State Archive of Ukraine in Lviv consist of 290 questionnaires – processed by about 210 volunteers[154] – and two letters by surveyors, while it remains unknown if these are all filled-in forms that were returned to NTSh.[155]

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[151] Sokil 2009.

[152] Hobsbawm, Ranger 2013; for Folklore in this concept cf. p. 6.

[153] Kyrchiv 2002, pp. 8, 17.

[154] Since not all volunteers filled in their name as required, the number is rounded.

[155] Statistical data, TsDIAL, f. 309, op. 1, spr. 2547–2553. All of the documents in these folders, except the two letters, are the discussed statistical questionnaires, so that the subsequent references will not contain separate denominations.

However, no concise evaluation of this data was ever conducted. In Hnatiuk's historical sketch on NTSh, prepared in 1924, the only definite confirmation on this matter is provided. He complained that the material has "remained hitherto unused", even though it contained "much valuable"[156] information. First of all, I want to scrutinize the possible reasons for the absent final evaluation of the questionnaires. Furthermore, these available documents allow a look at strategies, motivations and problems of citizen science on a broader scale. The material is also interesting as it is able to provide empirical evidence regarding theoretical positions on the influence of categories in the census.[157] In order to do so, the process of organizing the commission and the execution of this project have to be reconstructed. Based on that, it is possible to discuss the volunteers, their efforts and problems with the accumulated data.

In 1906, NTSh founded a statistical commission to deal with Ukrainian national statistics, in Eastern Galicia as well as in other Ukrainian lands, to research the Habsburg census in particular and foster public awareness on the matter. The inspiration was sparked by Stanislav Dnistrians'kyĭ, a legal scholar elected to the House of Deputies of the Imperial Council (Reichsrat) in 1907. In the same year, he set statistics on the agenda of Ukrainian politicians by proposing – without success – to change the law on the census. This is barely a coincidence, considering the increasing importance of the decennial counting. In Galician-Ukrainian national discourse, the engagement with Habsburg language statistics was all but new. Two general issues were suitable to interest scientists, politicians and other national activists.

Firstly, the issue of equitable distributions of state resources between the respective national or language groups was addressed. The Cisleithanian census and its imprecise concept of *Umgangssprache* triggered mistrust in officials and served as explanation for believed denationalization. In Ukrainian-Ruthenian discourse, this was addressed shortly after the first time the category had come to use in the census of 1880 by a publicist Volodymyr Barvins'kyĭ (1850–1883).[158] This

156 Hnatiuk 1984, p. 76.

157 Göderle 2016.

158 Barvins'kyĭ's continued articles on the matter were reissued posthumously in Barvins'kyĭ 1901. For an overall discussion of the issues on *Umgangssprache* cf. Brix 1983; for Habsburg census in Galician-Ukrainian discourse Rohde 2016.

perspective comes close to the idea of linkage between citizen rights and citizen science,[159] as the corrected data and identified faults should have been used to demand equal distribution and support political arguments referring to this. Secondly, the ethnographic description of Ukrainian territories and their inhabitants was a topic most relevant for several projects of NTSh: on a regional level in order to discuss national predominance in the crown land Galicia or respectively its eastern part and on a transimperial level to describe the nation as a quantified whole.[160] Both perspectives had the potential to be used in national propaganda, calling people to action by emphasizing danger and injustice. Already in August 1910, a campaign began to foster awareness for the assumed indications of the upcoming census, initiated by Dnistrians'kyĭ. This does not only concern a transregional newspaper campaign, but also agitation on the village level.[161] Dnistrians'kyĭ intended to use political as well as scholarly means to deal with statistical injustice – as he understood it. In Vienna, he directly addressed the Abgeordnetenhaus for many proven violations of the law by Galician officials in preparing and conducting the counting. Since his attempts would not bear fruit, he requested village priests to oversee the process and mediate if necessary. On a scientific level, he planned to conduct his own surveys to challenge the official results of the census.

While he openly communicated the plan of using mediators in the press as well as in the Abgeordnetenhaus,[162] the intended surveys were kept secret for as long as possible. This fact already hints at the expected political problems, even though they were practically legal. Czech and German national organizations had already conducted similar initiatives regarding the census of 1900, afterwards broadly discussed in parliament. Regional authorities tried to prohibit projects like these, but their measures were not approved by the state, because such surveys did not categorically interfere with state affairs.[163] Also, the published Chronicle of NTSh, which usually documented research projects in great detail, only mentioned that the historical-philosophical section had decided

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159 Leach, Scoones 2005, p. 22.
160 Tomaschiwskyj 1908.
161 Dnistrians'kyĭ 1910b.
162 Dnistrians'kyĭ 1910b.
163 Brix 1982, pp. 276–78.

to ask the committee to pay for the production of questionnaires in November 1910. While the committee granted the requested money, as noted in the protocols,[164] the published information remained silent about that matter, especially about the time when the surveys were to be conducted. It seems even unlikely that the publication was available before the surveys were conducted.

As the timing of the copies provided by NTSh indicate and a couple of hints in the letters and on the questionnaires prove,[165] the survey was conducted in parallel to the official census executed in January 1911. The overwhelming mass of the surveyors were Greek-Catholic clergymen; in Drohobych/Drohobycz (4 persons), Kaminka Strumylova/Kamionka Strumiłowa (9), Zbarazh/Zbaraż (11), Zhydachiv/Żydaczów (6) and Zhovkva/Żółkiew (3), it was exclusively clerics doing the counting.[166] Local knowledge of the clerics was exceptionally valuable for this kind of inquiry, since many of them served for long periods of time in the same village, took care of parish registers and worked with the local reading halls. Affiliates of Prosvita got to know how many people were actually taking part in cultural activities related to the Ukrainian language group. As sample checks from the Ternopil' area clarify, several of the people who counted were affiliated to, and often in a leading position of, a local Prosvita reading hall.[167] Teachers,[168] advocates[169] and a student of the higher school of economics in Dubliany/Dublany[170] volunteered as well. Advocates were an advancing stratum of secular

[164] TsDIAL, f. 309, op. 1, spr. 34, ark. 130 zv., Protocols of the historical-philosophical section.

[165] TsDIAL, f. 309, op. 1, spr. 2547, ark. 16–20, 52, 55; spr. 2550, ark. 22 zv., 39 zv.

[166] TsDIAL, f. 309, op. 1, spr. 2548, ark. 23–26; spr. 2549, ark. 1–5, 5a, 6, 10–20; spr. 2550, ark. 1–9.

[167] Cf. the reports of local reading halls to the Ternopil'-branch: DATO, f. 294, op. 1, spr. 4, ark. 9–10, Spravozdanie 27th October 1912; f. 294, op. 1, spr. 20, ark. 41 zv., Spravozdanie 26th September 1911; f. 294, op. 1, spr. 29, ark. 3–4, Spravozdanie 12th September 1909. Affiliated members match the persons in charge of counting in TsDIAL, f. 309, Op. 1, Spr. 2553, ark. 15, 17, 20. All in all, the reports to the regional Prosvita branches and the documents on the survey are neither consistent enough nor were they conducted in a matching timeframe to compare the involved personnel on a broader level.

[168] TsDIAL, f. 309, op. 1, spr. 2551, ark. 34–35; spr. 2554, ark. 9.

[169] TsDIAL, f. 309, op. 1, spr. 2547, ark. 29, 31; spr. 2554, ark. 15.

[170] TsDIAL, f. 309, op. 1, spr. 2550, ark. 23.

national elites that worked also in smaller towns. The lawyer Damian Savchak, who was responsible for two surveys in Borshchiv/Borszczów district, was a former deputy of the Galician *sejm* and a founding member of NTSh.[171] The tasks Ukrainian volunteers would fulfill are therefore to be considered as "citizen science light" in Finke's conception. Anyway, there was no such thing as a 'professional' surveyor in the official census; these persons would be chosen and instructed by the respective village administration some time before the census.[172]

Dnistrians'kyĭ first articulated a detailed plan for the survey at the international Prosvita congress in 1909. He proposed a survey to investigate native languages, colloquial languages, language in family hierarchy, mixed-language and mixed-religion families, what part of the population was taking part in national literary or political life, what part affiliated itself with the nation or if migration or assimilation processes influenced the local conditions.[173] One of the most pressing issues to be researched was the large number of *latynnyky*. This term was frequently used for persons of the Roman-Catholic rite speaking Ukrainian. Like other identities deviating from the 'usual' national combination of Ukrainian language and Greek-Catholic rite, the *latynnyky* constituted a challenge for the Ukrainian national movement. As NTSh historian Stepan Tomashivs'kyĭ stated on the very same congress, the *latynnyky* were constantly ignored by the national movement, creating a scenario in which the Ukrainian nation was most likely to "lose" them, if no action was taken.[174] The congress decided that Prosvita should support the matter and handed it to the main council for execution.[175]

The first page of the questionnaire asked for the distributions of languages and rites in the village. Surveyors were supposed to answer them with the related number of inhabitants as well as their share of the total population. In the upper right corner, the data on rites from 1900 should be noted. Many answers contained only information

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[171] *Khronika* 1911, Nr. 45, p. 25.

[172] *Erinnerung betreffend Wahrung und Freiheit des Bekenntnisses der Umgangssprache bei der Volkszählung 1910*, AVA, k.k. Ministerium des Innern, Nr. 46485.

[173] Dnistrians'kyĭ 1910c, p. 197.

[174] Tomashivs'kyĭ 1910, p. 106. The issue of *latynnyky* is described in detail in Pavlyshyn 2014; for their perception in NTSh cf. Rohde 2016.

[175] Dnistrians'kyĭ 1910c, p. 201.

e.g. on the share of Ruthenian speakers, having stated neither the quantity of the concerned inhabitants nor their total number, leaving the statistician without a hint on the actual quantitative relations. Some surveyors provided exact answers, just not noted in the unit the statisticians had in mind, for example when the question "which part of all inhabitants are speaking Polish at home?" was answered with "10 families".[176] Someone else delivered only the official numbers of 1900 and submitted the questionnaire without any other addition.[177] Other numbers were entered inconsiderately and hence just made no sense, such as the numbers delivered for Kuty (district Kosiv/Kosów), where about half of the 6,689 inhabitants were considered Jews, while 6,689 Jews were supposed to speak Polish.[178] But the most frequent problem were estimates given instead of a real survey.[179] That is very obvious when exclusively rounded figures were given. Often, only (vague) proportions were entered without giving any exact number for the state of the population in 1910.

The second page contained more complicated questions to be answered with a text. They primarily addressed the interrelation of rite and language of the village population. Hence they asked if there were mixed marriages between Roman and Greek-Catholics, what language they spoke; if there was a recent change of rites between these groups or historical change of rites of a larger group, respectively a Polish colony; were the village population in general or *latynnyky* in particular polonizing (*polshchat' sia*) or ruthenizing (*rushchat' sia*)? Were there *latynnyky* consciously describing themselves as Ruthenians? Were their clothing or customs any different?[180] These questions were inspired by Dnistrians'kyĭ's earlier research agendas, but formulated in easier language and divided into less complicated subquestions. In five questions, additional terms were given in parentheses, so that they were understandable for everyone. For example, the question if there were Greek-Catholics "latinized" was thus translated to "change to Latin [rite, M.R.]"[181].

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[176] TsDIAL, f. 309, op. 1, spr. 2550, ark. 1.
[177] TsDIAL, f. 309, op. 1, spr. 2548, ark. 17, 19.
[178] TsDIAL, f. 309, op. 1, spr. 2550, ark. 12.
[179] TsDIAL, f. 309, op. 1, spr. 2550, ark. 2, 9, 70.
[180] TsDIAL, f. 309, op. 1, spr. 2447, ark. 1 zv.
[181] *Ibid.*

The only two questions containing abstract vocabulary without explanation were those asking about polonization or ruthenization.[182] These questions were also those most frequently unanswered. Even these terms were easy to acquire, not only through a general feeling of language, but also through the newspapers of the last year: the populist daily *Dïlo* and the UNDP-weekly *Svoboda* were dispatched to almost every Prosvita reading hall.[183] It is therefore possible to interpret the indifference to these questions also as indifference to these nationalized categories. Take, for example, one person who filled out 17 of the 18 questionnaires for the district Pidhaitsi/Podhajce. The surveyor delivered statistical data for the region in great quantity, even though foremostly only the questions easiest to know, i.e. not the information on languages spoken at home; this is an indication for the data just being guessed or taken over from the official census. The second page he ignored in almost all of these forms, so that overall the documentation for this district was rather weak.[184] This argumentation is not designed to stigmatize clerics or local volunteers in general, as some examples prove the opposite. Vasyl' Mydlovs'kyĭ, the local priest, described Rosokhach/Rosochacz (district Kolomyia, Pol. Kołomyja) in exact numbers, answered all questions comprehensibly and provided additional information on problems of the organization of the official census.[185] Since the number of clerics is overwhelming and not all volunteers can be categorized by their profession, a related investigation would not be promising. It might be more suitable to ask whether age/generation of the contributors allows any further conclusions, but this demands further research on lesser-known individuals.

Some persons surveyed two or more villages. They were often holding a position in the Greek-Catholic church that forced them to be mobile. This concerns either those being responsible for religious services in several places or clerics with specific tasks in the regional deanery. The most common examples are school inspectors taking care of a couple of villages, such as Liubomyr Zaryts'kyĭ in Berezhany/Brzeżany district

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[182] TsDIAL, f. 309, op. 1, spr. 2547, ark. 1 zv.
[183] Cf. the reports in fn. 167.
[184] TsDIAL, f. 309, op. 1, spr. 2551, ark. 13–20, 22–30.
[185] TsDIAL, f. 309, op. 1, spr. 2550, ark. 10.

or Ivan Koroliuk in Peremyshliany/Przemyślany district.[186] A few persons, not affiliated with the church, conducted a striking quantity of surveys. Antin Rak was an official and well-known activist in the small town Vynnyky/Winniki. Not only was he an organizer of a choir and a theatre group, but he also served as secretary of the local Prosvita branch, where he initiated the building of a *narodnyĭ dim* in 1910. In the following year, he successfully promoted the Ukrainian National--Democratic Party on the local level.[187] He was responsible for counting ten communities in the surrounding area of Lemberg,[188] similar to the case of Pidhaitsi/Podhajce. Stepan Baĭdala was an active member of the local Ukrainian-national community in Mostys'ka/Mościska. At least since 1907, he was an active member of the town's Prosvita branch and since 1910 he worked as head of their national credit cooperative.[189] Several times, he did not answer questions in the intended way, but just wrote all over the second page that "it is a pure Polish colony"[190]. For Mostys'ka/Mościska, 28 questionnaires were submitted, 13 of them by Baĭdala. Furthermore, there are 14 questionnaires from Mostys'ka/Mościska without names of surveyors. The handwriting as well as the style of filling out the form is very similar to Baĭdala's. For this reason, one can assume that Baĭdala was responsible in a way for these as well. Compared to the size of the district, it was the numerically most intensely surveyed of the crown land due to Baĭdala's activism.

About these young political activists one can doubtlessly state that they conducted their work because they believed in the importance of the census for the sake of the nation, just as it was communicated by Dnistrians'kyĭ. It has to be doubted that one person was able to survey all the places alone. Other questionnaires indicated that they used help of like-minded colleagues (on one form six persons directly signed as responsible).[191] Both of them were in influential positions in national

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[186] *Shematyzm vseho klyra hreko-katolytskoy mytropolychoy arkhieparkhiy l'vôvskoy na rôk 1911.* L'vôv 1910, pp. 217, 398; TsDIAL, f. 309, op. 1, spr. 2547, ark. 3–4; spr. 2551, ark. 11–12.

[187] Vlock 1970, pp. 184–185; Levyts'kyĭ 1970, pp. 210–211.

[188] TsDIAL, f. 309, op. 1, spr. 2550, ark. 17–18, 20–21, 24, 27, 30, 41, 44, 46.

[189] Martyn 2009, pp. 80–81.

[190] TsDIAL, f. 309, op. 1, spr. 2550, ark. 4 zv.

[191] TsDIAL, f. 309, op. 1, spr. 2547, ark. 28.

organizations. It seems logical to consider that the tasks were delegated and the responsibility hence assumed in case of legal consequences. At least when it comes to the anonymous surveyors in Mostys'ka/Mościska district, this seems highly probable. The location of the regional center with a well-connected Prosvita branch supports this assumption.

To conclude, must this operation be regarded as a fail? Asserting that the collected data is entirely worthless would be an overstatement. As numerous examples regarding the first half of the questionnaires showed, the numeric data needed for statistical purposes was often incomplete, so that it could not be used for calculations. Whereas a detailed evaluation of the material was never conducted, it is safe to consider that the survey shaped the further perception of the crown land's statistics by political representatives and the statistical commission. This becomes evident in Volodymyr Okhrymovych's publication on *Actual and Fictive Executions of Ruthenians* in 1912, as his points arguing for horrific falsification of language data were certainly inspired by the incidents of the census including the collected information. Suitable examples of information valuable without correct and complete numbers are provided by the written answers; in the district of Rohatyn, four out of four people reported polonization, in Sambir it was six out of seven.[192] These and other cases correspond with Okhrymovych's discussion of denationalization, *latynnyky* and fabricated "Poles of Greek rite".[193] Furthermore, the incidents regarding the census helped foster public and political awareness for Ukrainian issues at the state level, as the degree of awareness fostered by activism resulted in many complaints directly to the Austrian Ministry of the Interior and indirectly voiced by Dnistrians'kyĭ in parliament. The mistrust towards manipulated data and, consequentially, Polish officials, accompanied by these arguments, used to be important topoi in national propaganda during World War I and the Paris Peace Conference.[194] It has to be assumed that all these consequences were directly intended, as they were thoroughly foreseeable by coordination the proposed research with the official conduction of the census. While the outcome was barely used as a contribution to science, it shaped the views of scientists.

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[192] TsDIAL, f. 309, op. 1, spr. 2551 ark. 41–44; spr. 2552, ark. 1–7.
[193] Okhrymovych 1912, quotation p. 28.
[194] Rohde 2016, pp. 100–107, 113–116.

In comparison to direct, personal research for NTSh, there was no promise of integration, recognition or compensation. Quite the contrary, many clerics seemed to consider the task of overseeing the official counting as illegal and hence stayed away from participating. While Dnistrians'kyĭ assured them that it would not violate any existing law,[195] there is no secure base for judging his influence on them. Rather, it can be assumed that the appeal of Metropolitan Sheptyts'kyĭ was decisive for the large number of clerics participating. It was not only published in *Svoboda*, but also in the organs of the church. He warned against negative implications for the national movement as well as for the Greek-Catholic church.[196] Therefore, he did not only emphasize the usual national argumentation of Dnistrians'kyĭ and colleagues, but appealed to instances most relevant even for national-indifferent persons. It would be, however, erroneous to speak of encompassing success, considering that many districts had only very few volunteers.

Of course, not all affected districts were of particular interest, as they were ethnically more homogenous, such as Sniatyn/Śniatyn, Kosiv/Kosów and Tovmach/Towmacz. For others, such as Zhovkva/Żółkiew, Bereziv/Brzozów and parts of Stryĭ/Stryj, a huge number of falsifications were assumed by the statistical commission.[197] As Tamara Scheer and Rok Stergar have recently argued, processes of public nationalization were not just a result "of the nationalist's pressure and the subsequent concessions of the Habsburg state",[198] but catalyzed by bureaucratic classification practices, such as *Umgangssprache* in the census. The program was designed to counter undesired implications of the state's category, after initiatives with the same goal failed in parliament.[199] However, the survey is a suitable example to discuss nationalization, national indifference and mistrust in state institutions simultaneously.

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195 Dnistrians'kyĭ 1910a.
196 N. N.1 1910.
197 Okhrymovych 1909, pp. 100–101.
198 Stergar, Scheer 2018, p. 586.
199 Rohde 2016, pp. 83–88.

5. Conclusion

This paper covered examples of successful efforts on region-building from *boikivshchyna*, *hutsul'shchyna*, Pokuttya and Transcarpathia. Local experts and collectors provided material and support for research that is still relevant today for Ukrainian culture and regional identity, as they made the preservation of volatile oral literature possible to an extent, which would have been impossible to reach in this short timespan with NTSh's limited resources. The task to preserve and order the material was fulfilled by NTSh as long as it existed. After annexation of former Galicia by the Soviet Union, the material was assigned to different state-owned scientific organizations.[200] Until today, the respective collections, among others, are valuable sources for scholarly work.[201]

Even though NTSh gave up a lot of its elitist habitus, this did obviously not lead to an overall democratization of science, as dreamt of by today's programs for citizen science. Considering the education level of Galician rural territories, this diagnosis is not surprising. The reforms opened up several opportunities to participate, for amateurs and adepts of science alike. When it came to the self-conception of NTSh's mission, this assessment has to be restricted: as the debate between Zubryts'kyĭ and Franko shows, popularizing science was not the agenda of NTSh's leadership.[202] However, aspects of participation also served the scholarly education of interested locals. The duration of the collection programs contributed to the building of the scientific community. The statistical project, on the other hand, turned to existing networks – Prosvita and the Greek-Catholic church – for support. Stimulation for their work was limited to questionnaires and information about sources of falsification by officials.

While commissioned work was regularly asked for, Hnatiuk exclusively addressed these local experts, who had already shown dedication for work of that kind, offering them stimulations by questionnaires and outlines of the proposed texts. Anyway, possible contributions were not

[200] For the fate of NTSh archives after annexation, cf. Svarnyk 2005.

[201] Sokil 2011 is one of the latest examples.

[202] I recently pointed at that issue in Rohde 2019, even though a more detailed and systematized treatment would be in order. However, that is only one field of disputes, as NTSh was frequently the stage of inner disagreements between Hrushevs'kyĭ and his allies versus other groups. Cf. Zaĭtseva 2006.

limited to proposed topics. Properly individualized projects led to fruitful integration of locals, whereas the collection of statistical data suffered from communication. The complex and standardized categories of the questionnaires caused problems not regarding their language, but the related concepts. Nevertheless, the statistical inquiry contributed to national identification in the countryside as well as to the political recognition of Ukrainian demands by the state. This point illustrates how participation of locals could be "simultaneously functional and symbolic"[203]. In contrast to politically harmless collections, the census, its control and the additional survey by NTSh was part of the national conflict in Galicia. This had major implications, because different interest groups were mobilized or kept from participating. The surveyors did not have to be *liubyteli* and their work was nothing to do on a 'long winter evening' or during their holidays. They had to work under pressure of time and politics, which might be the major reason for the inconsistent quality of the results. The initial project was considered not only of situational value, but also for ethnographic description. From this standpoint, the influence of the project is marginal for Ukrainian history of science. However, it demonstrates the nexus of science and nationalization in the given period. But nationalization was no inevitable consequence of the participation in a scientific network, even in imperial context. As Coen has shown, the amateurs participating in earthquake observations were integrated on a supranational level.[204] However, NTSh put considerable effort in the nationalization of its members and other participants. While the statistical observation included a certain politicization of the involved persons, it is not possible to transfer this aspect to the ethnographic work, where only mild (regional) patriotism can be assumed without additional sources.

Since prospective participants were narrowed down to specific groups,[205] determined by nationality (or at least language) and education,

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[203] Vetter 2011, p. 136.

[204] Coen 2013, pp. 141–162.

[205] The given cases do not allow any generalization in terms of generation or geographical background, as the discussed contributors were fairly heterogenous. Such a conclusion could only be drawn from a close network analysis of a much larger amount of contributors. Unfortunately, the lost documents from NTSh administration during World War I would complicate such an undertaking.

the universal approach of the term 'citizen science' seems partly inappropriate for the given case study. While terms like 'lay' or 'amateur' participation bear the linguistic burden of suggesting a certain lack of expertise, the opposite proves to be true when it comes to the factor of local knowledge in given regions: Locality and local expertise were crucial, as the vast countryside of Eastern Galicia posed an impediment for the projects. Therefore, participants from the village *intelihentsia* might as well be considered 'local experts'. As the story of Hnatiuk shows, engagement in tasks like collecting folklore or describing local customs was an opportunity to establish contact with the scientific community. Cooperation usually started as a top-down relation, limiting the contributions to the mentioned tasks. In some cases of long-lasting affiliation, local experts had the possibility to renegotiate their status and become members of NTSh, providing them with greater possibilities to transcend borders between the discussed classifications. This was bound to individual expertise and willingness to devote time. In contrast to this phenomenon, many contributors remained 'amateurs' (positively understood as 'enthusiasts'), limiting their engagement to pursuing ethnographic research as leisure time activity. For local experts, aspects such as getting recognized by being allowed to contribute, being rewarded with a book or a small, yet symbolical amount of money or finally, such as Zubryts'kyĭ, being elected a 'real member' of NTSh, seem to be more relevant than demands of alternative scientific discourses. NTSh, ingratiating itself with the locals by paying them and local matters attention, was also considered the institutional alternative to more elitist or non-vernacular approaches to science.

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"We can and we must": The scientificity of trade-union history-writing in the Soviet Union in the 1920s[1]

Abstract

In the 1920s, the young Soviet Republic, rejecting the old social system, turned to the study of the past. Instead of engaging with professional historians, the new regime initiated a whole range of large-scale participatory projects incorporated into political and public institutions to produce new, revolutionary history. In this article, instead of approaching this topic in terms of ideology and memory I put it in the context of history of science.

[1] This article is based on the research I have done in the European University at St. Petersburg in 2014–2017.

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Focusing on the case of trade unions, I suggest considering the early Soviet non-academic history-writing as a form of radical citizen science. Even though trade unionists had no special education, they dared to use scientific methods in their research that ended with positive results. This story allows us to question the opposition between amateurs and professionals in the field of citizen science.

Keywords: *history-writing, citizen science, trade unions, The USSR, the 1920s, Marxism, archeography*, *Istprof*

„Możemy i musimy": Naukowość pisarstwa historycznego o związkach zawodowych w Związku Radzieckim w latach 20. XX wieku

Abstrakt

W latach 20. XX wieku młoda Republika Radziecka, odrzuciwszy stary układ społeczny, zwróciła się ku badaniom przeszłości. Zamiast współpracować z profesjonalnymi historykami, nowy reżim zapoczątkował całą gamę dużych projektów partycypacyjnych pod kontrolą instytucji politycznych i publicznych, których celem było stworzenia nowej, rewolucyjnej historii. W tym artykule, zamiast podchodzić do tego tematu w kategoriach ideologii i pamięci, umieściłem go w kontekście historii nauki. Skupiając się na przypadku związków zawodowych, sugeruję rozważenie wczesnego sowieckiego, nieakademickiego pisarstwa historycznego jako formy radykalnej nauki obywatelskiej. Mimo że związkowcy nie mieli kierunkowego wykształcenia, odważyli się wykorzystać metodę naukową w swoich badaniach zwieńczonych pozytywnymi rezultatami. Przykład ten pozwala nam kwestionować opozycję między amatorami i profesjonalistami w dziedzinie nauki obywatelskiej.

Słowa kluczowe: *pisarstwo historyczne*, *nauka obywatelska*, *związki zawodowe*, *ZSRR*, *lata 20. XX wieku*, *marksizm*, *archeografia*, *Istprof*

1. Introduction

In 1925, a year after Vladimir Lenin's death, two thin mass-circulated books with similar titles were published in the Soviet Union: Georgii Lelevich's *Lenin as a Historian of the Party and the Revolution* and Vadim Bystrianskii's *Lenin the Historian. Historicism in Leninism*[2]. Both authors stressed that Lenin used history in political struggle, basing his decisions on discussions of historical trends. Although Lelevich and Bystrianskii wrote about Lenin's use of history in the political sphere – an activity that is usually considered to be the opposite of academic history-writing – for both of these authors Lenin's use of history was scientific. Indeed, Soviet Marxists did not find in the interconnection between history and politics anything preventing truly scientific research of the past. This view was possible, in part, because the words "academic" and "scientific" were not synonyms for the Bolsheviks.

Marxists claimed that Marxism itself was a particular type of doctrine, scientific socialism, opposed to the utopian ideas of pre-Marxist socialists[3]. The Bolsheviks were committed to building their political program on scientific evidence and to correlate it with Marx's general social theory. Science for them was thus not only a way to interpret the world, but also a powerful tool for changing it. Even outside the academy, making war or conducting the economy, the Bolsheviks were engaged in science. This fact had a twofold consequence. On the one hand, right after the revolution the Soviet state started to invest heavily in scientific institutions supporting fundamental and, especially, applied research. On the other hand, the scientificity of Marxism undermined the independence of academic elites. The Bolsheviks deprived professionals of their monopoly over science, paving the way for the wide participation of amateurs in the production of scientific knowledge. The notion of citizen science is a very fruitful lens that allows us to see not only repressive, but also productive side of the Bolsheviks' scientificity. In this paper, focusing on trade-union history-writing, I will demonstrate how the scientificity of the Soviet regime determined both the intellectual

[2] Bystrianskii 1925; Lelevich 1925.

[3] Kotkin 1995, pp. 7–8.

and institutional contexts of the production of knowledge in the 1920s. But first, let me elaborate why I believe that it is necessary to consider Soviet history-writing in the context of history of science.

2. Early Soviet history-writing: Literature and context

The approach of several generations of scholars who have studied history-writing in the early Soviet Union has been shaped by the political implications of this topic. History-writing has been seen as a space of political struggle. Soviet historians who started to study this issue in the 1960s wrote about a bitter ideological struggle between "bourgeois" and "Marxist" camps in the historical discipline[4]. At the same time, scholars in the United States also began to explore how the new Soviet government had worked with the past. They wrote about the rivalry between Marxist and non-Marxist historiographies, exploring the mechanisms that allowed history to be used for propagandist and ideological aims[5]. In both cases, the Communist Party was the main character of the story, described either as the protector and patron of a truly scientific history, or as a manipulator and tyrant trying to monopolize its interpretations and schemes.

An important shift in the historiography of the Soviet history-writing occurred with the development of memory studies. As early as 1983, Eric Hobsbawm wrote about "invented traditions", which are "responses to novel situations which take the form of reference to old situations, or which establish their own past by quasi-obligatory repetition".[6] Hobsbawm and other contributors to the landmark volume *The Invention of Tradition* (1983) were highly critical of nationalist movements and focused their attention on the problem of how contemporary interests and social relations constructed the past. This new wave led to a certain normalization of the Soviet experience, where the annual Royal Christmas Message in the United Kingdom was just as much an "invented" tradition as Soviet May Day marches, and the French nation as much an "imagined" community as the Soviet *narod*.

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[4] Alekseeva 1968, p. 7. See also: Fediukin 1965; Ivanova 1968; Alatortseva 1989; Klushin 1971; Maksakov 1959.

[5] Enteen 1976; 1986; White 1985; Frankel 1966; Holmes, Burgess 1982.

[6] Hobsbawm 1983, p. 2.

Despite the importance of the contribution that memory studies have made in the field of Soviet history, it has an important limitation: it tends to focus on the pragmatics of the Bolsheviks' use of history, which leads to an overestimation of their freedom in constructing narratives. The overwhelming focus has been on history-writing as a useful tool for the legitimation of the revolutionary regime and for the mobilization of population. Frederick Corney writes about the "foundation tales" of the Soviet regime, which were constructed by the inscription of personal memoirs into the official scheme of revolutionary events. The Bolsheviks did not invent the history of the revolution, but they provided a vocabulary and structure for the narrative that was filled by numerous personal stories. This allowed the Bolsheviks to prove that October was not a *coup d'état*, but a real revolution, and that the people's will allowed them to take power. Corney writes, "Like all foundation narratives, the story of October is by definition a *legitimizing* process"[7].

Other scholars have emphasized how history was used in Stalin's 'Great Retreat' to shift the ideological focus away from class-oriented and internationalist revolutionary ideals to nationalist rhetoric in order to mobilize the population. David Brandenberger works with Russo-centric Stalinist mass culture and argues that the Party's use of Russian national heroes and myths to promulgate the dominant Marxist-Leninist line

> signaled a symbolic abandonment of an earlier revolutionary ethos in favor of a strategy calculated to mobilize popular support for an unpopular regime by whatever means necessary.[8]

The heroes of Russian national history represented in movies, textbooks, and popular brochures replaced the revolutionary imagery and played a pivotal role for the development of national identity that survived even after 1991.

It is difficult to reject the claim that history was used under the Bolsheviks as a tool in their political struggles,[9] but I claim that although the

[7] Corney 2004, p. 5. See also: Hartzok 2009; Narskii 2004; Dobrenko 2008, p. 6.

[8] Brandenberger 2002, p. 2. See also: Platt, Brandenberger 2006; 1999.

[9] For example, see the political "Literary discussion" around Trotsky's historical writings. See: Corney 2015.

Bolsheviks were engaged – in the words of Pierre Bourdieu – in "strategic action" as they worked with history, history remained a part of the scientific endeavor of the Communist project. Bourdieu's nuanced approach to science is helpful here, as it allows us to see that "strategic action" need not negate the scientific qualities of knowledge.[10] It is necessary to shift the focus from the question "what was history used for?" to the question "why was it constructed in that particular way?" in order to see the social and intellectual contexts of the process. In this paper, I argue that to answer this question we have to scrutinize the specific scientificity of the Soviet regime. Focusing on the trade-union historical commissions, I will show that the composition of the Soviet revolutionary narrative was tightly bound, on the one hand, to the particular institutional context of the development of science, and, on the other, to the tension between inductive and deductive forms of scientificity in the history-writing of the 1920s.

Although the combination of "science" and "history" might look strange in an English language context, in Russian the two terms are more closely connected.[11] "Science" is not a precise translation of the Russian word *nauka*. Formed under heavy influence of German tradition in the 18th and 19th centuries, Russian *nauka* was closer to *Wissenschaft* than to "science" – this term had a broader meaning and also included the social sciences and humanities.[12] And this is more than a matter of words. "In the same boat" with scholars of the natural and social sciences who had begun to accomplish practical functions – providing new technologies for industry and new tools of population management – history also moved beyond the borders of academia.

The Bolsheviks did not introduce Russia to the utility of science. Indeed, the convergence of the scientific and political spheres was well under way in Russia long before 1917. As Peter Holquist writes, in the middle of the 19th century in Russia, as in other European countries, the idea that the population could be counted, managed and even improved,

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[10] Bourdieu 1975, p. 19.

[11] See, for example, Tikhonov 2016. In this work, he considers the historical discipline an integral part of Soviet science.

[12] Dmitriev 2015, p. 11. See also: Ringer 1990 (1969), pp. 102–103; Novick 1988, p. 24.

appeared. This attitude towards the population was based on the development of the social sciences, such as statistics, economics, anthropology, and criminology. These disciplines promised to equip governments with tools that would make social processes visible and manageable; this "contributed to officials' belief that they could grasp and manipulate such processes".[13] This allowed imperial officials after the revolution to adapt and to continue to work under the Bolsheviks quite easily, because they shared the same technocratic ethos.[14]

After the 1917 revolution, the development of applied social and natural sciences continued at a new level. In the 1920s, applied psychological disciplines such as pedology, defectology, psychotechnics, clinical psychology, pedagogy, and others institutionalized;[15] the ideas of eugenicists were utilized in medicine, especially in the sphere of reproductive healthcare;[16] physical anthropologists consulted economic planners;[17] the Bolsheviks' national policy was forged in close cooperation with ethnographers of the Russian Geographical Society and of the Academy of Sciences;[18] several criminological research centers were founded in order to rationalize the penal system.[19]

In addition, a similar process took place in the natural sciences, which moved from pure fundamental knowledge closer to applied research. This shift was reflected in the new types of scientific organizational structures formed in the USSR in the 1920–1930s. These were not universities, but rather special research institutions that played a pivotal role in the science of that time. Scientists in these institutions did not teach and could not freely choose the topic of their research, because they were tightly connected to the aims of the industry and the planned economy in general. This type of science was far from the noble values of independent research but gave scientists new opportunities. As Alexei Kojevnikov writes,

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13 Holquist 2001, p. 113. See also works on the population management by means of social sciences: Beer 2008; Engelstein 1994; Tolz 2011.

14 Holquist 2010.

15 Iasnitskii, Zavershneva 2009; Iasnitskii 2015.

16 Krementsov 2011.

17 Mogil'ner 2008.

18 Hirsch 2005.

19 Beer 2008, pp. 165–204.

> [u]nder Bolshevik rule, scientists lost much of their autonomy and independence but acquired more social prestige and de facto influence on politically important decision making.[20]

In general, the immediate post-revolutionary years were a boom period in the institutionalization of new research and educational centers. This process was initiated at once by both the academic community and the new regime. Although many of the most ambitious projects were not realized, they were an important component of the revolutionary moment in the history of science in Russia.[21]

History was considered to be one of the sciences in the USSR, and it is possible to see a similar pattern in its development in the 1920s. However, there was a crucial difference in the way the historical discipline was applied. In the cases of anthropology, criminology, pedagogy, and others the Bolsheviks engaged directly with professional communities, bolstering their institutionalization or employing the facilities of existing scientific institutions. In the case of history, on the contrary, the new regime was highly suspicious toward the academic establishment of the classical universities and the Academy of Science. Instead of engaging with professional historians, scientific method was utilized by amateurs. The manufacturing of a revolutionary narrative was initiated immediately after the Civil War with the creation of special centers – historical commissions incorporated into political and public institutions such as the Bolshevik Party, trade unions, the Komsomol, the Red Army, and others. These non-academic historical commissions were rarely staffed by professional historians. In most cases, their members were rank-and-file employees who did not research history as such but produced histories of (and for) particular institutions, while at the same time claiming to use scientific methods.[22]

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20 Kojevnikov 2008, p. 122. See also: Kojevnikov 2002.

21 Gruzdinskaia, Metel' 2018; Metel' 2017; Dolgova 2017.

22 As Evgeniia Dolgova rightly notes in her article, the 1920s were a period of deep crisis in the historical discipline. Debates and disorientation took the place of positivist methodological consensus at the beginning of the twentieth century. See: Dolgova 2013. Attempting to use historical methods in their work, party members and trade unionists could not but internalize these methodological discussions. Severe conflict broke out in 1924, for example, between the leaders of the Party historical commission,

Later historians have paid much attention to the Commission on the History of the Party (Istpart) that was directly subordinated to the Central Committee of the Bolshevik Party and had branches in every governorate.[23] Although Istpart was highly influential, it was just one case in a whole range of similar historical commissions. If we lose sight of other centers, we see a false picture, whereby the construction of revolutionary narratives was fully monopolized by the Party from the very beginning of the Soviet era. To move beyond this limiting approach, I focus on the case of trade-union history-writing to show how the scientificity of the Soviet regime influenced institutional and intellectual contexts of the history-writing in the 1920s. The Commissions on the History of the Professional Movement (Istprofs) existed within the complex structure of Soviet trade unions. Just as the institutional systems underlying Soviet history-writing were heterogeneous and complex, so the narratives they produced co-existed and competed with one another. The narratives of the history of professional movement did not contradict the Party line, but at the same time were not reducible to it.

I believe that recent developments in citizen science provide us with a fruitful lens for reconsidering the Early Soviet history-writing and the case of Istprofs in particular. Elena Aronova notes that there are two conflicting understandings of citizen science in literature. On the one hand, natural scientists and some historians of science use this term when writing about loyal participation of amateurs in the projects driven by professional scientists. For Science and Technology Studies (STS) scholars, on the other hand, citizen science is a type of grassroot movement that challenges established scientific institutions and practices and produces alternative local knowledge.[24] I would like to use the Aronova's juxtaposition of "active volunteers' engagement" and "activist democratic engagement" as a framework for the analysis of the complex relationships between Istprofs and professional historiography. Istprof commissions were definitely not a loyal supplement to some academic research projects. They were staffed and ruled by trade unionists, not professional historians. However, rather than challenge existing historiography, Istprofs

Vladimir Nevskii and Mikhail Ol'minskii, who could not agree on the application of methods of primary source critique. See: Gilmintinov 2015.

23 Corney 2004; Holmes, Burgess 1982; Zelenov 2000.

24 Aronova 2017.

produced an alternative one. Before the 1917 revolution, very recent history of trade unions had not been considered as a legitimate research topic among professional historians, who had focused on the Medieval and Early Modern periods. That is why professional historiography was considered irrelevant for Istprofs, rather than being a rival.

In the following section of the paper, I will be comparing Istprofs and another historical commission incorporated into the same structures of trade unions, i.e. the Commission on the History of Labor in Russia: unlike Istprofs, it was staffed with professional historians. These two commissions coexisted for almost four years but failed to cooperate. I will then outline the scientificity of Istprofs, showing how it related to Marxist epistemology and the historiographical mainstream of the 1920s. I will demonstrate that Istprofs' research program did not exclude relations with professional historiography; even though Istprof members scarcely communicated with contemporary academic historians, they addressed their work to a "future historian". This will conclude with an examination of Istprofs' publishing strategy, in order to juxtapose Istprofs and archeography, an important historical sub-discipline that was engaged in similar activities, i.e. the preparation and publishing of materials for researchers. This comparison allows me to underline how much the particular institutionality of Istprof influenced the research agenda of its members.

3. The incorporation of history-writing into the structure of trade unions

At the Fifth All-Russian Conference of Trade Unions in November 1920, among intense arguments about the unions' role in the proletarian dictatorship, Mikhail Tomskii, the leader of the Soviet trade unions, announced organizing special commissions on the history of the professional movement within the institutional structures of the trade unions. Although the Bolsheviks were still very far from the victory in the Civil War at the time of the conference, Tomskii claimed that "the moment has come when we can and we must think about the preparation of our labor history".[25] The commissions were named Istprofs, and very

[25] [N.N.1] 1921, pp. 177–178.

soon they had developed into a branching network. At the same time, the Petrograd Council of Trade Unions, an agency that administered union organizations in the former capital of Russia,[26] founded an independent historical commission – the Commission on the History of Labor in Russia. In this section, I use the cases of these two commissions to show the complexity of the quest for a proper form of non-academic history-writing in the 1920s.

Let us begin with the Istprofs. Their agenda and structure echoed those of the Party historical commission (Istpart), which had been founded just before Istprofs. Initially, the leadership of the Soviet trade unions even considered delegating the historical work to a subcommission of Istpart.[27] However, once the structure of Istprofs was established, there is no evidence among their documents to show that party historians attempted to control the work of their union colleagues.

Istprofs were so deeply incorporated into the structure of the trade unions that we can see their narratives of the professional movement not only belonging to trade-unionism as a whole, but also to the agendas of particular trade union organizations. The commissions were not managed and funded centrally, but by each trade union organization separately. Although the central Istprof was the leading commission that enunciated the aims and scientific methods of the professional movement's research, it could not directly control the activities of other Istprof commissions. They depended more on their trade union organizations than on the central Istprof; this led to a polyphony in the professional movement narratives, which were written from different points of view.

These organizations were formed into two parallel hierarchies: spatial and sectoral. The All-Union Central Council of Trade Unions (VTsSPS), controlled, on the one hand, central committees of particular

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[26] St. Petersburg, the capital of the Russian Empire, was renamed Petrograd in 1916 and then Leningrad in 1924. In 1918, Moscow became the capital of the Soviet Russia.

[27] David Riazanov, a prominent Bolshevik intellectual and union activist, who was initially appointed as the chief of the central Istprof, was too busy to tackle the history of the professional movement as well. See one of the first reports of the central Istprof: The State Archive of the Russian Federation (here after GARF), fond 6935 *Komissiia po Izucheniiu Istorii Professional'nogo Dvizhenia pri Vsesoiuznom Tsentral'nom Sovete Professional'nykh Soiuzov*, op. 1, d. 2, l. 4.

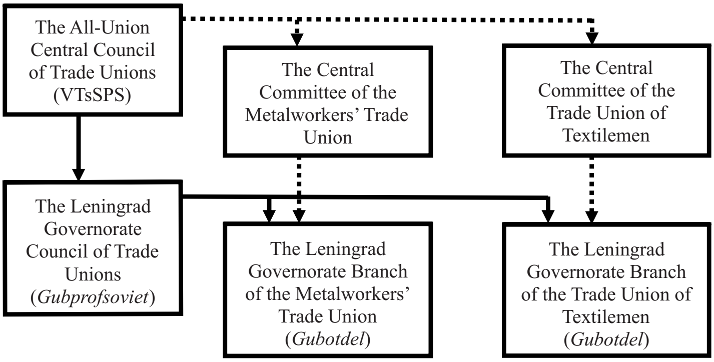

Diagram 1. Drawing on the example of the trade unions of metalworkers and textilemen in the Leningrad governorate, the intersection of the sectoral and spatial hierarchies is shown here. The dotted arrows demonstrate hierarchic lines from VTsSPS to the trade unions of particular industries and through them; the black arrows indicate the spatial scale of Soviet trade unionism – the subordination of the governorate councils to the VTsSPS.

industrial trade unions in Moscow, and, on the other hand, the governorate councils of trade unions (*Gubprofsovety*). Particular trade unions also had governorate branches – *Gubotdely*, which were subordinated to the *Gubprofsovet* of their governorate and at the same time to the central committee of the union as well (see Diagram 1).

The depth of Istprofs' incorporation into the trade unions was reflected in the sensibility of the professional movement narrative to the interests of the trade union organizations to which they were subordinated. Indeed, for example, one of the first books published by the Istprof of the Leningrad council was a collection of materials on the history of the professional movement in St. Petersburg in 1905–1907. S.I. Gruzdev, its editor and a member of the Leningrad Istprof, described the first moments of the uprising among the metalworkers of the Putilov factory. He proudly wrote that "the proletariat of Petersburg was the initiator of the movement".[28] Former metalworker Fedor Bulkin, in the book *The History of the St. Petersburg Union of Metalworkers* released by the Istprof of the Central Committee of the Metalworker's trade union in Moscow, described the same events with similar pride, but shifted focus from the spatial context to the sectoral one – he highlighted that

[28] Gruzdev 1926, p. 36.

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metalworkers were the catalyst for the uprising.[29] The workers of the Putilov factory were Petersburgers and, at the same time, metalworkers, but they played different roles in the narratives of different trade unions – their residence was much more important for the Leningrad council, while the Metalworker's trade union focused on their profession.

Istproftran – the Istprof of the Railwaymen Trade Union – provides us with one of the most interesting cases showing the complexity of the relations between the spatial and sectoral scales of the professional movement narrative. Istproftran was one of the most active commissions and started to release books even earlier than the central commission of the network, the Istprof of the VTsSPS. Furthermore, local branches of Istproftran had always avoided active participation in the activities of the Istprofs of the governorate councils, being tightly bound to their central commission in Moscow. This shift from spatial hierarchies to sectoral ones might be explained by the specific structure of the Railwaymen Trade Union, as it was divided not into governorate branches like other trade unions, but rather into certain rail lines. These lines usually passed through the territories of several governorates, and the Istproftran network did not match the network of the governorate councils of trade unions. Railwaymen hardly ever placed anything in the collections of materials published by trade unions councils and only occasionally participated in the councils' meetings.

Unlike Istprofs, the Commission on the History of Labor in Russia did not have a strong connection to the Petrograd Council of Trade Unions, the trade union organization that funded it. The crucial difference lay in the membership profiles of these commissions. Whereas Istprofs consisted of trade unionists (many of whom were former workers), the Commission on the History of Labor was headed by one of the leading figures of Russian historiography, Professor Sergei Platonov of the Academy of Science. Although direct administration was conducted by less eminent historians and economists of that time (Yulii Gessen, Iosif Kulisher, Alexander Presniakov, and others), they were also members of the Petrograd academic community. The only representative of the Petrograd trade unions among the members of the commission was Grigorii Tsiperovich – one of the leaders of the

[29] Bulkin 1924, p. 6.

Petrograd trade unions council who held several more appointments in the Soviet and the Party offices and thus could not participate in the commission activities.

The agenda of the Commission on the History of Labor in Russia included the processing of recently unveiled archives and the compilation of catalogues of literature and materials containing data on the history of labor. In addition, the commission published a journal *Archive of the History of Labor in Russia* that was enthusiastically received among professional historians. In 1921, the commission joined the Council of Scientific Institutions and Institutions of Higher Education that was founded in 1918 and united the most influential educational and academic institutions of Petrograd, the former capital of Russia.[30]

Despite this almost purely academic format, the Commission on the History of Labor in Russia did not avoid setting practical goals for their activities. In the programmatic article, "The History of Labor and its Significance" in the first volume of the *Archive* Evgenii Tarle wrote:

> It is impossible to glean in any historical field as many lessons are to be found in the field of economic history. Economy depends so little on the individual: on the contrary, people with their so-called "free will" depend so much upon economic evolution, and the laws and character of this evolution are still so mysterious and barely perceptible that, of course, it is only through a clear understanding of the past that we can hope to obtain at least some sort of guiding threads for the future.[31]

Tarle promised the policy-makers that studying the history of labor would provide them with guiding threads, in other words, he claimed that history might have a forecasting power. This was an ambitious declaration, but it was not particularly connected to the agenda of the trade unions.

In 1921–1924, the network of Istprofs co-existed with the Commission on the History of Labor in Russia. The latter was considered as

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[30] This issue was discussed during the meeting of the Commission on the History of Labor in Russia on 25 October 1921. The Central State Archive of St. Petersburg, f. 6276 *Leningradskii Oblastnoi Sovet Professional'nykh Soiuzov*, 1917–n.v., op. 46, d. 1, l. 74.

[31] Tarle 1921, p. 8.

an appropriate substitute for an Istprof commission in Petrograd until 1924. However, since the central Istprof had to collect local materials and form them into the All-Russian history of the professional movement, it was difficult to avoid the use of materials from Petrograd – the former capital of Russia, where many important revolutionary events had happened. In 1923, the central Istprof member V. Lifshits was sent to Petrograd to familiarize himself with the work of the Commission on the History of Labor in Russia and to try to come into contact with it in order to combine both commissions' activities. However, Lifshits reported that the Commission on the History of Labor could not be a substitute for the Istprof in Petrograd, because its members "delved into the remote ages" and did not process the archives of trade unions. The Istprof of the VTsSPS meeting took the decision to try to lead the Petrograd council to allocate several trade unionists for Istprof activities.[32] By the end of 1923, the commission was finally organized. In 1924, both Istprof and the Commission on the History of Labor in Russia co-existed, but soon Istprof remained the only historical commission in the structure of the Leningrad trade unions council.

4. Istprofs and forms of scientificity in history-writing in the Soviet Union in the 1920s

The Istprofs consisted of trade unionists, not professional historians. While they lacked theoretical training, they operated within a scientific framework shaped by a mixture of positivist and Marxist ideas. This amalgam included attitudes such as emphasis on archival documents rather than on memoirs, cumulativeness, collectivity, objectivity, and the impossibility of interpreting events of the near past. I will focus on one crucial idea – that the analysis of history should be preceded by a long period of accumulating facts. And the Istprofs' aim was articulated as preparation materials for the "future historian". But first I will show the wider contexts in which this form of scientificity evolved.

In the two works on Lenin's use of history with which this paper began, both Georgii Lelevich and Vadim Bystrianskii celebrated Lenin's use of history, but, despite the similarity of their accounts, they were

[32] GARF, f. 6935, op. 1, d. 11, l. 1.

significantly different. Bystrianskii and Lelevich explained in two quite divergent ways the question of why Lenin had been so successful using history, of what in particular had made him both a brilliant historian and an auspicious politician. Lelevich, on the one hand, induced "lovers of head-spinning hypotheses and aprioristic generalizations" to learn from Lenin how to fastidiously analyze "raw factual material" and base all their conclusions on it.[33] Bystrianskii, on the other hand, wrote that Lenin did what no regular historian could do – he oriented himself in "contemporary history", when there was not and could not be "evident factual material" such as statistics or archival documents, which could allow a historian to unveil the "genuine face" of the past. Nonetheless, Lenin correctly treated contemporary history, because he was a consummate Marxist and "Marxism is historical in its essence", and this theory explained the main pathways of history, which was much more important than the accumulation of facts.[34]

What is the basis for truly scientific knowledge – reliance on solid facts or having a correct view of the whole? This was an open question for Soviet Marxist historians in the 1920s. In *Anti-Dühring* (1877), a widely read text in that period, Friedrich Engels, on the one hand, wrote that the extraction of any fact from its "natural or historical connection" was an artificial operation; dialectical method implied a view of the world as an entire whole.[35] On the other hand, Engels claimed that the cognitive decomposition of nature into separate parts had allowed science to reach the "colossal achievements" in modern times. It had been a reliance on "real facts" that had made Marx's theory a scientific socialism, not a utopian one. Marx's ideas and implications were not the results of his fantasy, his personal experience or peculiarities of his character, but necessary conclusions arising from the very facts of social and natural life.[36] For Engels analysis and synthesis were two separate stages of cognition, and analysis should precede synthesis, i.e. the overall picture must be derived from individual facts. Elaborating his idea in the brochure *Socialism: Utopian and Scientific* (1880) he wrote: "A certain amount of natural and historical material must be collected

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[33] Lelevich 1925, pp. 47–48.
[34] Bystrianskii 1925, p. 7.
[35] Engels 1987 (1877), p. 22.
[36] Engels 1987 (1877), pp. 592–593.

before there can be any critical analysis, comparison, and arrangement in classes, orders, and species".[37]

This contradiction between the inductive approach, which took the individual facts as the basis of knowledge, and deduction, which saw the particular only through a common picture, had a tremendous impact on the formation of the Soviet revolutionary narrative. Both of these attitudes were quite legitimate in 1920s, as both could be viewed as Marxist and scientific.

Giving the first lecture of his course on Russian historiography at the Petrograd Communist University in 1923, Mikhail Pokrovskii, the most influential Marxist historian of that time in the Soviet Russia, exhorted his students that non-Marxist historical works should not be used even to seek ordinary facts, because what they would find there (even if supported by references to authentic documents) were not facts, but

> ideology, i.e. the reflection of facts. All ideologies are made from pieces of reality, an absolutely fantastic ideology does not exist, and yet every ideology is a distorting mirror, which gives us everything but the true picture of reality.[38]

Indeed, not only was secondary literature distorted, but primary sources were too. Pokrovskii said that historians always see the past with the ruling classes' eyes, and in order to catch sight of the suppressed they must be able to make an allowance for the optics of primary sources. Facts for Pokrovskii were not the basis of theory, nor did they form a general picture of the past, because they were deflected by ideological lenses.

Nonetheless, "ideology" for Pokrovskii was not a rigorous term. Although while arguing against "bourgeois historians" he called their ideology a "distorting mirror", he does not seem to consider ideology as only meaning false consciousness, because in other writings he discusses the "ideology of Marx".[39] So for Pokrovskii ideology was not only a "distorting mirror", but also could be a "magnifying glass", a tool allowing one to see what would be otherwise hidden. Facts were always refracted through an ideological lens, but they could be deflected both

[37] Engels 1989 (1880), p. 299.
[38] Pokrovskii 2012 (1933)a, p. 10.
[39] Pokrovskii 2012 (1933)b, p. 98.

in a right and wrong way, and it was a historians' primary goal to focus their lens to make the class struggle visible.

For Istprof members, the approach to the problem of the ideological distortion of facts was quite different. One of the leaders of the commission, Vasilii Iarotskii, argued that the main problem of the historiography of the professional movement was that authors approached material "not as historians, but as people practicing politics". For example, he treated the book *The Professional Movement in Russia* by the Menshevik Viktor Grinevich (who had held the chair of the Central Committee of Trade Unions in 1917) as nothing but a justification for his own political mistakes. Iarotskii wrote,

> And instead of an analysis of the role of circumstance in the class struggle, in this work we have… a political pamphlet.[40]

It is important to emphasize that Iarotskii criticized not so much Grinevich's Menshevist ideology, but rather his involvement of a political agenda in the theme being studied. Unlike Pokrovskii, Iarotskii's aim was not to focus an ideological lens correctly, but to eliminate ideology from research altogether.

These attitudes of the Istprof members meant that it seemed necessary for them to divide the research process into two separate stages: 1) the accumulation of an exhaustive set of primary sources alongside an ascertainment of the facts and 2) the analysis and explanation of materials. The editors of the main organ of the Istprof network *Materials for the History of the Professional Movement* (1924) wrote in the introduction to the first volume:

> The materials necessary for the study of the issue [of the history of the professional movement] are not yet available to researchers, and the implementation of their broad literary plans should be preceded by long, painstaking and systematic work of collecting and studying the primary sources, as only they can serve as a basis for the research of the said issue.[41]

The statement that the primary aim of the Istprofs was the accumulation of sources for the "future historian" was repeated in most

[40] Chekin (Iarotskii) 1924, p. 11.
[41] [N.N.2] 1924a, p. 4.

books, brochures and documents of the commissions. For that purpose, archives of the trade unions, factories and pre-revolutionary state departments were collected and systematized. Istprofs composed card catalogues of the relevant sources and secondary literature and published great numbers of documents. Iurii Milonov, one of the leading figures of the central Istprof and the author of its main manifestos, highlighted that the truly scientific historical analysis must be based on a vast amount of materials, thus their collecting and publishing became the "center of gravity" of the commission's activities.[42]

The correspondence between Istprof activists gives us further insight into the approach that the commissions tried to follow. Evsei Shatan, secretary of the Istprof of the Ukrainian Council of Trade Unions, sent his article on the history of the professional movement of the Donbass coal miners to R. Iakub, a member of the Istprof of the VTsSPS, in June 1925. In the cover letter, he explained the reasons for the delay and the inaccuracies in his work and reminded the recipient about the honorarium he expected to receive for his article. Shatan also wrote: "I kept in mind your requirements – to follow the method *exposer*, not *proposer* and to try to provide more original documentary material".[43] The French words *exposer* (to expose) and *proposer* (to propose) were written by hand and inserted into the gaps in the original typescript in Russian. Authors of the Istprof editions were instructed to "expose" the facts in their articles, to accumulate materials instead of "proposing" their views. In his reply, Iakub criticized Shatan's article for its length and inaccuracy and informed him that it was finally approved for publication in the fourth volume of the *Materials for the History of the Professional Movement*,[44] yet several sections of the article were cut as they contained data that was already known.[45]

Initially, when Istprof authors wrote about the "future historian", they seemed to refer to distant communist future. However, after accumulating materials for several years, Milonov and his colleagues considered themselves ready to start writing history of the professional movement in Russia. In 1928, participants of the All-Union Conference

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42 Milonov 1924, pp. 20–21.

43 GARF, f. 6935, op. 1, d. 50, l. 157.

44 GARF, f. 6935, op. 1, d. 50, l. 166.

45 Osipov (Shatan) 1925. This article was almost one-third contained of the citations from archival materials.

of Istprofs (May–June 1928) recognized that there were enough materials published and collected in Istprofs' archives to start the preparation of a "scientific and analytical" history of the trade unions.[46]

After this conference, Istprofs changed their mode of work. Introducing readers to the history of woodworkers' union, for example, Iurii Milonov and M. Rakovskii wrote that they wanted their 1928 book to be more than just a "chronicle of events":

> We considered it necessary to ascertain not simply the consequences of the events of the woodworkers' struggle… but also their relation to each other. We also considered it necessary to investigate the relationship between the woodworkers' movement and the labor movement in general, as well as their dependence on the political and economic situation.[47]

Milonov and Rakovskii not only told the story of the woodworkers' union but also put it in a wider context to explain the ebb and flow of the movement. They sought to identify patterns rather than communicate "pure facts" to the reader.

Istprofs did not have a chance to develop this activity, though. In 1929, the "right opposition" of Nikolai Bukharin, Alexei Rykov, and Mikhail Tomskii, who together championed a moderate program of economic development, lost their struggle to Joseph Stalin. Tomskii stepped down from his position as chairman of the VTsSPS in May 1929. Many of his supporters were purged from leadership positions in the unions, while the press harshly criticized his policies as anti-Bolshevist.[48] Trade unions lost the last vestiges of their independence. They had to stay "closer to the masses" to help the Party to mobilize workers for enthusiastic work. Moreover, funding to the VTsSPS was dramatically decreased.[49] In this context, Istprofs' ambition to write an analytical and scientific history was unachievable; in 1930, its archive and library were transferred to the Communist Academy, and the network of commissions was dissolved.

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46 GARF, f. 6935, op. 1, d. 68, l. 24.

47 Milonov, Rakovskii 1928, p. 5.

48 Nosach, Zvereva 2009, p. 149.

49 [N.N.4] 1930, pp. 129–136.

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5. The publishing strategy of istprofs and possibility of the exhaustive narrative

In this section, I show how the scientificity of Istprofs influenced their publishing strategies. The most important form of publication for Istprofs were "materials" – a genre that was tightly connected with new tendencies in the development of archeography, a subsidiary of the historical discipline. Then, I turn to the connection between materials and the institutionality of Istprofs in order to explain the commissions members' aspiration to accumulate exhaustive materials for the professional movement's narrative.

The objectivist scientificity of Istprof historians implied the accumulation of sources for the "future historian" and such claims made the question of publishing rather problematic. It was necessary to accumulate exhaustive materials for the history of the professional movement and only then to start writing it. The Istprof commissions conference in October 1923 defined the "center of gravity" of their actual works as the "reconstruction of the full and precise picture of particular moments",[50] which did not necessitate publishing anything, but called instead for searching and collecting the documents in the Istprof archives.

Nonetheless, in 1923–1929, the Istprof commissions released several dozen collections of materials, bibliographical guides, textbooks, and articles in the journals of trade union organizations. R. Iakub, a member of the central Istprof, in his report to a collective of his fellows from several other trade unions explained the reasons why the commission did not refrain from publishing activities and was releasing its main organ – the continued edition *Materials for the History of the Professional Movement*. Firstly, it was necessary because the materials collected in the Istprof archive could only be easily used by a few researchers, while many others would not be able to study them. This, he claimed, might lead to a "one-sided historical elaboration of these materials". Secondly, publishing materials supplying readers with new facts might correct the wrong common-sense accounts about seemingly well-known issues in the history of the professional movement even before "fundamental works" were published. And finally, Iakub talked about the *Materials for the History of the Professional Movement* as an organizational center for

[50] GARF, f. 6935, op. 1, d. 11, l. 19.

people who might contribute to Istprof work, but were "out of range of particular Istprofs".[51] He meant here former participants of the professional movement who were not subordinated to the trade unions' hierarchies at that moment, but might be attracted by the opportunity to publish their memoirs or documents from their personal archives in the organ of the VTsSPS. The first and the third reasons did not belong to the content of the published texts as such, but were part of a larger egalitarianism of Istprofs' goals as it attempted to organize a large number of supporters around the commissions' activities. The second point of Iakub's account explained why the facts themselves – still particular, but already reliable – were to be circulated among ordinary trade unionists: even though these facts were not enough to construct an exhaustive and scientific narrative, they carried a destructive charge that undermined unreliable existing narratives.

The way in which Istprof members defined the purposes of the commission influenced the choice of genres for its publications. The most important of them was the genre of historical materials, i.e. re--publication of historical documents. Materials never implied publishing raw documents, but rather required their selection, editing, and annotation. In this respect, historical materials were very much like physical materials such as fabric or bricks, which were already a product of the manufacturing, but not an off-the-shelf item – a dress or a building. Historical materials were some sort of semi-finished historical research, intermediates for the "future historian".

Istprofs were not pioneers in publishing historical materials; indeed this genre had a rich history in Russian literature long before 1917. Ideas regarding the necessity of a mediator between primary sources and historians began developing in Russia in the first half of the 19th century with the emergence of archeography – an auxiliary historical discipline that elaborated rules on the publishing of historical documents. The institutionalization of this discipline was associated with the initiation of the Archeographical Commission attached to the Ministry of National Education in 1834; this commission aimed to search, accumulate and publish "antiquities" – documents of the Medieval and the Early Modern history of Russia. The commission continued its activities after

[51] GARF, f. 6935, op. 1, d. 49, l. 24.

the revolution of 1917 and the discipline continued to evolve by using newly uncovered archival documents from the 19th–20th centuries in a new institutional context: due to a decree of the Council of People's Commissars in 1918, the State archival fund was established that was to unite all prerevolutionary archives into a single system.

At the First Congress of Archivists that took place in Moscow on March 18, 1925, Sigizmund Valk, a well-known archeographer from Leningrad, gave a talk about the specificity of historical revolutionary documents and the modalities of their publication. He said that in publishing materials one must, firstly, aim as closely as possible for the authentic reproduction of the original, and, secondly, undertake editorial revision according to "scientific standards" to enable the reader to understand the correct meaning of the document. Valk emphasized that "photographically precise" reprints of the original were not an appropriate solution, because they would only serve the first purpose. Documents bore many mistakes, discrepancies and features obscure to the modern reader. Moreover, there might exist several manuscripts and several editions of the original, which made the question of authenticity even more complex. Therefore, the publication of any document had to go through a juxtaposition of editions and a procedure of emendation – the correction of discrepancies of the text. However, the procedure of emendation had to be distinctively limited: an archeographer's competence embraced nothing but the formal aspects of the document, such as transliteration from the old, prerevolutionary spelling to the new, corrections of misprints, words agreement and so on.

This contradiction between authenticity and the readability of published documents raised heated discussions among the participants of the congress. Valk himself demonstrated the complexity of the issue through Lenin's manuscripts. Even before the reforms of Russian orthography in 1918, Lenin wrote without the character Ъ – a "hard sign" that was normally written at the end of a word when following a non-palatal consonant, even though it had no effect on pronunciation. If the transliteration from the old spelling to the new one was set as the common rule for publishing all prerevolutionary materials, readers would not be able to catch a glimpse of this remarkable feature of Lenin's style. Mikhail Pokrovskii, who also participated in the congress of archival workers, argued that a potential reader of materials would not always be interested in the style of the author of the document. He provided a counter-example: bills released by the 18th century rebel peasant army of Emel'ian Pugachev. They also bore a particular "style", yet it was nothing but the illiteracy of the humble scribe, which might not be interesting in itself. In that case, Pokrovskii argued, the content of document was much more important than its style.[52]

This discussion led participants of the congress to an important idea, namely that the modalities of material publication depended on the aims and interests of the potential audience. Researchers might use the same document to answer different questions: whilst one would look for traces of the development of the author's personality and ideas – which would require the publication of earlier and later drafts – the other would use the same document to write the history of social movements and the only relevant version of the document for them would be the one that was promulgated and had an impact on social relations. After the discussion, the participants of the congress carried the resolution to elaborate unified and universal rules for the publication of documents that would be a compromise between agendas of different researchers. However, these rules only appeared ten years later – in 1935[53] – and were severely criticized by Sigizmund Valk.[54] Professional archivists and archeographers could not compromise on a neutral way of publishing documents. Through publication they were converted into materials, which necessarily implied an image of the future research product.

Iurii Milonov and M. N. Zayats, members of the central Istprof, also participated in the First Congress of the Archival Workers in 1925 and attended the panel with Valk's presentation on publishing of archival documents. However, they did not have a say in this discussion. For them, the impossibility of publishing materials that were neutral and appropriate for any reader was not a problem. This lack of concern might be explained by the particular institutionality of Istprof. Being incorporated into a non-academic structure, it had particular and articulated aims. The "future historian" mentioned in the texts of Istprof was much more unambiguous than the abstract "researchers" whose agendas the participants of the archival congress of 1925 tried to guess.

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52 Valk 1926.

53 Sergeev 1935.

54 The review was written right after the publishing of the rules, but was published only in 1991. Valk 1991.

In the introduction to the first collection of their *Materials for the History of the Professional Movement* (1924), Istprof leaders wrote that as a result of their work there would be

> the emergence of a number of scientific-literary works and in particular – a whole history of the professional movement in Russia.[55]

The "future historian" was to write the concrete revolutionary history of the professional movement and trade unions, and Istprof had to provide exhaustive materials for it. This idea about the possibility of an exhaustive narrative stemmed from the particular institutionality of Istprof. Trade unions had a complex, but certain structure that preset the matrix for historical narratives. The general history of the professional movement narrative consisted of histories of particular industrial trade unions (of railwaymen, textilemen, metalworkers etc.) and of histories of particular spatial trade union organizations (republican, governorate, and municipal councils). Although this led to tensions between spatial and industrial scales in the narrative, as I described above, the very presence of the institutional matrix meant that the exhaustive narrative could be written – it required writing the history of each particular trade union on each particular territory.

However, the institutional structure of trade unions preset the matrix only for the synchronic dimension of the narrative. The diachronic scale of the trade unions history had no such univocal segmentation of the research topic. The main problem lay in the origins of the Russian trade unions, which appeared well-developed – in the words of trade unionist Reznikov – as a *deus ex machina*[56] riding on the wave of the revolutionary movements of the 1905. They could not have just arisen from nowhere, and Istprof had to find the transitional forms that preceded the trade unions.

With that problem in mind, the Istprof of the VTsSPS organized special discussions about the origins of the Russian professional movement on February 14 and 28, 1924. Vasilii Iarotskii, a keynote speaker of the discussion, offered to consider the mutual benefit societies that

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55 [N.N.2] 1924a, p. 5.

56 [N.N.3] 1924b, p. 18.

had legally existed from the middle of the 19th century as the origin of trade unionism in Russia. He argued that although these societies had had no revolutionary potential, they allowed workers to realize their unity, they converted class-in-itself into class-for-itself.[57] Most of the participants of the discussions did not agree with Iarotskii's account. Among other objections that were articulated, R. Iakub suggested that the origin of trade unionism might be found in the economic struggle of workers: they usually organized special temporary committees during strikes to support each other.[58] For Iakub, trade unions were a permanent and well-developed form of strike committees. David Riazanov, an eminent Bolshevik intellectual and leader of the Marx-Engels Institute, claimed that trade unions developed from the revolutionary movement of the proletariat and from its main organization – the Social-Democratic Party.[59] Several more opinions were expressed, but members of the discussions did not come to an agreement.

It is important to highlight that this discussion did not raise questions about the *causes* for the rise of trade unionism in Russia – this was to be done by the "future historian". Istprof members were simply trying to reduce the origins of the Russian trade unionism to a particular organizational form, be it the legal mutual benefit societies, the strike committees, or the Social-Democratic Party. Istprof members tried to avoid the mistake that Marc Bloch twenty years later would call "the idol of origins": historians' tendency to confuse two different categories – causes and beginnings. They explained phenomena with their beginnings instead of paying attention to actual causes that lay both in the past and in the present.[60] Istprof members, on the other hand, discussed only the beginnings of the trade unions – preexisting organizational forms without raising the question of the causes of this phenomenon.

Istprof members' idea that the subject of their research had an exhaustible nature and that the history of the professional movement consisted of a countable number of elements had an important implication: this attitude eliminated any hierarchy in the narrative. If the

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[57] Chekin (Iarotskii) 1924.

[58] Iakub evolved his response to Iarotskii's account into a long article. See: Iakub 1924.

[59] [N.N.3] 1924b, pp. 23–27.

[60] Bloch 1992 (1941), pp. 24–27.

aim was to describe *all* the events of the professional movement, there was no need to divide them into important and secondary ones, because eventually they all were to be described. Particular events, facts, and characters had to be exposed, not located on the coordinate system. For example, in the collection of materials published by the Istprof of the Nizhny Novgorod Council of the trade unions, Alexander Belozerov wrote about professional movement in the region in 1905–1910. Seeking to write an exhaustive narrative, Belozerov described all the professional organizations he had information about. One of these organizations – the Union of Pharmacists – was far from being a proletarian and revolutionary. Although this union was founded earlier than most of other organizations, it did not play any role in the professional movement of the region remaining nothing but a local and separate community. In spite of this, Belozerov wrote: "Let us be objective and tell all that we have in our materials about this organization".[61] Pharmacists were included in the history of the professional movement in Nizhny Novgorod only in order to make it exhaustive.

6. Conclusion

The Istprofs' "future historian" was not entirely an imagined figure. One of the crucial goals of Bolshevik cultural policies in the 1920s was a training of a generation to succeed the party leadership as well as "red specialists" who would combine loyalty to the regime with high qualifications.[62] The recent graduates of Sverdlov Communist University, the Communist Academy, and the Institute of Red Professors played a pivotal role in Stalin's Great Break campaigns at the turn of the 1930s, yet many of them were later purged amid the Great Terror of 1936–1938.[63]

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[61] Belozerov, p. 217.

[62] See: David-Fox 1997.

[63] For instance, Mikhail Pokrovskii's students from the Institute of Red Professors helped their professor to pave the way for the infamous "Academic affair" of 1929–1930 which resulted in widespread repressions against the old academic establishment. Such members of Pokrovskii's school as Grigorii Zaidel', Semen Tomsinskii, and Grigorii Fridliand, however, were also purged and sentenced to death in 1937–1938. See: Perchenok 1995.

The "future historians" eventually emerged, but instead of using the materials carefully prepared by Istprofs, they dismissed them out of hand. The dissolution of Istprofs in 1930 was followed by a special conference organized at the Communist Academy, which inherited the abundant archives of the union historical commissions and employed a share of Istprofs' staff. The keynote address was delivered by Efim Mil'shtein, a former worker-typesetter who joined the Bolshevik party in 1920, graduated from Moscow State University in 1926, and entered the PhD program in history at the elite Institute of Red Professors in 1929.[64] In his speech, Mil'shtein criticized the Iurii Milonov's idea that definitive arguments could be made only about the events of the distant past because authors who wrote about recent history could not help but replace the "real meaning" of facts with their own "desirable interpretations." For Mil'shtein, the Istprofs' inductivism was a symptom of methodological eclecticism and a lack of party-mindedness.[65] As he put it,

> This led to the tenet of the veracity of facts communicated by […] political enemies of the Soviet power. In thrall to bourgeois historical methodology, comrade Milonov forgot that those facts, gleaned in a particular manner, were the *ideology* hostile to the proletariat.[66]

A doctoral student with Mikhail Pokrovskii at the Institute of Red Professors, Mil'shtein echoed here his professor's idea that there were no "pure" facts because they were always reflected through ideological lenses.

The dissolution of Istprofs was doubtless a part of the wider political campaigns in Stalin's Great Break and in the struggle against the "right opposition" in particular. Yet this external political context did not negate the intellectual meaning of the methodological and theoretical

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[64] Dolgova 2018, p. 913.

[65] Vladimir Lenin's concept of *partiinost'*, which is usually translated as partyness or party-mindedness, implied that there was no such thing as neutral social theory. As class conflict was the backbone of social life, historical materialists must be able to recognize this conflict and openly adopt the standpoint of a particular social group. By the end of the 1920s, this concept had been diluted, and in the Stalinist Soviet Union *partiinost'* meant that scientific knowledge was irrelevant if it did not serve the regime's objectives. See: Barber 1979; Joravsky 2013, pp. 24–44.

[66] [N.N.5] 1932, p. 10.

discussions among trade unionists and Bolsheviks, historians and archeographers, or amateurs and professionals.

By moving away from the dominant paradigm of political pragmatics through which the Soviet official history-writing is routinely perceived and narrated, we can better appreciate the complicated contexts of this topic. In this paper, I aspired to demonstrate how the scientificity of the Soviet regime shaped the intellectual and institutional frameworks of the Soviet history-writing in the 1920s. Similar to other social and natural sciences, history moved beyond the borders of academy at that period, but this process was institutionalized in the form of citizen science rather than applied research. Against the background of the abortive experience of the engaging with the professional historians, trade unionists themselves utilized scientific methods to write their history. Funded by and formally subordinated to the Petrograd Council of Trade Unions, the Commission on the History of Labor in Russia consisted of well-known and experienced professional scholars who addressed their work to the academic community and promised nothing but obscure "guiding threads for the future" to policymakers. The Istprofs, on the other hand, were tightly bound to trade union organizations, not only providing them with historical narratives, but also accomplishing routine functions. Nevertheless, Istprofs remained connected with the scientific sphere too, but in quite a different manner: its members were not consummate historians, but they used methods and procedures that they considered to be scientific.

The basic tenet of the scientificity of the Istprofs did not coincide with the Soviet historiographical mainstream of the 1920s, i.e. Pokrovskii's claims that it was not possible to avoid the ideological lens in the historical research. Iurii Milonov and his colleagues, on the contrary, clearly differentiated the process of accumulation of "pure facts" and their analysis and evaluation. Because the Marxist canon was still flexible in the 1920s, the Istprofs' inductive form of scientificity was also considered as Marxist and legitimate up until 1930.

The juxtaposition of the Istprofs' publishing strategies with practices of archeography, an auxiliary discipline that worked on the theory of historical documents publication, shows how the incorporation of Istprof into a non-academic structure influenced the narrative of professional movement. Archeographers in the middle of the 1920s tried to find a way to publish documents appropriate for *any* researcher, but did not succeed.

Istprof members were free from this problem, because they prepared materials for the concrete narrative: the institutional history of trade unions.

Claiming the scientificity of Istprof, I do not mean that it was "good" as opposed to "bad" Party history-writing. Neither have I sought to evaluate a distance between Istprof and "real" historical science. My primary task in this paper has been to show that Soviet official history-writing was far from being a mere tool of political struggle, but was embodied into particular ideological, institutional and intellectual traditions, which framed the new authorities' way of constructing the historical narrative. They could not just tell an invented story about themselves, but they had to tell it in a specific – scientific – way to prove its relevance and legitimacy.

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Trójwymiarowe modele matematyczne na przykładzie obiektów ze zbiorów Muzeum Uniwersytetu Jagiellońskiego

Abstrakt

W artykule podajemy przykłady modeli matematycznych, obecnie niemal zapomnianych, które jeszcze kilkadziesiąt lat temu odgrywały wielką rolę w dydaktyce matematyki. Z końcem XIX wieku

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powstała prężna produkcja tych modeli na użytek szkół i uczelni. W Muzeum UJ zachowały się w doskonałym stanie trzy takie modele.

Słowa kluczowe: *modele matematyczne*, *Felix Klein*, *dydaktyka matematyki*, *Muzeum UJ*

Three-dimensional mathematical models illustrated by objects from the collections of the Jagiellonian University Museum

Abstract

This paper presents examples of mathematical models which have almost passed into oblivion, yet a few decades ago still played a significant role in the teaching of mathematics. In the late nineteenth century such devices started to be produced on a large scale for schools and universities. The Jagiellonian University Museum has three such models in perfect condition in its collections.

Keywords: *mathematical models*, *Felix Klein*, *didactics of mathematics*, *Museum of the Jagiellonian University*

1. Wizualizacja wiedzy – motywacje dydaktyczne

Trafna wizualizacja matematycznych struktur, głównie funkcji jednej lub dwu zmiennych, jest ważnym zadaniem w procesie dydaktycznym. Podobnie jest, gdy chodzi o rozmaite struktury chemiczne lub fizyczne, które nie są bezpośrednio dostępne dla wzroku, ale które można opisać matematycznie. Wspomniana wizualizacja to problem ważny z punktu widzenia dydaktyki i nadal rozwijany.

Przykład z nauczania początkowego: Wynalezione stosunkowo późno (1945 r.) tzw. klocki Cuisenaire'a, zwane również *kolorowymi liczbami* lub *liczbami w kolorach*, to przykład prostej, ale skutecznej i wciąż popularnej wizualizacji dotyczącej arytmetyki elementarnej[1]. Jest oczywiste,

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[1] W Polsce zwolennikiem tej metody był pedagog Henryk Moroz (ur. 1924), który opracował własny układ klocków zatwierdzony później przez Ministerstwo Oświaty.

że nieodpowiedzialnie prowadzone nauczanie może nieodwracalnie zrazić młodych ludzi do matematyki jako dziedziny rzekomo „trudnej", „nudnej" lub „niehumanistycznej". Stąd dramatyczne i raczej retoryczne pytanie postawione przez cenionego amerykańskiego popularyzatora matematyki Williama Dunhama (ur. 1947):

> Dwie częste i typowe reakcje, gdy ktoś spotka matematyka: „Nie znoszę matematyki" lub: „Boję się matematyki". […] Dlaczego tak wielu ludziom matematyka kojarzy się z operacją chirurgiczną bez znieczulenia? Czyżby, jako dzieci, byli torturowani przez matematyka-sadystę?[2].

Były w historii dydaktyki matematyki chybione trendy dydaktyczne preferujące, w dobrej wierze – w imię czystej abstrakcji i szacunku dla precyzji – porzucenie przystępnych i poglądowych metod wizualizacji jako „nieścisłych" i uwłaczających godności królowej nauk. Prowadziły one czasem do kuriozalnych sytuacji. Przykładem tego jest przypadek dotyczący wybitnego matematyka francuskiego, członka sławnej grupy N. Bourbakiego[3] – Claude'a Chevalleya (1909–1984), który, właśnie w imię swoiście rozumianej czystości matematycznej, nie zalecał wykonywania nawet poglądowych rysunków. Chevalley

> był zdecydowanie przeciwny używaniu rysunków, i to nawet w rozumowaniach geometrycznych. Pewnego razu wygłaszał bardzo abstrakcyjny wykład i w pewnej chwili

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[2] Dunham 1994, s. 164.

[3] Grupa wybitnych matematyków francuskich działających pod pseudonimem Nicolas Bourbaki preferowała metody czysto abstrakcyjne. Są autorami serii nieprzystępnych podręczników, odrzuconych przez fizyków matematycznych. W dydaktyce oficjalnie zalecano odejście od starych i sprawdzonych podręczników np. Édouarda Goursata w imię programu tzw. *New Math*. Podejście to ostro skrytykował matematyk rosyjski, pracujący pod koniec życia we Francji, Władimir Igoriewicz Arnold (1937–2010) w znanym eseju *O nauczaniu matematyki* (Arnold 2001).

Program bourbakistów postulował rewolucję w spojrzeniu na matematykę w ogóle, stawiając na pierwszym miejscu kwestię struktury i hierarchii pojęć. W założeniu nie dotyczył dydaktyki matematyki, chociaż z programu bourbakistów „nadgorliwcy" wysnuwali pewne wskazania także dla dydaktyki matematyki.Inaczej mówiąc, owe chybione trendy dydaktyczne były zawinione nie tyle przez samych bourbakistów, ile raczej przez ich interpretatorów. Za zwrócenie uwagi na tę kwestię dziękuję [K.M.] jednemu z Recenzentów.

> po prostu pogubił się. Po chwili namysłu odwrócił się do tablicy i – starając się, żeby nikt nie widział tego, co tam robi – dyskretnie narysował mały diagram, przyjrzał mu się, szybko go zmazał, po czym ponownie zwrócił się do słuchaczy i kontynuował wykład[4].

Anegdota ta pokazuje, że nawet wybitny matematyk może odnieść korzyść z trafnej wizualizacji i nie powinien nią gardzić.

2. Obrazy w fizyce, chemii i matematyce

Z kolei w chemii popularne są poglądowe modele przestrzenne związków chemicznych jako kolorowych kulek (czasem jeszcze z literowymi oznaczeniami pierwiastków z tablicy Mendelejewa) odpowiednio łączonych patyczkami – wiązaniami chemicznymi (Ryc. 1, 2). Z punktu widzenia aktualnej teorii, tj. opartej na równaniu Schrödingera chemii kwantowej, niewiele mają one wspólnego z rzeczywistością. Jednak pozwalają zrozumieć i skutecznie zapamiętać pewne prawidłowości. Oczywiście, początkującym adeptom chemii zawsze trzeba przypominać, że takich modeli nie należy brać dosłownie. Jednak tłumaczenie wiązań chemicznych, na wstępnym etapie nauki, jako abstrakcyjnej chmury prawdopodobieństwa opisanej zespoloną funkcją falową, tj. rozwiązaniem równania różniczkowego Schrödingera – byłoby z góry skazane na dydaktyczną porażkę.

Można też podać przykład bardziej wyrafinowanej metody wizualizacji, która jednocześnie ma pewną głęboką interpretację teoretyczną. Są to tzw. diagramy Feynmana (Ryc. 3) wymyślone przez wybitnego fizyka amerykańskiego Richarda Feynmana (1918–1988), używane w kwantowej teorii pola – wyjątkowo trafna wizualizacja pewnych (z natury niedających się wyobrazić) procesów kwantowych, która w dodatku radykalnie ułatwia oraz porządkuje pewne bardzo zawiłe rachunki (1948 r.). Podobno sam Feynman, znakomity fizyk teoretyk i natchniony wykładowca, ilustrował swoje diagramy, używając patyczków.

Przejdźmy do konkretnego przykładu matematycznego, tj. do pojęcia pewnej funkcji zwanej *silnią*. (Polska nazwa jest dość osobliwa; we wszystkich innych językach funkcja ta ma czytelną nazwę pochodzącą

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[4] Cyt. za: Sinclair 2008, s. 236.

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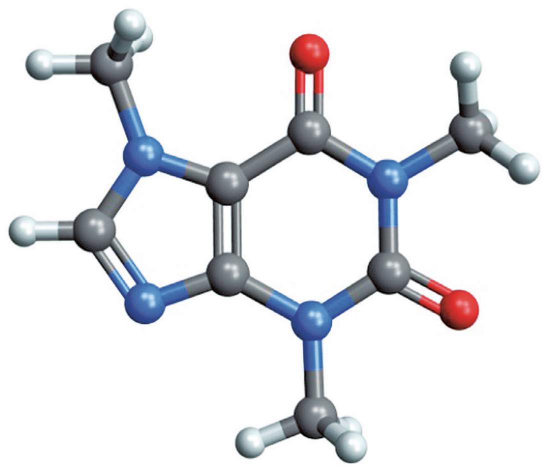
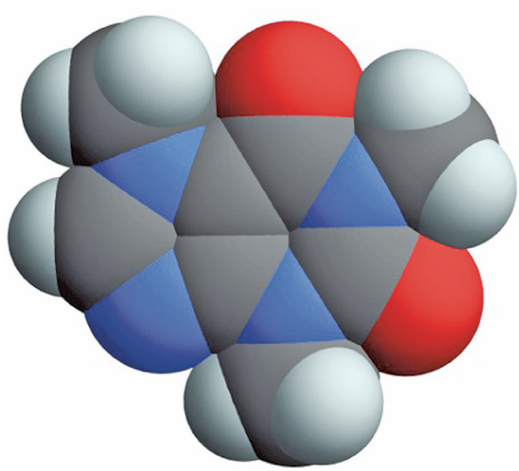




Ryc. 1, 2. Dwa poglądowe modele cząsteczki kofeiny $C_8H_{10}N_4O_2$ (rysunki wykonano korzystając z programu *Mathematica* firmy Wolfram Research)

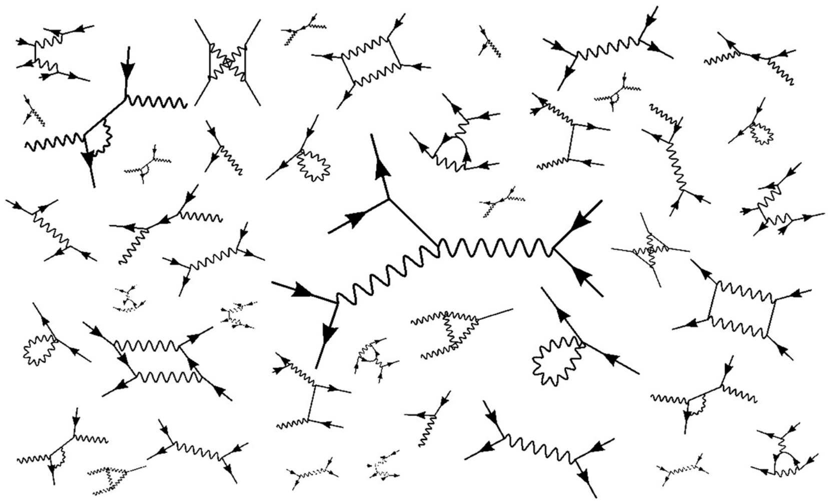
od łacińskiego określenia *factorialis*, która sugeruje mnożenie). Dla liczb naturalnych definicja silni jest bardzo prosta:

$$n! = 1 \cdot 2 \cdot 3 \cdot \ldots \cdot (n-1) \cdot n = \prod_{k=1}^{n} k \tag{1}$$

Nieco mniej intuicyjna jest definicja rekurencyjna:

$$1! = 1 \tag{2}$$

$$n! = (n-1)! \cdot n$$



Ryc. 3. Diagramy Feynmana, z których można obliczyć prawdopodobieństwo zajścia rozmaitych procesów w świecie cząstek elementarnych (rysunek wykonano przy użyciu ogólnodostępnego pakietu xACT)

Matematycy mają, niemal podświadomą, tendencję do uogólnień i formułowania związków, w których zakres zmiennej niezależnej nie jest ograniczony. Wielki matematyk szwajcarski Leonhard Euler (1707–1783) zauważył, że następująca definicja z użyciem całki oznaczonej ma identyczną własność, jak powyższa rekurencja (2):

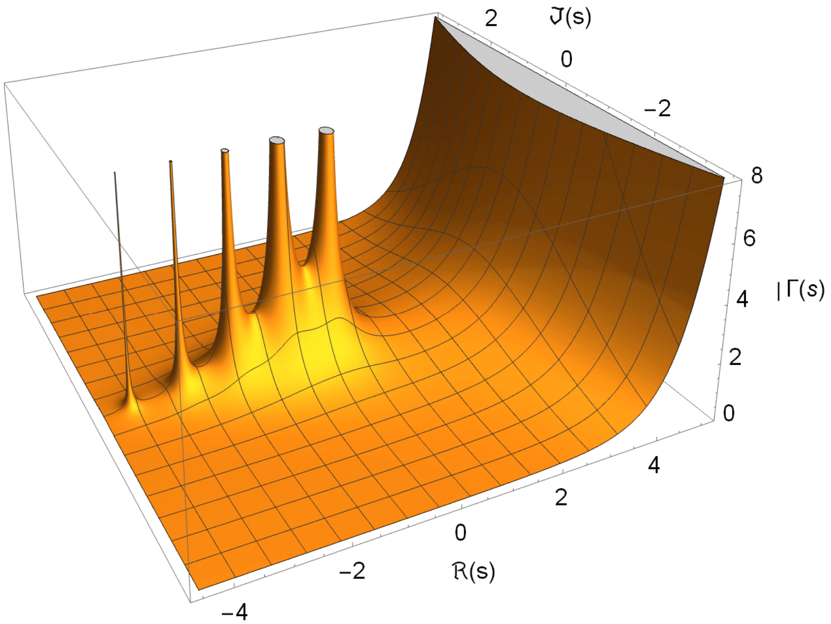
$$x! = \int_0^\infty t^x e^{-t} \mathrm{d}t \qquad (3)$$

$$x! = (x-1)! \cdot x$$

Taka niewątpliwa komplikacja ma jednak nieocenioną zaletę: formuła (3) pozwala nadać sens pojęciu silni dla liczb rzeczywistych x większych od minus jeden, a nawet dla liczb zespolonych s, których część rzeczywista Re s jest większa od minus jeden (Ryc. 4). Oczywiście, oryginalna, prosta definicja (1) zupełnie nie nadaje się do tego. Ścisła procedura tzw. przedłużenia analitycznego pozwala z kolei przypisać jednoznaczne wartości *wszystkim* liczbom zespolonym. W szczególności można uzyskać jednoznaczny i zaskakujący wynik:

$$\left(\frac{1}{2}\right)! = \frac{\sqrt{\pi}}{2} \qquad (4)$$

Żeby zobaczyć pełny wykres funkcji $s!$, która dowolnej liczbie zespolonej (tj. parze liczb rzeczywistych) przypisuje odpowiednią liczbę zespoloną, trzeba by mieć do dyspozycji czterowymiarową tablicę. Tymczasem przestrzeń fizyczna ma (z nieznanych dotąd powodów) tylko trzy wymiary. Trzeba więc pójść na kompromis i wykonać np. wykres wartości bezwzględnej funkcji $s! \equiv \Gamma(s+1) = (x+iy)!$, który parze liczb rzeczywistych przypisze jedną liczbę rzeczywistą, a taki wykres można już narysować. Dzisiaj mamy do dyspozycji szybkie komputery wyposażone w odpowiednie programy (*Mathematica*, *Maple* i in.), które bez trudu rozwiążą takie zadanie.



Ryc. 4. Wykres wartości bezwzględnej funkcji silnia uogólnionej przez Eulera – formułą (3). Zaskakujące są osobliwości widoczne po lewej stronie w postaci serii coraz węższych „kominów" (rysunek wykonany przy użyciu programu *Mathematica* firmy Wolfram Research)

Tymczasem w wieku XIX i do połowy XX, gdy jedyną pomocą rachunkową były tablice matematyczne i suwaki logarytmiczne, sporą popularność przy wizualizacji skomplikowanych funkcji matematycznych zdobyły modele gipsowe, niekiedy z użyciem elementów metalowych i nitek jedwabnych. Niektóre z nich miały elementy ruchome – pozwalały na efektowną animację. Np. obrót zębatego koła o jakiś kąt odpowiadał zmianie pewnego abstrakcyjnego parametru matematycznego.

Staranność i precyzja wykonania tych przyrządów do dziś budzą podziw. Były konsultowane z zawodowymi matematykami, m.in. z twórcą sławnego programu erlangeńskiego (1872 r.) – wciąż aktualnego poglądu na istotę geometrii polegającego na oparciu jej na teorii grup – Felixem Kleinem (1849–1925).

Interesującą dziedziną jest sztuka lub architektura inspirowana przez geometrię tzw. powierzchni minimalnych[5] – dziedzina wciąż aktywnie rozwijana, o czym będzie jeszcze dalej mowa.

3. Od Oliviera do Kleina. Matematycy i artyści

Aby zrozumieć lepiej pojawienie się i rozpowszechnienie w XIX wieku trójwymiarowych modeli abstrakcyjnych obiektów matematycznych, a także powstanie ogólniejszej tendencji do tworzenia modeli materialnych w innych obszarach wiedzy ścisłej, zwłaszcza w fizyce i chemii, należy przyjrzeć się bliżej trendom rozwojowym tego okresu w naukach matematyczno-przyrodniczych, technice, ich edukacji, a także szeroko pojętej komunikacji wizualnej. Jak można się przekonać, wymienione obszary, w wielu przypadkach i przykładach, często spotykały się czy wręcz przecinały.

Korzenie wytwarzania i posługiwania się trójwymiarowymi modelami matematycznymi, jak się wydaje, tkwią w tym okresie rozwoju matematyki, który ściśle wiąże się z powstaniem znaczących europejskich, choć głównie francuskich, ośrodków kształcenia inżynierów wojskowych i cywilnych w drugiej połowie XVIII wieku (z najsłynniejszą École Polytechnique na czele). Podjęty w nich typ kształcenia wymagał od zatrudnianych w tych szkołach nierzadko wybitnych matematyków nie tylko wysokich kwalifikacji naukowo-teoretycznych, ale także umiejętności w posługiwaniu się formami przekazu nauczanych treści, jako że absolwentami mieli być głównie praktycy.

We wspomnianej ogólniejszej perspektywie wyróżniały się dokonania ściśle naukowe, ale i dydaktyczne jednego ze współzałożycieli École Polytechnique – Gasparda Monge'a (1746–1818) i jego uczniów.

[5] Są to powierzchnie, których tzw. średnia krzywizna Gaussa jest w każdym punkcie zero.

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Monge uważany jest powszechnie za twórcę geometrii wykreślnej, a także za pioniera teoretycznych rozwiązań z zakresu geometrii rzutowej i różniczkowej. Jak się okazuje, francuski matematyk nie stronił od wykorzystywania w dydaktyce środków poglądowych uprzystępniających i wyjaśniających wykładane treści. W jednym z katalogów zbioru przyrządów i modeli zgromadzonych w założonej w czasie Rewolucji uczelni Conservatoire National des Arts et Métiers w Paryżu można znaleźć informację o wykonanych przez Monge'a, niestety niezachowanych do dzisiaj, trójwymiarowych modelach nitkowych hiperboloidy[6].

Nietrudno więc zrozumieć, że do wykładów z geometrii wykreślnej w paryskich szkołach technicznych własne modele wykorzystywał później uczeń Monge'a –Théodore Olivier. Znaczna część tych modeli reprezentowała matematyczne powierzchnie prostokreślne. Olivier budował przede wszystkim modele ruchome, przedstawiające zarówno generowanie takich powierzchni, jak i tworzenie krzywych powstałych przez ich przecięcie[7]. Ponieważ wiele modeli skonstruowanych na podstawie pierwotnych rozwiązań Oliviera przetrwało do dnia dzisiejszego w zbiorach muzealnych, obecnie można snuć przypuszczenia, jak dalece mogły one w XIX wieku pobudzać wyobraźnię przestrzenną słuchaczy wykładów z geometrii, zwłaszcza przyszłych inżynierów i architektów[8].

Wraz ze spadkiem prestiżu geometrii wykreślnej we Francji około połowy XIX wieku zmalało także zainteresowanie wśród tamtejszych wytwórców m.in. modelami nitkowymi Oliviera. Tymczasem od lat 60. tego wieku stopniowo wzrastało zainteresowanie trójwymiarowymi modelami obiektów geometrycznych w ówczesnych państwach niemieckich[9]. Z jednej strony tendencja ta pokrywała się z dynamicznym wzrostem, w tym samym okresie, znaczących wyników uzyskiwanych przez tamtejszych matematyków[10]. Z drugiej, szczególnie po

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[6] *Catalogue* 1882, s. 31; por. także: Sattelmacher 2016, ss. 137–138.

[7] Hervé 2007, ss. 295–318. Olivier budował także modele rozmaitych układów mechanicznych z przekładniami zębatymi.

[8] Lorraine J. Daston sugeruje wpływ empirystycznej filozofii Johna Locke'a na francuskich matematyków końca XVIII wieku. Dotyczyłoby to nie tylko źródeł empirycznych wiedzy matematycznej, ale także potrzeby sposobów jej prezentowania w postaci „empirycznych" wizualizacji w dydaktyce geometrii; zob.: Daston 1986, ss. 271––274; także: Friedman 2018, s. 116.

[9] Friedman 2018, s. 118.

[10] Zob. np.: Struik 1960, ss. 250, 299.

wojnie francusko-pruskiej i zjednoczeniu Niemiec, nastąpiło na ich obszarze znaczne ożywienie wytwórczości dydaktycznych przyrządów naukowych, w tym modeli, co pozwoliło na konkurowanie na tym polu z odpowiednikami francuskimi i angielskimi[11]. Wzrosło również zainteresowanie tymi modelami w dydaktyce uczelnianej w innych państwach, zwłaszcza w Stanach Zjednoczonych.

Tendencje do wykorzystywania modeli materialnych w dydaktyce matematyki, jak się okazuje, szły w parze z trendem budowy trójwymiarowych modeli obiektów, stanowiących przedmiot zainteresowania dziewiętnastowiecznej fizyki i chemii. W przypadku tej pierwszej na uwagę zasługują ruchome modele odkrywanych od lat 20. tego wieku zjawisk elektromagnetycznych. Ponieważ opis teoretyczno-matematyczny tych zjawisk, w szczególności w postaci ujęcia maxwellowskiego, daleko odbiegał od możliwości intuicyjnego wyobrażenia, m.in. sam James C. Maxwell (1831–1879), a także Oliver Lodge (1851––1940) i Ludwig Boltzmann (1844–1906) projektowali realizowane później w praktyce modele mechaniczne i hydrodynamiczne elektromagnetyzmu (Ryc. 5)[12]. Niewątpliwe dla tej praktyki były także przyświecające jej pobudki związane z rozpowszechnionym w tym okresie przekonaniem o konieczności „wtłoczenia" nowo odkrywanych zjawisk fizycznych w ramy tradycyjnych ujęć mechanicznych (tzw. mechanicyzm)[13].

Nieco inne przesłanki przyświecały twórcom trójwymiarowych modeli chemicznych w drugiej połowie XIX wieku. Należeli do nich pionierzy stereochemii, w tym Friedrich A. Kekulé (1829–1896), August W. von Hofmann (1818–1892) i Jacobus H. van 't Hoff (1852–1911)[14]. Był to okres jakościowej zmiany: przechodzenia od chemii przemian i stosunków stechiometrycznych do chemii struktur przestrzennych

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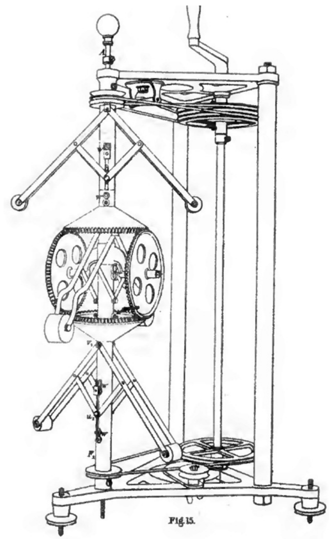
[11] Brenni 2012, s. 202; Sattelmacher 2013, ss. 294–296.

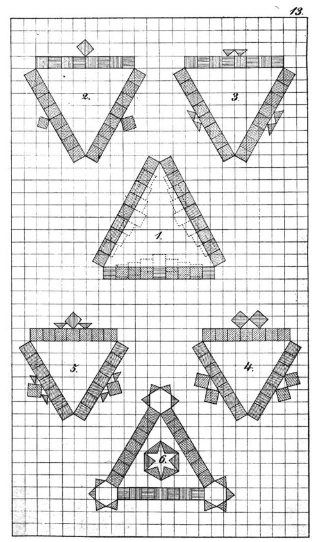
[12] Brenni 2004.

[13] Zob. np.: Harman 1982, ss. 149–155; Szumilewicz 1974, ss. 37–90. Jest czymś charakterystycznym, że wraz z powstaniem nowych rewolucyjnych teorii fizycznych na początku XX wieku (mechaniki kwantowej i mechaniki relatywistycznej) ograniczających zakres stosowalności tzw. mechaniki klasycznej, zarówno z obszaru wytwórczości, jak i dydaktyki stosunkowo szybko znikły modele mechaniczne elektromagnetyzmu.

[14] Meinel 2004; 2009.

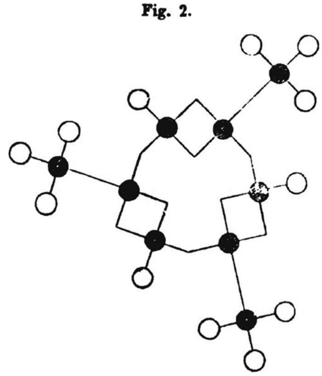


Ryc. 5. „Bicykel” Boltzmanna (1908, tabl. II, fig. 15)



Ryc. 6. Układy klocków według zaleceń F. Fröbela (1874, tabl. 13)

Fig. 2.



Ryc. 7. Struktura mezytylenu (1,3,5-trimetylobenzen, $C_6H_3(CH_3)_3$) według Kekulégo (1867, t. 10, ss. 216, fig. 2)

związków. Mimo kontrowersji i sporów, na trwałe do słownika tej dziedziny wiedzy wchodziły stopniowo pojęcia atomu, molekuły i wiązania chemicznego. Modele trójwymiarowe molekuł związków organicznych nie tylko odzwierciedlały przypuszczalny rozkład przestrzenny atomów, ale także pobudzały wyobraźnię, stanowiąc z czasem swoisty heurystyczny impuls do stawiania śmiałych hipotez, a nawet projektowania przemian chemicznych prowadzących do wytworzenia związków o zadanej uprzednio strukturze.

Na podstawie wymienionych skrótowo przykładów tendencji budowania modeli materialnych w dziewiętnastowiecznej matematyce, fizyce i chemii można wyciągnąć ostrożny wniosek, iż w znacznym stopniu były one następstwem, zwłaszcza w dwóch pierwszych obszarach wiedzy, powstania nowych koncepcji teoretycznych, charakteryzujących się wysokim stopniem abstrakcji, a jednocześnie oddaleniem od intuicji i poglądowości. Na gruncie matematyki wiązało się to m.in. z powstaniem w omawianym okresie geometrii nieeuklidesowych, abstrakcyjnych pojęć krzywizny i rozmaitości *n*-wymiarowej oraz tendencjami do uogólniania teorii matematycznych. W fizyce dalekim od poglądowości charakterem odznaczały się rozwijane w tym czasie m.in. teorie światła, ciepła czy elektromagnetyzmu. Potrzebę konkretyzacji i wizualizacji odczuwali nie tylko dydaktycy, ale – jak wskazują wymienione wyżej nazwiska – nawet wybitni uczeni, nierzadko twórcy wspomnianych teorii.

Niemiecki historyk chemii Christoph Meinel (1949–) uważa, iż istnieje głęboki kulturowy związek łączący ideę modeli struktur chemicznych Kekulégo (Ryc. 7) i van ’t Hoffa ze stylem architektonicznym licznych dziewiętnastowiecznych budowli użytkowych (takich jak konstrukcje hal wystawowych lub dworców kolejowych) oraz ideą dziecięcych zabaw percepcyjno-manipulacyjnych drewnianymi klockami w formie figur geometrycznych (np. kuli, walca, sześcianu) w ramach pedagogiki przedszkolnej niemieckiego teoretyka edukacji Friedricha Fröbela (1782–1852) (Ryc. 6). Upraszczając, związek ten wyraża się w nowym, właściwym dla omawianego okresu, sposobie i zarazem umiejętności uczenia się, rozumienia, ale i kształtowania relacji przestrzennych. Obejmował on pozornie oddalone od siebie obszary aktywności społeczno-kulturowej[15].

Zbieżności konstrukcyjno-przestrzenne widoczne są również w procesie charakterystycznej migracji idei powierzchni prostokreślnych od trójwymiarowych modeli matematycznych do rozwiązań architektonicznych końca XIX wieku, jak również wieku XX. Tendencję tę można dostrzec choćby w licznych rozwiązaniach budowlanych takich

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[15] Meinel 2004, ss. 266–269; 2009, ss. 14–17. Na temat idei pedagogicznych Fröbela zob. np.: Nawroczyński 1987, ss. 153–156. Fröbel był w znacznym stopniu inspirowany pracami szwajcarskiego pedagoga i teoretyka edukacji Johanna H. Pestalozziego (1746–1827), który za naczelną dyrektywę nauczania uznawał poglądowość (*Anschaulichkeit*).

Ryc. 8. Wieża ciśnień projektu Szuchowa (z 1896 r.) w Niżnym Nowgorodzie. © Fot. M.P. Dmitriev (1858–1948). Dostęp online (5.07.2018), domena publiczna

Ryc. 9. Rzeźba Nauma Gabo (z lat 1954–1957) przed domem towarowym w Rotterdamie. © Fot. F. Eveleens (praca własna). Rotterdam, Coolsingel. Kunstwerk „Gestileerde bloem" van Naum Gabo. Dostęp online (5.07.2018), CC-BY 3.0, zdesaturowany z oryginału

autorów jak Władimir G. Szuchow (1853–1939) zwany „rosyjskim Edisonem" (Ryc. 8), Antoni Gaudi (1852–1926) czy Le Corbusier (1887–1965)[16]. Jest interesujące, że modele nitkowe typu Oliviera, jak również późniejsze warianty modeli matematycznych proweniencji niemieckiej inspirowały także współczesnych przedstawicieli sztuki abstrakcyjnej, takich jak rzeźbiarze Naum Gabo (1890–1977) (Ryc. 9), Henry Moore (1898–1986) czy współczesny architekt-matematyk Artyom Maxim (Ryc. 10)[17]. Zdaje się to potwierdzać wspomnianą powyżej tezę Chri-

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[16] Xavier, Pincho 2016, s. 362. Można sądzić, że zainteresowanie architektów powierzchniami minimalnymi wynikało nie tylko z ich własności „estetycznych", ale i z tego, że powierzchnie te są rozwiązaniami pewnych problemów wariacyjnych (np. zapewniają maksymalną wytrzymałość przy zadanych parametrach). Za zwrócenie uwagi na ten aspekt wyrazy wdzięczności należą się jednemu z Recenzentów.

[17] Vierling-Claassen 2010, ss. 11–18. Zob. także: Lodder, Hammer 2000; Hedgecoe, Moore 1968, s. 105.

Ryc. 10. Współczesna instalacja Artyoma Maxima inspirowana matematyczną powierzchnią minimalną. © Fot. G. Wrigley (praca własna). Customs House Library Exhibit. Dostęp online (30.08.2018), CC-BY 3.0, zdesaturowany z oryginału

stopha Meinela, a nawet sugerować konieczność objęcia nią szerszych obszarów kultury.

Powracając do wspomnianego zainteresowania trójwymiarowymi modelami matematycznymi w Niemczech w drugiej połowie XIX stulecia, nie można nie wspomnieć o roli, jaką w tym procesie historycznym odegrał przywołany już powyżej niemiecki matematyk Felix Klein, którego nazwisko wiąże się głównie z programem z Erlangen. Mniej znane są natomiast jego zainteresowania trójwymiarowymi modelami obiektów matematycznych, którymi skutecznie zaraził go Julius Plücker (1801–1868) w czasie studiów Kleina w bońskiej Polytechnische Schule. Plücker do końca życia sam budował modele z gipsu, drzewa, tektury i linek na wzór szkoły Monge'a. Dodatkowym impulsem były odkrywane w tym okresie nowe krzywe i powierzchnie geometrii algebraicznej oraz różniczkowej[18]. Od tej pory modele trójwymiarowe stały

[18] Friedman 2018, s. 118.

się istotnym elementem nie tylko nauczania i pracy badawczej Kleina, ale i jego przedsięwzięć na polu reorganizacji edukacji matematycznej w niemieckich uniwersytetach i szkołach średnich[19].

Swoje zainteresowanie modelami Klein kontynuował w czasie krótkiego pobytu jako wykładowca w Getyndze, następnie jako profesor w Erlangen i znowu w Bonn, gdzie wraz z Alexandrem Brillem (1842–1935) nie tylko zaprojektował setki nowych modeli, ale także sekundował dynamicznemu rozwojowi ich wytwórczości komercyjnej. W Bonn doktoranci Kleina i Brilla mieli obowiązek wraz z pracą doktorską przedłożyć także zbudowane przez siebie modele badanych obiektów matematycznych[20]. Dla obydwu niemieckich uczonych posługiwanie się modelami trójwymiarowymi miało na celu nie tylko rozwijanie wyobraźni i intuicji matematycznej, co potwierdzało wagę ich użycia w procesie poznawczym i edukacyjnym (funkcja reprezentująca, wyjaśniająca i dydaktyczna), ale także stymulowanie odkrywania nowych twierdzeń matematycznych (funkcja heurystyczna)[21]. Odwołując się do francuskiej tradycji budowy modeli przez Monge'a i Oliviera, Klein pisał:

> Podobnie jak dziś, także wtedy celem modeli nie było może skompensowanie słabości intuicji (*Anschauung*), co *rozwinięcie* [kursywa Kleina – J.R.] żywej, jasnej intuicji – celu, który najlepiej osiągnąć przez samodzielne ich wykonanie[22].

Jak się wydaje, kluczowym pojęciem w poglądach Kleina na temat statusu modeli trójwymiarowych, a nawet – szerzej – na temat statusu współczesnej mu geometrii, było wymienione wyżej pojęcie intuicji (*Anschauung*), albo inaczej – swoistej intuicyjnej wyobraźni matematycznej. W ujęciu niemieckiego matematyka element intuicji, w szczególności intuicji przestrzennej (*räumlicher Anschauung*), wykracza poza sferę abstrakcyjności idei geometrycznych wyrażoną w formalizmie, ale stanowi zarazem dopełnienie, zwłaszcza w jego rozumieniu i twórczym rozwijaniu[23].

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[19] Schubring 2010, ss. 5–9.

[20] Friedman 2018, s. 121.

[21] Na temat wielości typów modeli naukowych i ich funkcji por. np.: Hajduk 1972.

[22] Klein 1926, s. 78.

[23] Eduard Glas (2000, ss. 80) wymienia dwa przykłady owocnej intuicji w pracach Kleina, pierwszy dotyczący wykorzystania algebraicznego pojęcia grupy do pro-

Niesprowadzalną jedynie do abstrakcyjnego formalizmu geometrię Klein nazywa „właściwą" i wspomina o niej, a także o roli w niej modeli, we własnym komentarzu do słynnego wykładu programowego wygłoszonego w Erlangen w 1872 roku:

> Istnieje geometria właściwa (*eigentliche Geometrie*), która, w przeciwieństwie do badań omówionych w tekście, nie chce być jedynie formą uzmysłowioną (*veranschaulichte Form*) badań bardziej abstrakcyjnych. W takiej geometrii figury przestrzenne są pojmowane w całej ich rzeczywistości postaciowej (*gestaltlichen Wirklichkeit*) i – co właśnie stanowi stronę matematyczną – związki do nich się odnoszące są pojmowane jako wyniki widoczne twierdzeń zasadniczych poglądu przestrzennego (*räumlicher Anschauung*). Model wykonany, spostrzegany (*angeschaut*) lub tylko żywo wyobrażany, jest dla tej geometrii nie środkiem do osiągnięcia pewnego celu, lecz samą rzeczą (*die Sache selbst*)[24].

Odnosząc się do powyższego cytatu, niemiecki historyk matematyki Herbert Mehrtens (1946–) zwraca uwagę nie tylko na istotne dla Kleina pojęcie „intuicji przestrzennej", ale także pojęcie „rzeczywistości postaciowej" (*gestaltlichen Wirklichkeit*) lub „postaci/kształtu" (*Gestalt*). Zdaniem Mehrtensa stanowi ona rodzaj istoty danego obiektu geometrycznego, którą matematyk może wyrazić tylko za pomocą notacji symbolicznej, diagramów graficznych lub modeli trójwymiarowych. Te ostatnie są dla „rzeczywistości postaciowej" różnymi reprezentacjami. W tym sensie naoczny model nie jest jakimś ograniczonym dodatkiem w działalności matematycznej, lecz jej integralnym elementem

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gramu klasyfikacji systemów geometrycznych i drugi dotyczący rozwikłania problemu rozwiązywalności równań algebraicznych piątego stopnia przez rozważenie grupy symetrii dwudziestościanu foremnego.

[24] Klein 1895, s. 56. Praca ta została przełożona przez Samuela Dicksteina (1851–1939), a przez autorów niniejszego artykułu częściowo przystosowana do wymogów aktualnego polskiego słownictwa. Wyrażenie *räumlicher Anschauung* Dickstein przetłumaczył jako „pogląd przestrzenny", co można lepiej oddać, w duchu Kleina, jako „intuicja przestrzenna". Schubring zauważa, że pojęcie *Anschauung*, często niezbyt poprawnie przekładane na różne języki, od czasów Immanuela Kanta (1724–1804) stanowi również kluczowe pojęcie w filozofii niemieckiej; zob.: Schubring 2016, ss. x–xi.

ucieleśniającym, w tym wizualizującym, postać (*Gestalt*) danego obiektu matematycznego[25].

Kres produkcji, a także szerszego zainteresowania trójwymiarowymi modelami matematycznymi nadszedł w pierwszych dwóch dekadach XX wieku wraz z wybuchem I wojny światowej. Michael Friedman uważa, że jedną z pozamatematycznych przyczyn tego procesu była także – paradoksalnie – nadmierna komercjalizacja produkcji oraz nadpodaż modeli, przeważająca nad ich znaczeniem poznawczym. Autor ten dodaje, że z przyczyn ściśle związanych z matematyką należy wymienić również odkrycie pod koniec XIX wieku, dalekich od kleinowskiego pojęcia *Anschauung*, funkcji ciągłych nigdzie nieróżniczkowalnych – tzw. analitycznych monstrów. Jednak najcięższy cios tradycji budowy modeli materialnych w matematyce, jak się wydaje, zadał formalistyczny program aksjomatyzacji matematyki Davida Hilberta (1862–1943), notabene kolegi Felixa Kleina z Getyngi[26]. A potem nastał we Francji okres działalności sławnej skądinąd grupy matematycznej Nicolasa Bourbakiego, o której amerykański fizyk i noblista David Gross pisze, że

> wywarła katastrofalny (*disastrous*) wpływ na styl publikacji matematycznych, których autorzy byli wręcz zobowiązani, by usuwać wszelkie ślady intuicji, jak również by nie ujawniać drogi dojścia do wyniku – nie mówiąc już o poglądowej wizualizacji, którą „oficjalnie" traktowano jako rzecz niemal wstydliwą i niegodną matematyka[27].

Nie oznaczało to jednak zupełnego zaniku tych tendencji w matematyce XX wieku, które nadal dostrzegały w intuicji przestrzennej istotną siłę napędową dla twórczości i odkryć[28].

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[25] Mehrtens 2004, ss. 289–290.

[26] Friedman 2018, s. 124; Mehrtens 2004, s. 290.

[27] Gross 1988. Oczywiście, nikt i nigdy formalnie nie „zobowiązywał" do usuwania wszelkich śladów intuicji. Jeżeli istniało jakieś „zobowiązanie", to raczej w charakterze przestrogi, by nie ufać zbytnio intuicji i starać się – w celu uniknięcia ewentualnego błędu – maksymalnie formalizować rozumowanie. Jest to np. ważne przy przejściu ze skończonego wymiaru do wymiaru nieskończonego, gdzie skończenie wymiarowe intuicje często zawodzą (za uwagę tę K.M. dziękuje Recenzentowi).

[28] Można tu wspomnieć choćby nurt w matematyce i filozofii matematyki zwany intuicjonizmem z jego twórcą Luitzenem E.J. Brouwerem (1881–1966) na czele.

4. Modele matematyczne w zbiorach Muzeum Uniwersytetu Jagiellońskiego

W historycznych zasobach Uniwersytetu Jagiellońskiego w Krakowie zachowało się kilka cennych modeli matematycznych z początku XX wieku[29]. Warto przy okazji ich omawiania przypomnieć o wcześniejszych pomocach w nauce matematyki na krakowskim uniwersytecie.

Nauczanie matematyki na uczelni sięga swymi korzeniami początków powstania Studium Generale, tj. II połowy XIV wieku. Szybki rozwój tej dyscypliny datuje się na rok 1402, kiedy to mieszczanin krakowski Jan Strobner ufundował katedrę astronomii i matematyki. Jedną z najstarszych pomocy dydaktycznych w przyswajaniu wiedzy z geometrii stanowiły na uniwersytecie w Krakowie figury matematyczne w formie fresków naściennych. Ich unikatowe relikty z XVII wieku zachowały się do dziś w jednym z lektoriów najstarszego budynku Uniwersytetu Jagiellońskiego, w Collegium Maius (Ryc. 11).

Pomocą w wizualizacji pojęć geometrycznych były również ryciny zamieszczane w podręcznikach i wydawnictwach encyklopedycznych XVIII wieku. Były to pomoce „dwuwymiarowe". Przestrzenne modele pojawiły się nie wcześniej niż w XIX wieku, szczególnie na potrzeby obrazowania krzywych i płaszczyzn, wykorzystywanych praktycznie w rozwijających się wówczas naukach inżynieryjnych i w technice[30].

Wytwórczość trójwymiarowych modeli matematycznych rozwinęła się w latach 1870. w Niemczech. Jedną z pierwszych wytwórni była firma Ludwiga Brilla, wydawcy, którego firma funkcjonowała od 1877 r. w Darmstadt[31]. W tym czasie, od 1875 r., matematycy Alexander von Brill i Feliks Klein prowadzili rozszerzone kursy matematyki dla

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[29] Zbiór modeli matematycznych i przyrządów liczących w Muzeum UJ jest obecnie w trakcie opracowywania. Do najcenniejszych obiektów, oprócz omawianych w artykule, należą XVII-wieczne kostki według J. Napiera oraz XIX-wieczny arytmometr według Thomasa de Colmara. Niestety, Uniwersytet Jagielloński nie posiada jednego z ważnych dla historii polskiej nauki przyrządu – integratora według Brunona Abdanka-Abakanowicza.

[30] Friedman 2018; Polo-Blanco 2007.

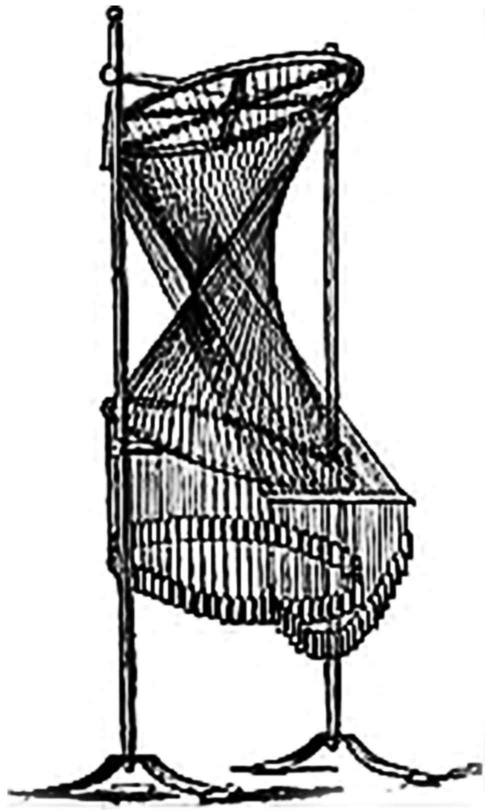
[31] Polo-Blanco 2011, s. 33; 2007; Kidwell 1996, ss. 197–208; Schilling 1903. Ludwig Brill wydał dwa katalogi modeli matematycznych pod tym samym tytułem: *Catalog mathematischer Modelle für den höheren mathematischen Unterricht veröffentlicht durch die Verlagshandlung*, Darmstadt, datowane odpowiednio 1881 i 1892.

Ryc. 11. Relikty oryginalnych fresków matematycznych wraz ze współczesnymi uzupełnieniami w lektorium Collegium Maius, Muzeum Uniwersytetu Jagiellońskiego, fot. G. Zygier

szczególnie uzdolnionych studentów w Szkole Politechnicznej (Technische Hochschule) w Monachium. Feliks Klein, propagator używania modeli w dydaktyce, projektował, podobnie jak Alexander Brill, modele dla firmy Ludwiga Brilla. W prace te angażowali oni również studentów[32]. Wśród nich był m.in. inny przyszły autor modeli przestrzennych dr Walther Dyck (1856–1934), późniejszy rektor Technische Hochschule w Monachium[33].

[32] Polo-Blanco 2007, ss. 3–5; Friedman 2018, s. 120. O wytwórcach modeli w USA patrz Kidwell 1996, ss. 197–208.

[33] Polo-Blanco 2007; Hashagen 2003.



Ryc. 12. Model strunowy oferowany przez Walthera Dycka w 1892 r.[34]

W II połowie XIX wieku wykonywano trzy typy przestrzennych modeli matematycznych. Najliczniejszą grupę stanowiły modele statyczne, wykonywane zwykle z gipsu, drewna lub kartonu. Drugi typ to ruchome modele nitkowe (strunowe). Na odpowiednio wyprofilowanych elementach metalowych, ruchomych w ustalonych płaszczyznach, naciągnięte były barwne nici, przecięcia których obrazowały odpowiednio krzywe lub płaszczyzny matematyczne (Ryc. 12 i 13).

Typ trzeci to również modele ruchome, określane w katalogach wytwórców jako kinematyczne[35]. Ich konstrukcja oparta była na układzie

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[34] Dyck 1892, s. 256.

[35] Shell-Gellasch 2015.

Ryc. 13. Model strunowy, wyk. C.M. Clinton, Ithaca, NY, USA, pocz. XX wieku, wł. Muzeum UJ, fot. G. Zygier

dźwigni i kół zębatych. Ilustrowały w sposób mechaniczny powstawanie krzywych, zamianę ruchu obrotowego na posuwisty, liniowy. Ten typ modeli został zaprojektowany przez Fredericka Schillinga (1868–1950) ok. 1898 r., wówczas profesora matematyki na Uniwersytecie w Getyndze (1899–1904), a jednocześnie doradcy naukowego firmy swego brata, Martina Schillinga w Halle[36]. W 1903 r. był dyrektorem zbioru modeli matematycznych na Uniwersytecie w Getyndze. Od 1904 r. związany był z Technische Hochschule Danzig (obecnie Politechnika Gdańska), gdzie w latach 1917–1919 pełnił funkcję rektora (Ryc. 14).

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[36] Schilling 1903, *Przedmowa* M. Schillinga z 1902 r.

Ryc. 14. Frederick Schilling. Źródło: *Beiträge und Dokumente zur Geschichte der Technischen Hochschule Danzig: 1904–1945. Zum 75. Gründungstag* (1979)

Do dnia dzisiejszego modele kinematyczne według F. Schillinga zachowały się jedynie w nielicznych kolekcjach muzealnych i uniwersyteckich.

5. Modele kinematyczne Martina Schillinga

Choć modele matematyczne z elementami ruchomymi oferowane były przez kilku innych wytwórców, to największą popularność, szczególnie w szkołach niemieckich, zyskały modele firmy M. Schilling.

W lipcu 1899 r. Martin Schilling przejął firmę Ludwiga Brilla, przeniósł jej siedzibę z Darmstadt do Halle, a około 1903 r. ponownie zmienił miejsce firmy na Lipsk[37]. Do 1904 r. firma M. Schilling oferowała 32 serie modeli. W katalogu z 1911 r. znajdujemy już 41 serii zawierających łącznie 377 modeli[38]. Firma funkcjonowała do 1935 r. i była naj-

[37] Palladino N. 1999–2000; Schilling 1903, *Przedmowa* M. Schillinga z 1902 r.; Shell-Gellasch 2015, ss. 67–179.

[38] Schilling 1911; Polo-Blanco 2011.

bardziej znaczącą, jeśli chodzi o wytwórczość modeli matematycznych. Prezentowała swe modele m.in. na Międzynarodowym Kongresie Matematycznym w 1904 r. w Heidelbergu, podczas którego F. Schilling wygłosił wykład poświęcony modelom kinematycznym[39].

M. Schilling wykonywał dwie serie modeli kinematycznych. Seria wcześniejsza oferowana była po raz pierwszy w roku 1903, oznaczona w katalogu jako seria XXIV, z podziałem na cztery grupy[40]. Modele nr 1–4, ujęte w grupie I, ilustrowały powstawanie krzywych cyklicznych kreślonych przez okrąg toczący się po zewnętrznej, lub wewnętrznej, powierzchni drugiego okręgu[41]. Kreślone w ten sposób krzywe należą do rodziny trochoid. Trzy modele grupy II, o numerach 5–7, służyły do kreślenia odpowiednio elips, ewolwent i cykloid. Modele nr 8 i 9 (grupa III) demonstrowały zamianę ruchu obrotowego odpowiednio na ruch liniowy (posuwisty) i zwrotny (model 8), i zasadę mechanizmu Watta (model 9)[42]. W grupie IV wydzielone zostały trzy modele typu inwersor, których rozróżnienie w katalogu wskazane jest poprzez nazwiska ich autorów: „nr 10. Inversor von Peaucellier, nr 11. Inversor von Hart, nr 12. Inversor von Sylvester-Kempe"[43]. Modele typu inwersor ilustrowały również zagadnienie mechanicznej zamiany ruchu z wykorzystaniem mechanizmów dźwigni i przekładni[44].

W katalogu z 1911 r. znajdujemy, obok serii XXIV, drugi zespół modeli kinematycznych liczący 11 typów, oznaczony jako seria XXXI i opisany: „Zweite Sammlung kinematischer Modelle, insbesondere zur Verzahnungstheorie, herausgegeben von Dr. Fr. Schilling, Professor an der Kgl. Technischen Hochschule in Danzig"[45]. Pięć modeli z tej serii znajduje się w Muzeum Boerhaave w Lejdzie[46].

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[39] Disteli 1904, ss. 724–728, 734; tamże lista wytwórców modeli matematycznych.

[40] Schilling 1899, ss. 214–227; Schilling 1903.

[41] W technice taki ruch ma miejsce np. w łożysku rolkowym.

[42] Shell-Gellasch 2015, ss. 167–179.

[43] Schilling 1903.

[44] Matematyczny opis modeli: Schilling 1903, s. 56; Shell-Gellasch 2015.

[45] Schilling 1911. Frederick Schilling pracował w Königliche Hochschule w Gdańsku od 1904 r., w latach 1917–1919 pełnił funkcję rektora.

[46] Są to modele oznaczone w Katalogu Schillinga: Seria XXXI numerami: 3, 5, 6, 7, 8. Numery inwentarzowe Rijksmuseum Boerhaave: V16942, V16946/, V16949/, V16942/, V16940.

Opracowane przez Fredericka Schillinga, a oferowane przez firmę M. Schilling modele posiadały charakterystyczną, rozpoznawalną do dziś konstrukcję. Mechanizm główny osadzony był w kasecie zamkniętej od góry płytą szklaną. Poszczególne elementy modelu poruszano poprzez obrót korbki osadzonej pod podstawą. Wszystkie modele tego typu posiadały ten sam rozmiar kasety, w której zostały osadzone: 22 × 27 cm. Na podstawie drukowany był katalogowy numer serii, numer modelu oraz krótki opis matematyczny. Firma oferowała je do około 1935 r. Dziś, zastąpione nowoczesnymi technikami wizualizacyjnymi, stanowią one artefakty muzealne i są obiektem badań historyków nauki.

6. Kolekcje modeli matematycznych na świecie

Jako że Ludwig Brill, a później Martin Schilling byli głównymi wykonawcami modeli matematycznych w Niemczech, ich kolekcje zachowały się stosunkowo licznie w zbiorach muzealnych i uniwersyteckich.

Irene Polo-Blanco wymienia trzy kolekcje niderlandzkie: sto osiemdziesiąt modeli Brilla i Schillinga zachowane w Uniwersytecie w Amsterdamie, około stu modeli należących do Uniwersytetu w Lejdzie, dwadzieścia modeli przechowywanych w Uniwersytecie w Utrechcie. Szczegółowo omawia ona kolekcję około stu pięćdziesięciu modeli zachowanych w Uniwersytecie w Groningen[47]. Szerszy ogląd zachowanych zbiorów matematycznych wraz z literaturą tematu przedstawia Stefan Neuwirth[48] oraz Angela Vierling-Claassen[49]. Listę instytucji posiadających kolekcje zamieszczają również autorzy projektu Touch-Geometry Project[50]. Nicla Palladino omawia zbiory włoskich muzeów i szkół wyższych[51].

We Francji zbiory modeli matematycznych znajdują się w Conservatoire des Arts et Metiers (modele strunowe, ok. 1830 r.) oraz w Poincaré Institute w Paryżu, gdzie przechowywany jest zespół około czterystu

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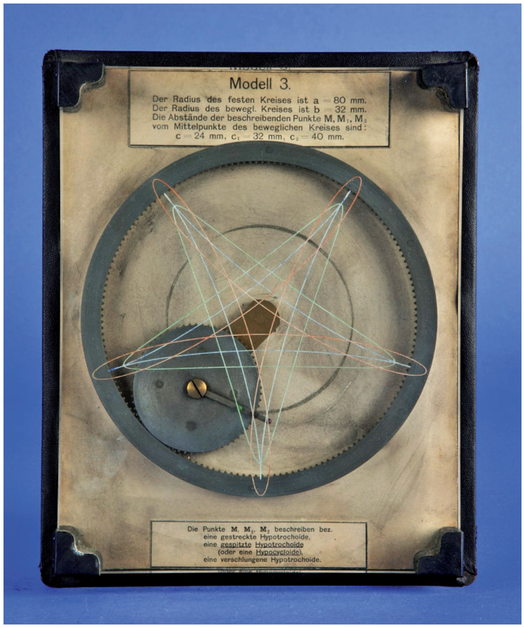
[47] Polo-Blanco 2007, ss. 9–21; 2011, s. 35. Irene Polo-Blanco i Lotte van der Zalm (2018) zamieszczają inwentarz zachowanych w Uniwersytecie w Groningen modeli różnych wytwórców, w tym M. Schillinga.

[48] Neuwirth 2014.

[49] Vierling-Claassen 2018.

[50] Karazin 2018.

[51] Palladino N. 1999–2000; Palladino N., Palladino F. 2001, ss. 781–790.



Ryc. 15. Model nr 3 serii XXXI, nr 331 Smithsonian Institution,
negative number DOR2013-50214[52]

modeli[53]. Do liczących się zbiorów niemieckich należą m.in. kolekcje Eberhard Karls Universität w Tübingen, Martin-Luther-Universität Halle-Wittenberg, Technische Universität Dresden, Universität Regensburg. Inne europejskie kolekcje to włoskie zbiory w Università degli Studi di Torino, Università degli Studi di Napoli Federico II oraz zbiory matematyczne uniwersytetów w Pawii i Mediolanie. Wiedeńska kolekcja Uniwersytetu Technicznego liczy około dwustu modeli. W Science Museum w Londynie przechowywany jest zespół modeli strunowych wykonanych przez Fabre de Lagrange'a w Paryżu[54]. W London Mathematical Society znajdują się modele autorstwa Juliusa Plückera (1801––1868)[55], którego uczniami byli Feliks Klein i Walther Dyck. Zbiory matematyczne posiadają również uniwersytety w Coimbrze, Lizbonie,

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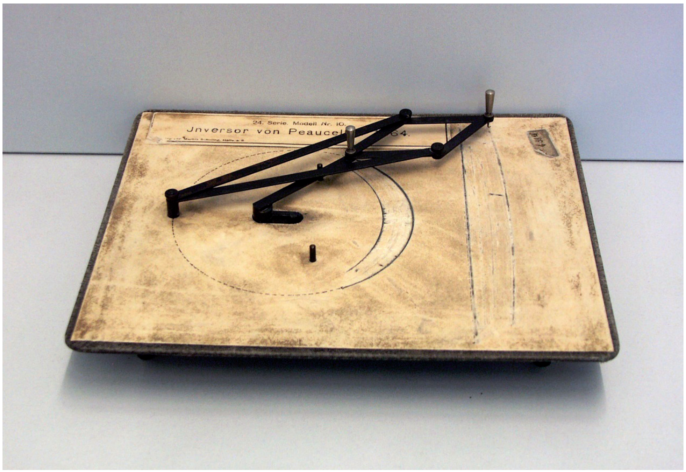
[52] Shell-Gellasch 2014. Dostęp online (15.09.2018).
[53] Instytut Henri Poincarégo 2018.
[54] Science Museum w Londynie 2018.
[55] Londyńskie Towarzystwo Matematyczne 2018.

Saragossie, Charkowie[56]. W Stanach Zjednoczonych znajdujemy zachowane modele z wytwórni Brilla i Schillinga, ale także wykonane przez lokalnych wytwórców. W literaturze wymieniane są kolekcje w Harvard University w Cambridge, Masssachusetts (modele gipsowe Brill/Schilling)[57], MIT Cambridge (modele gipsowe Brill/Schilling), w University of Illinois w Urbana-Champagne (w Altgeld Hall), Fairfield University w Connecticut, University of Arizona (modele Richarda Bakera (1886–1937)). Jednym ze znaczących jest zbiór modeli należących do Smithsonian Institution i przechowywanych w The National Museum of American History[58].



Ryc. 16. Model nr 10 typu inwersor von Peaucelliera, Martin-Luther-Universität Halle-Wittenberg[59]

Modele kinematyczne w powyższych zbiorach stanowią rzadkość. Wydaje się, że żadna z kolekcji nie posiada kompletu liczącego dwanaście modeli kinematycznych grupy XXIV, a także wszystkich z grupy XXXI, oferowanych przez Martina Schillinga. Najliczniejszy ich

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[56] Vierling-Claasen 2018; Mathematics & Computer Science Library. The Hebrew University of Jerusalem 2018.

[57] Vierling-Claassen 2007.

[58] *Ibidem*.

[59] Martin-Luther-Universität Halle-Wittenberg 2018. Dostęp online (2.09.2018).

zbiór znajduje się w National Museum of American History[60] (Ryc. 15). Muzeum to posiada dziesięć modeli serii XXIV, z wyjątkiem dwóch, o numerach 2 i 5.

Pojedyncze modele kinematyczne znajdują się m.in. w Harvard University, University of Groningen (model nr 7)[61], Utrecht University (modele serii XXIV nr 9, 10, 11, 12)[62], w Instytucie Matematyki Martin-Luther-Universität Halle-Wittenberg (trzy modele typu inwersor nr 10, 11, 12), w University of Illinois w Urbana-Champaign (modele nr 1, 3, 10, 11, 12)[63].

Trzy modele kinematyczne Schillinga w zbiorach Muzeum Uniwersytetu Jagiellońskiego w Krakowie uzupełniają powyższą listę.

7. Modele kinematyczne Schillinga w Muzeum Uniwersytetu Jagiellońskiego

Zachowane w Krakowie modele to urządzenia z grupy II serii XXIV, ilustrujące mechaniczny sposób kreślenia krzywych matematycznych. Są to modele o numerach 5, 6 oraz 7.

Model nr 5 ilustruje konstrukcję figur kreślonych przez punkty umieszczone na średnicy, i jej przedłużeniu, koła mniejszego, które porusza się wewnątrz koła o średnicy dwukrotnie większej od koła mniejszego. Odpowiednie punkty kreślą elipsy. Punkty umieszczone na końcach promienia koła mniejszego tworzą podczas jego obrotu proste prostopadłe. Model ten, oprócz tworzenia elipsy, ilustruje zamianę ruchu obrotowego na ruch posuwisty (Ryc. 17a).

Model nr 6 obrazuje powstawanie ewolwenty koła, którą kreśli punkt (punkty) umieszczony (umieszczone) na prostej i na końcach odcinka prostopadłego do prostej poruszającej się po obwodzie koła (Ryc. 17b).

Model nr 7 obrazuje sposób powstawania cykloidy – krzywej, jaką zakreśla punkt leżący na obwodzie koła, które toczy się po prostej. Model obrazuje powstawanie trzech typów cykloid w zależności od

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60 Shell-Gellasch 2015; Kidwell 1996, ss. 197–208; Smithsonian Institution 2018 (szczegółowy opis dziesięciu modeli wraz z literaturą).

61 Polo-Blanco, van der Zalm 2018.

62 Uniwersytet w Utrechcie 2018.

63 Uniwersytet Stanu Illinois 2018.

Ryc. 17 a–c. Modele kinematyczne M. Schillinga, seria XXIV, nr 5, 6 oraz 7, wł. Muzeum UJ, fot. G. Zygier położenia punktów, które kreślą krzywe: cykloidę zwykłą, wydłużoną i skróconą[64] (Ryc. 17c).

Wyjaśnienia wymaga proweniencja tych przyrządów. Z dużym prawdopodobieństwem datować je można na rok 1902[65]. W tym okresie I Katedrą Matematyki na Uniwersytecie Jagiellońskim kierował Kazimierz Żorawski (1866–1953), powołany na to stanowisko w 1895 r.[66] Był on również kierownikiem Wyższego Seminarium Matematycznego[67], które funkcjonowało przy Wydziale Filozoficznym już od lat siedemdziesiątych XIX wieku[68]. Wiadomo, że prowadził wykłady m.in. z tematyki: geometria elementarna, krzywe i powierzchnie[69]. Wiadomo również, że mocą reskryptów c.k. Ministerstwa Wyznań i Oświecenia przyznawane mu były regularne dotacje na zakup książek i modeli dla Seminarium Matematycznego. W 1899 r. K. Żorawski otrzymał z kasy Ministerstwa kwotę „dwustu złotych na zakupno najważniejszych modeli i książek dla rzeczonego seminaryum"[70]. W roku 1902 dotacja ta wyniosła 400 zł. Można sądzić, że właśnie z jej części zakupione zostały badane modele[71]. Wykłady Seminarium Matematycznego odbywały się w sali nr XXXI budynku Collegium Novum przy ul. Gołębiej 20 (obecnie nr 24). Tam też znajdowały się dwie szafy całkowicie zapełnione „zbiorami seminaryjnymi"[72] (rok 1899).

Nie udało się dotychczas odnaleźć spisu modeli należących do Seminarium Matematycznego. Dalsze poszukiwania być może pozwolą

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64 Punkt na końcu promienia koła kreśli cykloidę zwykłą, punkt na przedłużeniu promienia kreśli cykloidę wydłużoną, punkt położony na promieniu wewnątrz koła kreśli cykloidę skróconą.

65 W pudełku jednego z modeli znajduje się fragment gazety *Göttinger Zeitung* z 22 XI 1902 r.

66 W latach 1917–1919 był rektorem Uniwersytetu Jagiellońskiego. Dalsza jego kariera naukowa związana była ze środowiskiem uczonych warszawskich.

67 AUJ, sygn. WF II 163 Katedry i Instytut Matematyczny 1851–1945, k. 38 i inne.

68 Materiały dotyczące funkcjonowania Seminarium Matematycznego zawarte są w zespołach pod sygnaturami: AUJ sygn. SII 865 Wydział Filozoficzny. Matematyka oraz AUJ sygn. WF II163 Katedry i Instytut Matematyczny 1851–1945. Zachowane materiały dotyczą spraw finansowych i organizacyjnych Seminarium.

69 Ciesielska, Domoradzki 2014, s. 64.

70 AUJ, sygn. WFII 163, k. 38, pismo do c.k. filialnej Kasy w Krakowie z dnia 21 VI 1899; AUJ SII 865 pismo: SEN Ak. L1026. „Odpis do akt" z dnia 30 VI 1899.

71 Cena zakupu za trzy modele podana w katalogu wynosiła 183 marki.

72 AUJ sygn. SII 865, pismo K. Żorawskiego z dnia 25 XI 1899 r.

wyjaśnić, ile przyrządów zakupiono w firmie M. Schilling i jakie były dalsze ich losy. W roku 1923 Ministerstwo Wyznań Religijnych i Oświecenia Publicznego, na wniosek Wydziału Filozoficznego UJ, wydało zgodę na utworzenie Instytutu Matematycznego Uniwersytetu Jagiellońskiego złożonego z zespołu „seminariów matematycznych łącznie z biblioteką seminaryjną i zbiorem modeli"[73]. Zapewne z tego zbioru pochodzą zachowane modele Schillinga.

Warto wspomnieć o wcześniejszym gabinecie modeli matematycznych, który istniał na Uniwersytecie Jagiellońskim już w roku 1834. Z tego roku zachował się „Inwentarz Modeli dla Katedry Matematyki Elementarnej sprawiony". Spis początkowo liczył 16 pozycji i obejmował 28 modeli drewnianych i mosiężnych, wykonanych przez lokalnych rzemieślników[74]. Dwa lata później gabinet wzbogacił się o kolejne 15 modeli, a w 1841 r. stwierdzono ich kradzież. Data ich zniknięcia nie została dokładnie określona, co pozwala sądzić, iż nie były zbyt często wykorzystywane. Szybko ujęto sprawcę, odzyskano i naprawiono uszkodzone modele[75]. W roku 1851 podjęta została decyzja o likwidacji gabinetu i przekazaniu modeli do gimnazjum św. Anny[76]. Powodem likwidacji zbiorów był brak pomieszczenia na ich przechowywanie, co również sugeruje, iż nie były specjalnie pomocne w edukacji. Ówczesny profesor matematyki Jan Kanty Steczkowski (1800–1881) zdecydował o zatrzymaniu dla własnego użytku kilku modeli, w tym

> 1. Hiperboloid kołowy obrotowy, 2. Hiperboloid kołowy nieobrotowy, 3. Konoid i hiperboloid obrotowy na jednym modelu, wszystkie 3-nitkowe z obrączkami i słupkami mosiężnymi, każdy umieszczony w oddzielnym pudełku tekturowym […][77].

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[73] AUJ sygn. WFII 163, pismo Departamentu Nauki i Szkół Wyższych Ministerstwa Wyznań Religijnych i Oświecenia Publicznego z dnia 31 VIII 1923 r.; tu również projekt Regulaminu Instytutu Matematycznego Uniwersytetu Jagiellońskiego.

[74] AUJ, sygn. WFII 163, „Inwentarz Modeli dla Katedry Matematyki Elementarnej sprawiony", karty bez numeracji.

[75] Ich naprawę wykonał Jacenty Taborski, mechanik uniwersytecki w latach 1825–1857, zm. 1862 – zob. Gablankowski 2004, s. 42.

[76] Na własność Gabinetu Mineralogicznego przekazano wówczas 159 modeli krystalograficznych: AUJ WFII 163, notatka z dnia 14 XI 1870 r.

[77] AUJ sygn. WFII 163, notatka z dnia 14 XI 1863 r. na okoliczność zwrotu tych modeli przez prof. Jana Kantego Steczkowskiego.

Modele te nie zachowały się, podobnie jak i inne wymienione w spisie.

Podsumowując, spośród trzech modeli kinematycznych Schillinga znajdujących się w Muzeum Uniwersytetu Jagiellońskiego, model nr 5 być może jest jedynym, zachowanym w zbiorach polskich muzeów. W Muzeum Uniwersytetu Wrocławskiego zachowały się modele kinematyczne serii XXIV o numerach 2, 3, 4, 6. Wraz z opisywanymi ze zbiorów krakowskich reprezentują łącznie 6 rodzajów modeli z tej serii.

Nie jest wiadomo, czy zakupiona została przez Uniwersytet Jagielloński cała seria XXIV modeli lub modele z innych serii.

Bez wątpienia można uznać, że w środowisku profesorów matematyki prowadzących Seminarium Matematyczne istniała świadomość i potrzeba posiadania pomocy dydaktycznych, jakimi były modele geometryczne. By ocenić, czy i jak szeroko używano ich w praktyce dydaktycznej, konieczne jest badanie nieco innego charakteru źródeł niż wykorzystane w niniejszym artykule.

8. Podziękowania

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Vestium i Ruthenium na tle historii chemii

Abstrakt

Trwający już ponad sto lat spór o to, czy odkryty przez Jędrzeja Śniadeckiego, a nieuznany przez jemu współczesnych pierwiastek Vestium jest odpowiednikiem rutenu, wydaje się dziś bardzo trudny do rozstrzygnięcia. Sam ten spór, który w istocie rzeczy sprowadza się do ustalenia pierwszeństwa odkrycia, nabiera w świetle historii chemii – a zwłaszcza historii badań surowej platyny – zupełnie innego znaczenia. W tym kontekście okazuje się bowiem, że Jędrzej Śniadecki był pierwszym uczonym, który starał się udowodnić istnienie szóstego platynowca. Czas pokazał, że miał rację. Późniejsze wyodrębnienie metalicznego rutenu przez Karla Ernsta Clausa tylko tę rację potwierdziło.

Słowa kluczowe: *Jędrzej Śniadecki*, *Uniwersytet Wileński*, *surowa platyna*, *platynowce*, *west*, *Vestium*, *ruten*, *Ruthenium*

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Westium and Ruthenium against the background of the history of chemistry

Abstract

Is the chemical element Vestium discovered by Jędrzej Śniadecki the same as the Ruthenium? The dispute on this subject has been going on for more than one hundred years. At present, this dispute over recognition of the priority of discovery is very difficult to resolve. However, from the point of view of the history of chemistry, another aspect is more important in all this. In this context, it turns out that Jędrzej Śniadecki was the first researcher who tried to prove the existence of the sixth platinum metal. Time showed that Śniadecki was right. The confirmation of this was the subsequent separation of the metallic ruthenium by Karl Ernst Claus.

Keywords: *Jędrzej Śniadecki, Vilnius University, crude platinum, platinum metals, Vestium, Ruthenium*

1. Wprowadzenie

Historia odkrycia westu vel rutenu, chociaż doczekała się już stosunkowo bogatej literatury, wciąż ekscytuje wielu badaczy dziejów chemii w Polsce. Jedni z uporem utrzymują, że west należy traktować jako wytwór błędu i imaginacji; inni – że jest to pierwiastek tożsamy z rutenem. Spór trwa do dziś. Zaczął się, gdy sprawą zainteresowali się chemicy, czyli nieco ponad pół wieku po śmierci Jędrzeja Śniadeckiego (1768–1838) i prawie sto lat po ogłoszeniu przez niego doniesienia o odkryciu[1] nowego metalu w surowej platynie. Wcześniej o życiu i wielokierunkowej działalności Jędrzeja Śniadeckiego pisali głównie historycy i lekarze, przede wszystkim Michał Baliński (1794–1864), Józef Bieliński (1848–1926), Zygmunt Kramsztyk (1849–1920), Adam Wrzosek (1875–1965) i inni[2]. W kwestii westu autorzy ci albo nie wypowiadali się wcale, albo też podzielali dość bezkrytycznie negatywną opinię francuskich akademików.

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1 Śniadecki 1808.
2 Żurawska, Konieczyńska 1970.

Kontrowersje pojawiły się na początku XX w., kiedy to nareszcie chemicy postanowili przyjrzeć się zawartemu w rozprawie Śniadeckiego opisowi badań, dotyczących chemicznego składu rudy platynowej. Pierwsza ważna publikacja[3] na ten temat ukazała się w 1907 r. w *Chemiku Polskim*. Jej autorem był inżynier technologii chemicznej, adiunkt na Politechnice Warszawskiej – Wacław Kączkowski (?–1943). Na podstawie badań przeprowadzonych zgodnie z opisanymi przez Śniadeckiego doświadczeniami Kączkowski doszedł do wniosku, że podobieństwa między westem i rutenem nie pozostawiają wątpliwości, iż jest to ten sam pierwiastek. Zaobserwowane różnice uznał autor za mało istotne i przypuszczalnie spowodowane tym, że Śniadecki nie zdołał dość dokładnie oczyścić otrzymanej soli nowo odkrytego pierwiastka, zwłaszcza że jej ilość musiała być bardzo mała.

Podobny pogląd wyraził w niemieckojęzycznej publikacji[4] Jan Zawidzki (1866–1928) – wybitny fizykochemik, uczeń i współpracownik noblisty Wilhelma Ostwalda w Lipsku, a wówczas profesor Akademii Rolniczej w Dublanach. Spośród obcokrajowców pisał o Jędrzeju Śniadeckim jako o odkrywcy[5] szóstego platynowca inny uczeń Ostwalda – łotewski chemik Paul Walden (1863–1957), profesor Politechniki w Rydze. Niemal sto lat później podobnego zdania byli amerykańscy autorzy James L. Marshall i Virginia Marshall[6].

W okresie międzywojennym do sprawy westu i rutenu powracano wielokrotnie, zgadzając się na ogół co do tego, że pierwszeństwo odkrycia należy się Śniadeckiemu. Takie stanowisko[7] reprezentował na przykład Kazimierz Sławiński (1870–1941), profesor chemii organicznej na Uniwersytecie Stefana Batorego w Wilnie. W podobnym tonie pisano[8] i wypowiadano się szczególnie przy okazji obchodów 100. rocznicy śmierci Jędrzeja Śniadeckiego. Rocznicę tę wyjątkowo uroczyście uczciły dwa towarzystwa naukowe, a mianowicie Polskie Towarzystwo Fizjologów oraz Polskie Towarzystwo Chemiczne, organizując wiosną 1938 r. w Wilnie wspólny Zjazd poświęcony Śniadeckiemu.

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3 Kączkowski 1907.
4 Zawidzki 1909 (cyt. za Siemion 2009, s. 102).
5 Walden 1917 (cyt. za Siemion 2009, s. 102).
6 J.L. Marshall, V. Marshall 2010.
7 Sławiński 1933.
8 Pleśniewicz, Sarnecki 1938.

Kontrowersje i ostre niekiedy polemiki pojawiły się u nas dopiero po II wojnie światowej. Toczyli je chemicy: Kazimierz Sarnecki (1909–1991), Włodzimierz Hubicki (1914–1977), Rajmund Sołoniewicz (1929–1993), Ignacy Z. Siemion (1932–2015) i wielu innych. Do współczesnych krytyków Śniadeckiego, negujących podobieństwo między westem i rutenem, należy Roman Mierzecki; do konsekwentnych obrońców natomiast – Roman Edmund Sioda (1937–2018).

Mierzecki przedstawił bodaj najpełniej swoje stanowisko w tej sprawie na łamach *Wiadomości Chemicznych*, w artykule „Jędrzej Śniadecki i ruten”[9]. Nie wszystkie przytoczone tam racje są przekonujące, miejscami uderza tendencyjna nadinterpretacja, a sięgnięcie do terminologii zastosowanej w polskim przekładzie podręcznika Jakoba Spielmanna[10] musi być odebrane jako anachronizm, zwłaszcza że podręcznik ten był uznany za przestarzały już w czasie jego druku w Krakowie. Na artykuł R. Mierzeckiego odpowiedział Roman Sioda publikacją: „Różnorodność czy identyczność Vestium/Ruten?”[11]. Jednakże znacznie bardziej niż ta polemika warte podkreślenia są artykuły[12] Siody, w których na podstawie dokumentów archiwalnych oraz korespondencji z francuskimi archiwistami, jako pierwszy podważył wiarygodność werdyktu paryskiej Akademii.

Wystarczy prześledzić większość starszych i nowszych publikacji na temat westu, aby się przekonać, że zawierają one z grubsza ten sam zasób informacji i opierają się na tych samych źródłach, nie wnosząc na ogół do dysputy niczego nowego. Jedyne, co je w istocie różni, to sposób interpretacji rozprawy Jędrzeja Śniadeckiego o wescie oraz stosunek do negatywnej opinii, wyrażonej w 1808 r. przez Narodowy Instytut w Paryżu.

2. Kontekst historyczny

Nie wnikając w argumentację żadnej ze stron, spróbujmy spojrzeć na cały ten problem z perspektywy historii nauki. Cofnijmy się w tym celu do roku 1797, czyli do czasów, kiedy to Jędrzej Śniadecki rozpoczynał swoją chemiczną karierę, obejmując wykłady tego przedmiotu na

[9] Mierzecki 2011, ss. 510–527.
[10] Spielmann 1791.
[11] Sioda 2011a.
[12] Sioda 2011b; 2012.

Uniwersytecie Wileńskim. Trwało jeszcze Oświecenie – epoka dla rozwoju chemii wybitnie sprzyjająca, bo zafascynowana przyrodą, a zwłaszcza możliwościami praktycznego wykorzystania jej zasobów. Dlatego też problematyka substancjalnego składu ciał oraz umiejętność ich realnego przekształcania zyskała rangę, jakiej nie miała nigdy wcześniej. Nowoczesna chemia w Europie dopiero się kształtowała, odrzucając dziedzictwo sędziwej alchemii. Awangardą była stworzona przez Antoine'a Lavoisiera (1743–1794) i jego współpracowników szkoła naukowa, której Jędrzej Śniadecki był gorliwym propagatorem w Polsce.

Arystotelesowskie żywioły, które przez całe wieki służyły do wyjaśniania wszelkich zjawisk przyrody, dla chemii stały się nieprzydatne, podobnie jak prowadząca na manowce alchemiczna triada Paracelsusa (1493–1541). Za elementarne składniki wszechświata zaczęto uznawać konkretne substancje, izolowane z konkretnych materiałów metodą rozkładu. Jako jeden z pierwszych takie stanowisko wyraził Robert Boyle (1627–1691), który nazwał pierwiastkami ciała otrzymywane u kresu analizy[13]. Sto lat później podobną definicję sformułował Lavoisier[14], przyjmując za pierwiastki wszystkie takie substancje, których nie udało się rozłożyć na składniki prostsze. Lavoisier nie przesądzał przy tym o liczbie pierwiastków. Nie był także pewien, czy ciała uznawane w danej chwili za elementarne nie zostaną w przyszłości rozłożone.

Z najwcześniejszymi założeniami szkoły Lavoisiera, czyli nową definicją pierwiastków, prawem zachowania masy oraz tlenową teorią spalania, Jędrzej Śniadecki zetknął się już podczas studiów w Krakowie. Późniejsze studia w najlepszych uczelniach europejskich oraz znajomość najnowszej literatury, w tym *Philosophie Chimique*[15] Antoine'a F. Fourcroya (1755–1809), ukierunkowały ostatecznie jego poglądy.

Śniadecki wykładał chemię w języku ojczystym, w czym także wzorował się na szkole francuskiej. Francuskie systemowe słownictwo chemiczne opracowali wspólnie Louis B. Guyton de Morveau (1737–1816), Antoine Laurent Lavoisier, Claude Louis Berthollet (1748–1822) oraz Antoine François de Fourcroy. Rzecz[16] została opublikowana w Paryżu (pierwsze wydanie w 1787 r.).

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13 Boyle 1661.

14 Lavoisier 2001.

15 Fourcroy 1792.

16 Guyton de Morveau, Lavoisier, Berthollet, Fourcroy 1787.

Śniadecki starał się to francuskie słownictwo wiernie zaadaptować do języka polskiego.

Ułożony przez niego słownik polskiej naukowej nomenklatury chemicznej, zawierający również nazwy łacińskie, ukazał się w 1800 r. w pierwszym wydaniu jego podręcznika *Początki chemii*[17].

W tym czasie znano zaledwie około 30 substancji, które nie poddawały się próbom rozkładu. Mieściły się w tej liczbie gazowe składniki powietrza (azot i tlen), składnik wody – wodór, a także węgiel, fosfor i siarka oraz 22 metale. Na czele tej listy, którą można było traktować jako listę pierwiastków, Lavoisier umieścił dwa ciała najbardziej rozpowszechnione i najlżejsze ze wszystkich, a mianowicie: światło i cieplik. Śniadecki posunął się w tej kwestii jeszcze dalej. Jego oryginalnym pomysłem było stworzenie oddzielnej grupy ciał prostych, którą nazwał „pierwiastkami promienistymi" i do której zaliczał, oprócz światła i cieplika, także elektryczność i magnetyzm.

Lista pierwiastków szybko się powiększała, mimo że możliwości chemii analitycznej były wówczas bardzo skromne. Najstarszą i najlepiej opanowaną metodą była destylacja w różnych jej odmianach. Pozwalała na rozdzielanie składników badanej próbki na podstawie różnic temperatury wrzenia. Wstępna identyfikacja otrzymanych frakcji polegała na prostych obserwacjach – smaku, zapachu, barwy, konsystencji itp. Dalszym etapem identyfikacji było przeprowadzanie reakcji chemicznych z użyciem charakterystycznych odczynników. Z uzyskanych produktów takich reakcji można było wnioskować, z jaką substancją miało się do czynienia. Rzadko wyznaczano ciężar właściwy wydzielonego składnika; jeszcze rzadziej wykonywano oznaczenia ilościowe.

Nowszy i uzyskujący z czasem dominujące znaczenie był sposób analizy polegający na próbach rozpuszczania badanego ciała kolejno w różnych rozpuszczalnikach (najczęściej kwasach mineralnych i alkoholu). Otrzymany roztwór i część nierozpuszczoną badano oddzielnie. Część nierozpuszczoną starano się rozpuścić, zmieniając rozpuszczalnik. Roztwór zaś poddawano działaniu odczynników (dobieranych metodą prób i błędów) w celu wytrącenia osadu, który należało zebrać za pomocą dekantacji lub przesączania i zidentyfikować. Przy identyfikacji osadu ważną wskazówką był jego wygląd, w szczególności kolor

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[17] Śniadecki 1800.

– traktowany jako cecha charakterystyczna. Pozostały po oddzieleniu osadu przesącz podlegał w taki sam sposób dalszemu badaniu. Cały tok analizy był tym dłuższy i bardziej skomplikowany, im więcej składników zawierała próbka.

Bardzo atrakcyjne dla chemii okazały się wynalazki Alessandra Volty (1745–1827), a zwłaszcza ogniwo prądu stałego zbudowane w 1800 r. i ze względu na konstrukcję noszące nazwę stosu. Stos Volty od razu stał się narzędziem służącym chemikom do rozmaitych eksperymentów. Najbardziej efektywnie realizował te eksperymenty Humphry Davy (1778–1829), który poprzez elektrolizę roztworów soli i soli stopionych otrzymał kilka nieznanych wcześniej pierwiastków. W 1807 r. odkrył sód i potas, a rok później magnez, wapń i bar.

W tym czasie Jędrzej Śniadecki nie miał jeszcze możliwości uczestniczenia w światowym nurcie badań. Po swoim poprzedniku – Józefie Sartorisie (?–1799) odziedziczył niewielkie i słabo wyposażone laboratorium. Na bieżąco śledził wszystkie nowe osiągnięcia chemii i zajmował się dydaktyką oraz pisaniem pierwszego w języku polskim podręcznika chemii dla studentów. Już na początku swojej akademickiej działalności podjął usilne starania o budowę nowego kolegium chemicznego, podobnego do tych, jakie widywał i w jakich kształcił się za granicą.

Jego zabiegi wkrótce przyniosły pozytywne rezultaty. Przy dawnym placu św. Michała w Wilnie stanął okazały gmach Kolegium Chemii. Nad jego projektowaniem i budową Śniadecki czuwał osobiście. Ponad połowę powierzchni gmachu zajmowało okrągłe dwupiętrowe audytorium, do którego przylegały pracownie naukowo-dydaktyczne oraz gabinet profesorski[18]. Była to nowocześnie urządzona placówka naukowo-badawcza, o którą Śniadecki troszczył się stale, dokonywał zakupów odpowiedniej aparatury, zamawiał odczynniki, sprowadzał specjalistyczną literaturę.

3. Szósty platynowiec Jędrzeja Śniadeckiego

Gdy w naukowej prasie europejskiej pojawiły się doniesienia o wielości metalicznych domieszek zawartych w ziarnach surowej platyny, wileńskie Kolegium Chemiczne było już na ukończeniu. Surowa platyna,

[18] Koskowski 1938.

w której obok złota, srebra, miedzi, ołowiu, żelaza, chromu i tytanu brytyjscy badacze – Smithson Tennant (1761–1815) oraz William Hyde Wollaston (1766–1828) – w ciągu zaledwie dwóch lat (1803–1804) odkryli jeszcze cztery nowe pierwiastki, zbliżone właściwościami do platyny (iryd, osm, pallad i rod), fascynowała wielu chemików.

Zafascynowany tymi odkryciami był również Jędrzej Śniadecki, toteż w 1806 r., dysponując stosunkowo niewielką próbką surowej platyny, przystąpił do jej analizy. Pragnął powtórzyć doświadczenia Tennanta i Wollastona. Udało mu się wykryć liczne znane już metale, a ponadto małą ilość niezidentyfikowanej soli. Badania musiał jednak przerwać z powodu wyczerpania całego zasobu surowca. Powrócił do nich po roku, gdy Uniwersytet zakupił nową porcję platynowego minerału.

Próbka, którą teraz zajął się Śniadecki, ważyła 400 gramów. Pozwalało to na wielokrotne wykonywanie analizy jakościowej. Za każdym razem otrzymywał sól, której właściwości istotnie różniły się od soli znanych metali. Poświęcił temu zagadnieniu prawie dwa lata pracy i w końcu był pewien, że w ziarnach platyny odkrył szósty, podobny do platyny metal. Nazwał ten nowy metal Vestium[19].

Dalszy ciąg tej historii był już po wielekroć opisywany. Ówczesny rektor Uniwersytetu Wileńskiego – Jan Śniadecki (1756–1830), prywatnie brat Jędrzeja, wziął sprawę w swoje ręce. Krótką, napisaną w języku francuskim przez Jędrzeja, szczegółową relację z przeprowadzonej analizy przekazał do Institut Imperial de France (tak się wtedy nazywała Akademia Nauk w Paryżu). Podobny dokument dołączył do listu adresowanego do Petersburskiej Cesarskiej Akademii Nauk, zawierającego informację o odkryciu nowego pierwiastka. Każda z tych Akademii na wiadomość o Vestium zareagowała inaczej.

Jak na podstawie francuskich archiwaliów udowodnił[20] Roman E. Sioda, doniesienie o towarzyszącym platynie, nieznanym wcześniej metalu referował na posiedzeniu Akademii Nauk w Paryżu w dniu 11 VII 1808 r. astronom, dyrektor Obserwatorium Paryskiego – Jean Baptiste Joseph Delambre (1749–1822). Tydzień później odbyło się następne posiedzenie, na którym zdecydowano, aby sprawę westu zbadała komisja w składzie: C.L. Berthollet, L.B. Guyton de Morveau, A. de Fourcroy

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[19] Śniadecki 1808.
[20] Sioda 2011c.

oraz Louis Nicolas Vauquelin (1763–1829). Wszyscy czterej członkowie tej komisji byli w społeczności francuskiej postaciami cieszącymi się dużym autorytetem, odgrywającymi ważne role nie tylko w nauce, ale także – a może nawet przede wszystkim – w polityce.

Berthollet z wykształcenia był lekarzem. W czasie rewolucji francuskiej pełnił wysokie funkcje państwowe. Był dyrektorem mennicy, następnie szefem ekspedycji do Włoch z zadaniem gromadzenia cennych przedmiotów oraz dzieł sztuki i transportowania ich do Paryża. Zawarł wówczas znajomość z młodym generałem Napoleonem Bonaparte, którego później był bliskim przyjacielem. Gdy Napoleon objął władzę, Berthollet otrzymał godność senatora i księcia. Po klęsce Napoleona stał się jego zagorzałym przeciwnikiem i jednym z najgorliwszych stronników restauracji. Król Ludwik XVIII mianował go za to członkiem Izby Lordów[21].

Guyton de Morveau ukończył w rodzinnym Dijon studia prawnicze, a następnie studiował literaturę w Paryżu oraz zajmował się pisaniem poezji i utworów satyrycznych. Po powrocie do Dijon został członkiem tamtejszej Akademii. Wtedy po raz pierwszy zaczął interesować się chemią. Podjął samodzielne studia w tym kierunku i kontynuował je do końca życia, nie zaniedbując przy tym nauk prawniczych oraz własnej twórczości literackiej. Zgodnie z duchem swojej epoki cenił chemię jako naukę mającą wielkie możliwości praktycznych zastosowań. Opracował dział chemiczny w Wielkiej Encyklopedii Francuskiej. W czasie rewolucji przybył do Paryża. Brał udział w ekspedycji wojskowej do Belgii jako dowódca oddziału balonowego. Podczas bitwy pod Fleurs wzniósł się balonem nad pozycje nieprzyjaciela, aby obserwować liczebność i przemieszczanie wojska. Po powrocie do Paryża uczył w Szkole Politechnicznej. Napoleon obdarzył go tytułem barona[22].

Fourcroy, syn paryskiego aptekarza, ukończył studia medyczne, lecz praktyka lekarska nie pociągała go wcale. Wolał zajmować się chemią. Począwszy od 1784 r. był profesorem chemii w kilku uczelniach, organizatorem nauczania tego przedmiotu, autorem prac metodycznych i popularyzatorskich. Aktywny jakobin w czasach Rewolucji piastował eksponowane stanowiska, w których jego chemiczne kwalifikacje

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[21] Szabadváry 1966, ss. 106–107.

[22] *Ibidem*, ss. 205–206.

okazywały się przydatne (produkcja prochu strzelniczego, przemysł zbrojeniowy, mennica). Jego zasługi docenił cesarz Napoleon Bonaparte, nadając mu tytuł książęcy. W historii nauki Fourcroy zdobył trwałą pozycję przede wszystkim dzięki badaniom składu chemicznego leczniczych wód mineralnych oraz materiałów pochodzenia roślinnego i zwierzęcego[23]. Uczniem i współpracownikiem Fourcroya, a także współautorem większości jego prac dotyczących analizy naturalnych substancji organicznych był Louis N. Vauquelin.

Vauquelin pochodził z Normandii. W wieku 15 lat rozpoczął naukę jako uczeń w aptece w Rouen. Wykazywał wybitne zdolności i robił szybkie postępy. Opanował podstawowy kurs chemii i fizyki, korzystając z podręczników pożyczanych od studentów. W celu kontynuowania nauki udał się do Paryża, gdzie praktykował w kilku aptekach. Tam poznał Fourcroya, który zatrudnił go jako asystenta. W tym czasie Vauquelin ukończył studia na kierunku klasycznym, a po uzyskaniu dyplomu całkowicie poświęcił się farmacji i chemii. Przygotowywał doświadczenia, które Fourcroy demonstrował podczas swoich wykładów, a później sam także został wykładowcą. Był utalentowanym eksperymentatorem, toteż w pracy laboratoryjnej przerósł niebawem swego mistrza. Pierwsza wspólna praca Fourcroya i Vauquelina ukazała się w 1790 r., ale dopiero w latach, które nastąpiły po Rewolucji, powstała większość ich wspólnych publikacji[24].

Nie ulega wątpliwości, że spośród członków komisji powołanej w 1808 r. do oceny doniesienia o odkryciu przez Jędrzeja Śniadeckiego nowego pierwiastka, najlepszymi znawcami przedmiotu byli Fourcroy i Vauquelin. Obydwaj mieli za sobą już kilkuletnie doświadczenie w badaniach składu chemicznego surowej platyny. Analizą tego minerału zaczęli zajmować się w tym samym czasie, co Tennant i Wollaston. Wprawdzie nie do nich, a do ich brytyjskich kolegów należało odkrycie czterech nowych platynowców, ale obaj Francuzi po latach własnych badań mieli podstawy sądzić, że jakościowy skład surowej platyny nie kryje już żadnych niespodzianek[25]. Dlatego też niezbyt dziwi fakt, że w dokumentach paryskiej Akademii nie zachował się żaden zapis[26] wskazujący

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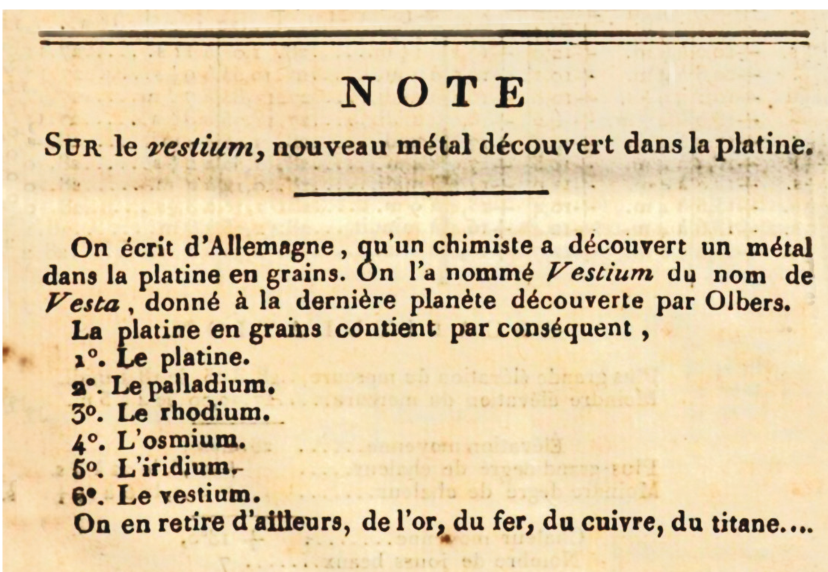
[23] *Ibidem*, ss. 278–279.
[24] Smeaton 1962, ss. 34–35.
[25] *Ibidem*, ss. 133–135.
[26] Sioda 2012, s. 204.

na to, aby ktokolwiek powtórzył wtedy opisaną przez Jędrzeja Śniadeckiego analizę. Wszyscy bowiem członkowie komisji, a w szczególności Fourcroy i Vauquelin byli przekonani, że znalezienie w ziarnach platyny czegoś, co byłoby jeszcze nieznane, mogło świadczyć tylko o pomyłce.

W świetle historii odkryć pierwiastków chemicznych rodzi się pytanie, dlaczego zajmujący się surową platyną brytyjscy i francuscy analitycy nie zidentyfikowali ostatniego platynowca, noszącego dziś nazwę rutenu. Pierwsza odpowiedź, jaka się nasuwa, to okoliczność, że oddzielenie rutenu od platyny jest zadaniem bardzo trudnym, z którym niekoniecznie umiano sobie wtedy poradzić. Inna odpowiedź, biorąca pod uwagę rodzaj badanych próbek, wydaje się wszakże bardziej prawdopodobna. Ziarna platynowe pozyskiwane z rozmaitych złóż różnią się bowiem znacznie zawartością rutenu – jedne zawierają go bardzo mało lub nie zawierają wcale; inne są w ten pierwiastek znacznie bogatsze. Najwięcej rutenu zawierają złoża występujące na Uralu. Czy to stamtąd właśnie pochodziła platyna, którą badał Jędrzej Śniadecki? Mogło tak być.

Tematem westu francuska prasa naukowa nie zainteresowała się niemal wcale. Krótką wzmiankę zamieścił lipcowy numer *Journal de Physique, de Chimie et d'Histoire Naturelle*.


NOTE

SUR le *vestium*, nouveau métal découvert dans la platine.

On écrit d'Allemagne, qu'un chimiste a découvert un métal dans la platine en grains. On l'a nommé *Vestium* du nom de *Vesta*, donné à la dernière planète découverte par Olbers.

La platine en grains contient par conséquent,

1°. Le platine.
2°. Le palladium.
3°. Le rhodium.
4°. L'osmium.
5°. L'iridium.
6°. Le vestium.

On en retire d'ailleurs, de l'or, du fer, du cuivre, du titane....

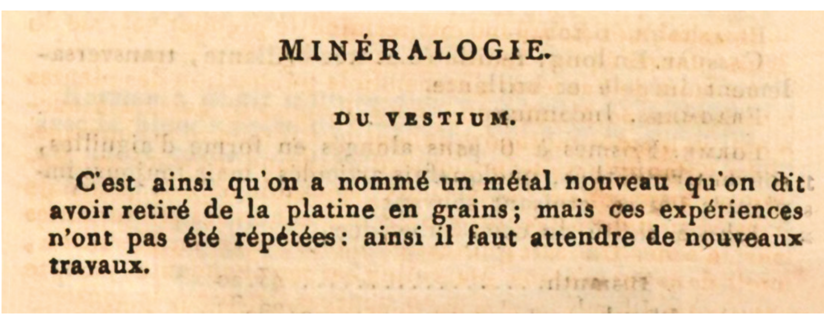
Ryc. 1. Wycinek z *Journal de Physique, de Chimie et d'Histoire Naturelle* 1808, t. 67, s. 71

W przekładzie na język polski brzmiałoby to mniej więcej tak:

> Donoszą z Niemiec, iż pewien chemik odkrył w ziarnistej platynie metal, który nazwał Vestium od nowej planety Vesta, odkrytej przez M. Olbersa. W ziarnistej platynie zawarte są więc następujące metale: 1. Platinum; 2. Palladium; 3. Rhodium; 4. Osmium; 5. Iridium; 6. Vestium. Niezależnie od tego: złoto, żelazo, miedź, tytan etc.[27].

Zważywszy, że tekst ten ukazał się w głównym organie paryskiej Akademii, nie sposób nie zauważyć dość zdumiewających nieścisłości: Śniadecki został tu potraktowany jako niemiecki chemik, którego nazwiska – trudnego w mowie i piśmie – nawet nie warto było przytaczać.

Identycznej treści informacja została powtórzona w *Archives des découvertes et des inventions nouvelles*[28]. Kilka miesięcy później, w następnym roczniku *Journal de Physique, de Chimie*… sprawa westu pojawiła się jako pierwsza wiadomość: „Du Vestium"[29] w sprawozdaniu z nauk fizycznych, w dziale „Mineralogia". Była to krótka, trzywierszowa wzmianka o tym, że zostało zgłoszone odkrycie nowego metalu w surowej platynie i że doświadczenia nie potwierdziły jego istnienia. Tam również nie znalazło się ani nazwisko odkrywcy, ani miejsce odkrycia, ani – kto wykonał sprawdzające doświadczenia. Podstawowy wówczas francuski periodyk chemiczny *Annales de Chimie* epizod odkrycia westu pominął natomiast całkowitym milczeniem.


MINÉRALOGIE.

DU VESTIUM.

C'est ainsi qu'on a nommé un métal nouveau qu'on dit avoir retiré de la platine en grains; mais ces expériences n'ont pas été répétées: ainsi il faut attendre de nouveaux travaux.

Ryc. 2. Wycinek z *Journal de Physique, de Chimie et d'Histoire Naturelle* 1809, t. 68, s. 29

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[27] *Journal de Physique, de Chimie et d'Histoire Naturelle* 1808, 67, s. 71.

[28] *Archives des découvertes et des inventions nouvelles* 1808, 1, s. 61.

[29] *Journal de Physique, de Chimie et d'Histoire Naturelle* 1809, 68, s. 29.

Bardziej powściągliwie do hipotetycznego odkrycia nowego pierwiastka odniosła się Cesarska Akademia Nauk w Petersburgu. Gdy w czerwcu 1808 r. wpłynęła francuska rozprawa Jędrzeja Śniadeckiego o Vestium (rękopis, prawdopodobnie jednobrzmiący z tym wysłanym do Paryża), Akademickie Zgromadzenie Petersburskiej Akademii Nauk przekazało tę rozprawę do zbadania członkowi Akademii – Jakowowi Dmitrewiczowi Zacharowowi (1765–1836). Ponieważ jednak Akademia w Petersburgu nie posiadała wówczas żadnej chemicznej pracowni, rosyjski uczony nie mógł doświadczalnie zweryfikować badań Śniadeckiego[30].

Francuskojęzyczny rękopis o odkryciu westu został odnotowany w wydawanych przez rosyjską Akademię *Mémoires de l'Académie Impériale des Sciences de St. Pétersburg* jako pozycja na liście komunikatów i innych rękopisów przedstawionych Akademii w latach 1807–1808[31]. Treść tej rozprawy natomiast została w całości przełożona na język rosyjski i opublikowana na łamach czasopisma *Tiechnołogiczeskij Żurnał*[32].

4. Ruthenium

Był rok 1828, kiedy niemiecki chemik i przyrodnik – Gottfried Wilhelm Osann (1796–1866), wówczas profesor chemii i farmacji w Dorpacie, ogłosił, że znalazł w surowej platynie aż trzy nowe pierwiastki[33], którym nadał nazwy: pluranium (od platyna z Uralu), ruthenium (od łacińskiej nazwy Rosji, Ruthenia), polinium (od greckiego *polia*, czyli szary). Osann blisko współpracował z Jönsem Jacobem Berzeliusem (1779–1848). Obaj badali skład chemiczny platyny pochodzącej z Uralu. Ponieważ Osannowi nie udało się otrzymać w postaci metalicznej ani jednego z tych nowych pierwiastków, a Berzelius zakwestionował wyniki jego analizy, dorpacki profesor wycofał się ze swoich ustaleń.

Od nieuznanego odkrycia westu minęło właśnie całe 20 lat. Dla rozwoju chemii było to bardzo wiele. Ugruntowała się w tym czasie atomistyczna teoria Johna Daltona (1766–1844), a straciła na znaczeniu tlenowa teoria kwasów Lavoisiera. Chemia analityczna wprowadziła

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30 Znaczko-Jaworski 1967.
31 Śniadecki 1810.
32 Śniadecki 1809 (cyt. za Siemion 2009, s. 102).
33 Sołowiew 1985, s. 144.

kilka nowych wskaźników kwasowo-zasadowych oraz dużą liczbę nowych odczynników. Znacznie powiększył się zasób stosowanych w analizie reakcji charakterystycznych. Praktyczne wykorzystanie praw stechiometrii zaowocowało możliwościami obliczania równoważników chemicznych (ekwiwalentów), co przyczyniło się do rozwoju analizy ilościowej. Powiększyła się także liczba pierwiastków, do których zaliczono nieznane wcześniej: Li (lit), Al (glin), Si (krzem), Se (selen), Br (brom), Cd (kadm), I (jod), Th (tor).

Jędrzej Śniadecki już wtedy nie zajmował się chemią. Od 1822 r. był na emeryturze i z dużym powodzeniem prowadził praktykę lekarską w Wilnie i okolicach. Kolegium Chemiczne przejął jego uczeń, Ignacy Fonberg (1801–1891), i kierował tą placówką aż do 1840 r., czyli do jej likwidacji wraz z całym Uniwersytetem przez rosyjskie władze zaborcze.

Fonberg był autorem znakomitego podręcznika chemii[34] oraz pierwszej w języku polskim encyklopedii chemicznej[35], a także wielu innych publikacji. Składem surowej platyny mało się interesował. Śniadecki do tego tematu również nie wracał, chociaż pozwalałoby na to wyposażenie uniwersyteckiego laboratorium. Że było ono w istocie bardzo dobre, świadczą pozycje *Inwentarza*[36] obejmujące kilka tysięcy przedmiotów. W spisie nie brakowało najnowocześniejszych przyrządów. Wiele z nich miało dużą wartość materialną. Także niektóre spośród odczynników i preparatów były nie tylko cenne naukowo, ale również kosztowne. Przeglądając ten *Inwentarz*, łatwo można zauważyć, że zawierał ponad 40 rozmaitych preparatów platynowych i metali towarzyszących platynie. Były wśród tych preparatów zarówno platynowce w postaci metalicznej (platyna, pallad, osm, rod, iryd), jak też ich stopy z innymi metalami, amalgamaty z rtęcią, a także wodorotlenki i liczne sole. Bardzo możliwe, że część tych preparatów pochodziła z zakupów, ale większość z pewnością była wykonywana we własnym zakresie przez Fonberga oraz jego doktorantów i współpracowników.

Platynowców ciągle było tylko pięć. Na odkrycie szóstego trzeba było czekać, aż chemia wypracuje doskonalsze metody analizy. Zabrało to niemal dwie następne dekady. Szósty platynowiec został odkryty dopiero w 1844 r. Dokonał tego Karl Ernst Claus (1796–1864), farmaceuta,

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34 Fonberg 1827–1829, t. 1; t. 2; t. 3.
35 Fonberg 1825.
36 Śniadecki, Fonberg, Sławiński 1938.

chemik i botanik, profesor chemii na Uniwersytecie w Kazaniu. Claus wyodrębnił nowy pierwiastek z surowej platyny pochodzącej z Uralu. Nazwał ten pierwiastek Ruthenium, nawiązując w ten sposób do badań wykonanych przez Osanna. Próbkę metalicznego rutenu przesłał cieszącemu się już wówczas światową sławą Berzeliusowi. Claus wyznaczył ciężar atomowy odkrytego pierwiastka i opisał jego właściwości[37].

5. Vestium vel Ruthenium lub Vestium versus Ruthenium

W chwili gdy Claus ogłaszał swoją pracę o rutenie, Jędrzej Śniadecki – przedstawiciel epoki poprzedniej, jeszcze pionierskiej – od siedmiu lat już nie żył, a o jego Vestium nikt nie słyszał lub nie chciał pamiętać. Jednakże trudno nie zauważyć, że uznane przez świat naukowy odkrycie rutenu było jednocześnie najlepszym potwierdzeniem dobrej naukowej intuicji Śniadeckiego. Czas pokazał, że to on, Śniadecki – nie z Niemiec, lecz z dalekiego Wilna – miał rację, gdy twierdził, że surowej platynie pochodzącej z niektórych złóż towarzyszy jeszcze jeden metal oprócz tych, które wyizolowali współcześni jemu brytyjscy chemicy.

Vestium vel Ruthenium – jak chcą jedni, lub Vestium versus Ruthenium – jak chcą inni, ma dziś już tylko wymiar ciekawostki, jakich w historii nauki wiele. Ciekawostki z rodzaju tych, które nie do końca wyjaśnione, pobudzają wyobraźnię i wciąż pozostają intrygujące dla badaczy.

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[37] W historii chemii, a także w historii wileńskiego Kolegium Chemicznego była to już zupełnie inna epoka. Gmach tej świetnej niegdyś placówki naukowo-badawczej przystosowano do innych funkcji. Audytorium przedzielono stropem usytuowanym w połowie wysokości, skutkiem czego powstały dwie kondygnacje, na których urządzono po kilka pomieszczeń (Kłos 1937, s. 143). Z biegiem czasu znajdowały w tym budynku swą siedzibę różne instytucje – najpierw mieścił się tam internat dla uczniów gimnazjum, później dom pracy i przytułek (Zahorski 1927, s. 37).

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Nieznane polonika kanadyjskie – William John Rose i jego archiwalna spuścizna

Abstrakt

Autor omawia archiwalną spuściznę Williama Johna Rose'a (1885–1968), kanadyjskiego slawisty, historyka i socjologa, pokazując jej przydatność do badań nad historią nauki oraz relacjami uczonych polskich z uczonymi z krajów anglosaskich. Ze względu na oddalenie Vancouver od Polski kolekcja zgromadzona w Archiwum Uniwersytetu Kolumbii Brytyjskiej nie była do tej pory przedmiotem zainteresowań polskich uczonych, warta jest jednak zauważenia ze względu na swoje bogactwo i różnorodność tematyczną.

Słowa kluczowe: *William John Rose, spuścizna, The University of British Columbia Archives, polonika, Kanada*

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Tomasz Pudłocki

Unknown Canadian Polonica – William John Rose and his archival legacy

Abstract

The author discusses the archival legacy of William John Rose (1885–1968), a Canadian Slavist, historian and sociologist, showing its usefulness in researching the history of science and the relations between Polish scholars and scientists from Anglo-Saxon countries. Due to the distance of Vancouver from Poland, the Rose Fond collected in the Archives of the University of British Columbia (Canada) has not been the subject of interest of Polish scholars so far, but it is worth noting due to its richness and thematic diversity.

Keywords: *William John Rose, archival legacy (fond), The University of British Columbia Archives, Polonica, Canada*

1. Wprowadzenie

Poznawanie kontaktów pomiędzy uczonymi oraz ich wzajemnych wpływów na siebie i wyniki swoich badań to jeden z bardziej aktualnych trendów w historiografii. Trudno się dziwić – tematyka ta związana jest z przepływem informacji czy szerzej wiedzy. Każda biografia jest poniekąd częścią portretu zbiorowego generacji reprezentowanej przez daną jednostkę i tym samym częścią historii komunikowania się. Co więcej, wziąwszy pod uwagę różne formy zdobywania doświadczenia naukowego i specyfikę pracy kilku formacji w ramach świata uniwersyteckiego, nie można mówić o jednym pokoleniu, które wpływa na badania i światopogląd uczonego – pozostaje on pod wpływem poglądów mistrzów (generacji starszej od siebie), kolegów (generacji współczesnej) oraz uczniów (zatem generacji młodszej). Zależności pomiędzy nimi to splot niekiedy wręcz niewiarygodnych okoliczności. Ta sama zależność dotyczy nauki polskiej – niepowstającej bowiem w próżni, ale w wyniku relacji uczonych zarówno w kraju, jak i poza jego granicami (zob. m.in.: Surman 2019, Górny 2017).

Wśród wielu interesujących zbiorów archiwalnych dotyczących kontaktów świata nauki polskiej z uczonymi zagranicznymi spuścizna, którą pozostawił po sobie William John Rose, jest jedną z najciekawszych i najmniej znanych. Pomimo iż nie brak rozsianych w różnych archiwach – polskich, kanadyjskich, brytyjskich i amerykańskich – śladów relacji Rose'a z kręgiem uczonych w krajach, gdzie mieszczą się te instytucje, kolekcja zgromadzona w Archiwum Uniwersytetu stanu Kolumbia Brytyjska w kanadyjskim mieście Vancouver (The University of British Columbia Archives) jest jedną z bogatszych, dokumentujących wzajemne powiązania naukowe Polski i krajów anglosaskich. Trudno się dziwić. Była przez kilkadziesiąt lat pieczołowicie gromadzona i tworzona przez W.J. Rose'a, zaś jego życie i zainteresowania naukowe z zakresu historii, literatury pięknej, socjologii, politologii, stosunków międzynarodowych spowodowały, że jest to cenne źródło do historii nauki.

Celem artykułu jest przybliżenie niemal zupełnie nieznanej spuścizny archiwalnej, która ze względu na oddalenie Vancouver od Polski nie była do tej pory w kręgu zainteresowań uczonych polskich[1]. Zwrócę zatem uwagę na tematyczny zasób zgromadzonych zbiorów oraz przedstawię przykłady nazwisk osób, których listy znajdują się w kolekcji. Często byli to bowiem przedstawiciele nauk humanistycznych i społecznych, którzy swoim dorobkiem oraz aktywnością w życiu publicznym czy politycznym wybijali się ponad przeciętność. Tym samym nie tylko odgrywali ważną rolę w zakresie zbliżenia Polski i świata anglosaskiego, ale także wpływali na edukację, politykę czy szeroko pojęte życie publiczne. To także doskonała okazja do przypomnienia, jak istotna jest znajomość zasobów archiwalnych dla poznawania historii nauki i biografistyki.

2. Twórca spuścizny – William John Rose (1885–1968)

William John Rose to znany badacz i działacz na rzecz zbliżenia Polski i krajów anglosaskich, aktywny przez kilka dziesięcioleci, głównie w I połowie XX w. Był postacią popularną i obecną w polskich mediach i życiu społecznym, kulturalnym, naukowym jako rzadki w tamtym czasie reprezentant świata anglosaskiego zainteresowany sprawami polskimi. Efekty tych dociekań pozostały w obszernym, choć niejednolitym dorobku naukowym, w którym wyróżnić można takie publikacje

[1] O ile mi wiadomo, poza autorem tego studium Polacy nie korzystali do tej pory z tych zbiorów.

jak: *Stanislas Konarski. Reformer of Education in XVIIIth Century Poland* (1929), *The Drama of Upper Silesia* (1936), *The Rise of Polish Democracy* (1944), *Poland Old and New* (1948) czy współautorstwo tomu 2 *The Cambridge History of Poland* (1941). Do tego trzeba dodać co najmniej kilkadziesiąt artykułów naukowych i liczne teksty popularne, drukowane na przestrzeni pięciu dekad.

Nie miejsce tu na dokładne przedstawienie życiorysu Rose'a. Warto jedynie wspomnieć, że był on Kanadyjczykiem, absolwentem filologii klasycznej uniwersytetów w Manitobie i w Oksfordzie. Dodatkowe studia w Niemczech i zaangażowanie w pracę metodystów na pograniczu polsko-czeskim spowodowały, że w 1914 r. znalazł się w Ligotce Kameralnej (Komorní Lhotka – obecnie w Republice Czeskiej). Internowany w czasie I wojny światowej na Śląsku Cieszyńskim, zaczął interesować się sprawami polskimi, od tego czasu stając się jednym z niewielu Anglosasów profesjonalnie się nimi zajmujących. Już od listopada 1918 r. zaangażował się w prace promujące sprawę polską w czasie paryskich obrad pokojowych. Po wojnie pozostał w Polsce jako dyrektor krakowskiego YMCA (Young Men's Christian Association – Związek Chrześcijańskiej Młodzieży Męskiej), podejmując studia uzupełniające najpierw w Warszawie, a następnie w Krakowie. Był pierwszym Anglosasem od czterystu lat, który otrzymał doktorat na Uniwersytecie Jagiellońskim, co już samo w sobie wzbudziło ogromne zainteresowanie jego osobą. W latach 1927–1935 był wykładowcą socjologii w Dartmouth College w mieście Hanover, w amerykańskim stanie New Hampshire. Od 1936 r. był wykładowcą, a od 1939 r. dyrektorem w londyńskiej School of Slavonic and East European Studies. W czasie II wojny światowej był zaangażowany w pomoc dla polskich uczonych i w promowanie sprawy polskiej w Wielkiej Brytanii. Po powrocie w 1950 r. do Kanady był m.in. wykładowcą uniwersytetu w Vancouver i de facto jednym z założycieli profesjonalnych studiów slawistycznych w tym kraju.

Już nawet tak pokrótce zarysowany życiorys pokazuje, że Rose nie mógł pozostać poza zainteresowaniem historiografii, zarówno polskiej, jak i obcej (Zięba 1989–1991; Stone, Starnawski 1994; Kwiatek 2008; Pudłocki 2015; 2018). Trudno się dziwić – to postać, która dla historii nauki jest ważna, m.in. poprzez pozostawione po sobie prace, traktujące o historii, socjologii, historii kultury i historii nauki. Za życia był uważany za ambasadora spraw polskich w krajach anglosaskich, a także spraw anglosaskich w Polsce. Poświęcano kanadyjskiemu uczonemu albo obszerne biogramy, albo szczegółowe prace odnoszące się do wycinków z jego biografii. Do tej pory nikt nie podjął się napisania jego biografii, może m.in. ze względu na rozległość zainteresowań, różnorodność przedsięwzięć, w które się angażował, jak i fakt, że materiały do jego życia rozproszone są po kilku archiwach północnoamerykańskich i europejskich[2].

3. William Rose Fond

Spuścizna W.J. Rose'a przechowywana jest w archiwum University of British Columbia w kanadyjskim mieście Vancouver. Opracowana została w 2004 r. przez Erwina Wodarczaka w formie inwentarza z niewielkim wstępem. Liczy 1,06 metra bieżącego i obejmuje sześć pudeł (23 tomy). Składają się na nią głównie materiały dokumentujące życie i karierę zawodową W.J. Rose'a, w językach polskim, angielskim i francuskim (rzadziej niemieckim lub szwedzkim). Zgromadzone archiwalia odnoszą się do kontaktów Rose'a z osobami z następujących krajów: Polska, USA, Wielka Brytania, Kanada, ale też Francja, Czechosłowacja, Niemcy, Sudan – nie są zatem ograniczone jedynie do relacji Polski i krajów anglosaskich.

Układ spuścizny jest następujący: korespondencja, materiały drukowane, notatki (z lat 1910–1958), notatniki i dzienniki (z lat 1914–1939), rękopisy, tłumaczenia, wywiady, wycinki prasowe, publikacje o W.J. Rosie (z lat 1904–1963), prace innych autorów, dokumenty dotyczące rodziny Rose'a (z lat 1861–1968), fotografie (łącznie 35 sztuk).

Spuścizna składa się głównie z korespondencji (z lat 1900–1966; przede wszystkim wpływającej, choć niekiedy wychodzącej, zawiera też bowiem szkice czy kopie listów Rose'a do różnych osób). Ponadto obejmuje dzienniki z lat 1895–1939 (choć nie prowadzone przez cały czas, lecz tylko w niektórych okresach), szkice czy pierwsze redakcje tekstów pisanych przez Rose'a (z lat 1924–1957), całe artykuły, drukowane w różnych wydawnictwach i periodykach (m.in. publikowane przez

[2] Materiały zgromadzone są m.in. w archiwum Uniwersytetu w Manitobie, w archiwum School of Slavonic and East European Studies w Londynie, w archiwum Fundacji Kościuszkowskiej w Nowym Jorku i in. Omawiana w artykule spuścizna była zbierana przez Rose'a i stanowi głównie zbiór archiwalny do poznawania jego życia i działalności naukowej.

Rose'a pod pseudonimem Jan Różycki z lat 1909–1968), raporty i sprawozdania pisane przez Rose'a (z lat 1918–1966), tłumaczenia na język angielski (głównie z j. polskiego; lata 1920–1941), poezję, fragmenty wykładów, wycinki gazet, fotografie, a także dwie wersje maszynopisu wspomnień pt. *Thirty Wander Years*[3].

Pudło nr 1 obejmuje przede wszystkim korespondencję (teczki od 1.1 do 1.29), artykuły opublikowane przez Rose'a (teczki 1.30–1.43) oraz pisane przez niego raporty i sprawozdania (teczki 1.44–1.54). W ramach „artykułów publikowanych przez Rose'a" włączono wycinki z gazet z artykułami Rose'a, jego drukowane listy, broszury czy całe artykuły zarówno z periodyków naukowych, jak i czasopism codziennych. W pudle drugim zamieszczono pozostałą część sprawozdań (teczki 2.1–2.5), w tym z działalności londyńskiej School of Slavonic and East European Studies z lat 1937–1947 (teczka 2.1) i raporty pisane w latach 1937––1948 na potrzeby Ambasady RP w Londynie (teczka 2.2). Pozostałą zawartość pudła stanowią notatki z socjologii, historii kultury, cywilizacji, poezji, ekonomii oraz wykłady i prelekcje z różnych dziedzin i na rozmaite tematy, m.in. o Conradzie, reformacji w Polsce, refleksje na tematy kanadyjskie itd. (teczki 2.6–2.57). W pudle trzecim znajdują się dzienniki w formie dziesięciu notatników z lat 1914–1933 oraz dziennik z podróży po Rumunii w roku 1939 (teczki 3.1–3.2), a także rękopisy i maszynopisy obejmujące różne zagadnienia (wśród nich są tematy dotyczące historii i fenomenu Łodzi, Warszawy, Poznania, Wilna, Torunia, Śląska, pisana przez Rose'a poezja czy w końcu cztery części wspomnień (teczki 3.3–3.26)). Ponadto na zawartość pudła nr 3 składają się tłumaczenia (3.27–3.33), recenzje książek Rose'a (3.34–3.38), publikacje o Rosie, głównie wycinki z gazet i periodyków, odnoszące się do jego życia i działalności (3.39–3.41), a także broszury różnych autorów, obejmujące dzieła wydane w latach 1874–1961 (3.42–3.45).

Trzy pozostałe pudła nie są już tak obszerne. Pudło nr 4 obejmuje wycinki prasowe dotyczące głównie kwestii religijnych, YMCA, Polski, wybranych książek, a także różne materiały dotyczące spraw kościelnych, od publikacji, przez notatki, a kończąc na korespondencji z lat 1932––1963 (teczki 4.1–4.9). Pudło nr 5 zawiera varia. Wśród zgromadzonych

[3] Wspomnienia te zostały wydane pod tytułem: *The Polish Memoirs of William John Rose* – zob. Stone 1975.

tam materiałów są broszury dotyczące Polski, poetów wiktoriańskich, a przede wszystkich liczne wycinki na rozmaite aktualne tematy z lat 1946–1967 (teczki 5.1–5.9). Ponadto na zawartość tego pudła składają się dokumenty dotyczące rodziny Rose'ów, w tym drobne prace drukowane, listy różnych członków rodu oraz drobne pamiątki (teczki 5.10–5.13). Ostatnie pudło, nr 6, odnosi się głównie do pozostałości po samym Rosie. Na jego zawartość składają się różne jego dokumenty, jak certyfikaty, zaświadczenia, dyplomy, drobne rzeczy o pamiątkowej wartości, listy gratulacyjne z różnych okazji, reprodukcje sztuki czy list od brata Arthura (teczki 6.1–6.6). Dwie ostatnie teczki tego pudła obejmują fotografie – głównie Rose'a lub bezpośrednio go dotyczące.

4. Wątki tematyczne i osoby występujące w spuściźnie

Pomimo iż z układu spuścizny, jak i z tytułów poszczególnych części ją tworzących, badacz może zorientować się w zawartości, to dopiero dokładne przejrzenie materiałów pozwala dostrzec bogactwo zgromadzonych archiwaliów i potencjalną tematykę, do której mogą być one wykorzystane. Naturalnie związana jest ona z życiem i działalnością twórcy spuścizny, choć obejmuje dość szerokie konteksty.

Pierwszą większą całością problemową, jaką można wyłonić ze zbioru, są materiały dotyczące początków akcji misyjnej kanadyjskich metodystów na ziemiach ówczesnej Galicji (1910–1914). Są to głównie listy pisane do Rose'a i dotyczą Śląska Cieszyńskiego, Krakowa i Przemyśla. Pisane były przez młodych metodystów, Kanadyjczyków, którzy spędzili kilkanaście miesięcy na obszarze polsko-czeskim i w Galicji. Edmund Chambers i Arthur O. Rose (brat Williama) dzielili się w nich swoimi wrażeniami i spostrzeżeniami z życia codziennego oraz pracy duszpasterskiej (zob. m.in. Stone 1975, Pudłocki 2016; 2018).

Krąg rodzinny i przyjacielski to drugi tematyczny wątek widoczny w spuściźnie – rozsiany po różnych jej częściach. Obejmuje materiały dotyczące rodziny Rose (od lat 60. XIX w. do lat 60. XX w.), środowiska studenckiego w kanadyjskiej Manitobie i angielskim Oksfordzie (kartki, programy imprez, zaproszenia na rozmaite wydarzenia kulturalno-naukowe), a także zaproszenia na przyjęcia, spotkania, listy od członków rodziny i znajomych, wysyłane przez kolejne dziesięciolecia długiego życia Rose'a. Większą część tworzą dwa cykle: gratulacje po doktoracie oraz wyrazy współczucia po śmierci żony.

Można też znaleźć sporo materiałów na temat historii YMCA w Polsce (ze szczególnym uwzględnieniem oddziału krakowskiego). Są to raporty z działalności, informacje na temat organizowanych obozów i szkoleń czy korespondencja zarówno ze światowymi liderami tej organizacji (np. z Johnem R. Mottem, późniejszym laureatem Pokojowej Nagrody Nobla), jak i tymi, którzy byli kluczowi dla polskiej YMCA (Frank Savery, Edmund Chambers, Paul Super). Chronologicznie obejmują one głównie pierwszą połowę lat 20. XX w. (Stone 1975).

Lata te to nie tylko czas, kiedy Rose był jednym z dyrektorów YMCA – równocześnie studiował wtedy w Warszawie i Krakowie. Okres jego studiów doktorskich to kolejne istotne, choć niewielkie objętościowo zagadnienie. Materiały z tej tematyki obejmują dokumenty uniwersyteckie (indeksy), przepustki pozwalające przemieszczać się w różnych obszarach, plakaty z wykładów Rose'a oraz mowę profesora Stanisława Kota, wygłoszoną podczas promocji doktorskiej 23 V 1926 r. w auli Collegium Novum Uniwersytetu Jagiellońskiego (Pudłocki 2015). Materiały te to doskonałe źródło do biografistyki, ale i do historii nauki – pokazują bowiem aktywność naukową młodego uczonego i pozwalają lepiej zrozumieć jego związki z nauką krakowską w późniejszych latach.

Ta problematyka łączy się bezpośrednio z listami licznych uczonych, z którymi Rose przez dziesięciolecia swego długiego życia prowadził korespondencję. Niekiedy to ledwie jedna czy dwie karty archiwalne, ale nierzadko listy są dużo obszerniejsze i obejmują minimum kilka lat znajomości. Polscy uczeni reprezentowani w spuściźnie to przede wszystkim przedstawiciele krakowskiego świata nauki, choć poza nimi nie brak osób i z innych środowisk. Wśród licznych wątków, które przebijają z treści listów, wiele odnosi się do Krakowa, Uniwersytetu Jagiellońskiego czy Polskiej Akademii Umiejętności. Dotyczą organizacji nauki w Polsce i w poszczególnych krajach, porad fachowych i próśb o wyjaśnienie kwestii terminologicznych czy naukowych (historycznych, socjologicznych, literackich, społecznych, politycznych), życia codziennego i bieżących spraw, czytanych lektur czy współpracy naukowej. Z przyczyn oczywistych wielu polskich uczonych czy młodych adeptów nauki było zainteresowanych możliwościami wyjazdu do krajów anglosaskich lub współpracą z naukowcami z tych obszarów. Wśród nich wyróżnić można takie osoby jak: Stanisław Estreicher, Stanisław Kot, Roman Dyboski, Julian Krzyżanowski, Wincenty Lutosławski, Florian Znaniecki, Adam Żółtowski, Tadeusz Mitana, Oskar Halecki, Wacław Borowy, Marian Kukiel, Jan Stanisławski, Władysław Konopczyński (Stone 1975, Pudłocki 2015).

Bogata epistolografia w spuściźnie Rose'a to również przykłady jego relacji ze światem uczonych z Wielkiej Brytanii, Stanów Zjednoczonych Ameryki oraz jego rodzinnej Kanady – są to klasyczne materiały, które powinny zainteresować historyków nauki. Listy obejmują różne lata, niemal sześć dziesięcioleci XX w., choć liczbowo wzrastają wraz z objęciem przez niego w 1936 r. posady wykładowcy, a potem dyrektora londyńskiej School of Slavonic and East European Studies. W ramach uniwersytetu londyńskiego Rose zmuszony był prowadzić różne bieżące sprawy podległej mu placówki, korespondować z kolegami w kwestiach organizacyjnych, naukowych czy studenckich. Wśród nazwisk, których listy i kartki przechowuje się w spuściźnie kolekcji, są tacy uczeni jak: Bernard Pares, Robert William Seton-Watson, Gleb Struve, Edwin Diller Starbuck. Ze względu na trudny czas II wojny światowej i katastrofalne położenie Polski i Polaków, wśród materiałów zachowały się przykłady zaangażowania Rose'a na rzecz sprawy polskiej (liczne inicjatywy kulturalno-naukowe, wystawy, publikacje). Z uwagi na fakt, że twórca spuścizny był stypendystą Royal Institute of International Affairs w Londynie, w jego spuściźnie można znaleźć korespondencję z osobami związanymi z tą instytucją. Materiały dotyczą także tłumaczenia słynnych wspomnień Jana Słomki[4] czy relacji z przedstawicielami Ambasady RP w Londynie, zwłaszcza ambasadorem Edwardem Raczyńskim, z którym utrzymywał kontakty prywatne (Kania 2014, s. 137; Pudłocki 2015).

W spuściźnie znajdują się listy od przedstawicieli licznych uczelni spoza środowiska londyńskiego, jak np. George Rapall Noyes, Eric P. Kelly, Arnold J. Toynbee, Arthur P. Coleman, Norman A.M. MacKenzie, Clement Charles Julian Webb, Stephen P. Duggan, Marek Wajsblum, William F. Reddaway. Korespondencja dotyczy wykładów, publikacji, konsultacji na przeróżne tematy, a także pracy Rose'a na Uniwersytecie Kolumbii Brytyjskiej. Są jednak wyjątki, np. Harold Gordon Skilling radził się Rose'a w sprawach Czechosłowacji, mimo iż ten tłumaczył, że kraj ten od lat znajdował się poza obszarem jego

[4] Opublikowano pt. *From Serfdom to Self-government. Memoirs of a Polish Village Mayor* [*Jan Slomka*], *1842–1927* – zob. Kot, Rose 1941.

zainteresowań. Z kolei Edward Johns Urwick, rektor University of Toronto, próbował ściągnąć Rose'a do pracy w podległej mu placówce. Wiele spraw naukowych obejmuje też korespondencja z Canadian Association of Slavists. Oprócz uczonych, w spuściźnie zachowały się listy od dziennikarzy, literatów, polityków czy pastorów, ponieważ różne kościoły i związki wyznaniowe były przez lata zainteresowane współpracą z Rose'em (Stone 1975).

Świat nauki reprezentowany w spuściźnie Rose'a nie ogranicza się tylko do krajów anglosaskich. Takim przykładem jest interesująca korespondencja z francuskim slawistą André Mazonem z Institut d'études slaves Uniwersytetu Paryskiego. Jednak to krąg anglo-amerykański dominuje w zbiorach zgromadzonych w Vancouver. Warto podkreślić fakt, że spośród licznych instytucji polonijnych Rose zachował materiały odnoszące się do Fundacji Kościuszkowskiej w Nowym Jorku i listy wymieniane z jej sekretarzem generalnym, a potem prezesem zarządu – Stevenem Mizwą (Stefanem Mierzwą). Dotyczą one wydania książki Rose'a o Stanisławie Konarskim, współpracy z Alliance College w Cambridge Springs (którego Mizwa był krótko rektorem), odczytów i akcji promujących Polskę (m.in. w okresie pracy w Dartmouth College), w tym książki o Górnym Śląsku, czy wydawnictwa *Great Men and Women of Poland*. Korespondencja z czasów II wojny światowej dotyczy pomocy dla polskich uczonych oraz wymiany myśli o tym, co działo się w Polsce w tym czasie (Pudłocki 2013).

Ostatnim interesującym zagadnieniem tematycznym, które można znaleźć w spuściźnie, są materiały odnoszące się Śląska Cieszyńskiego i Górnego Śląska – a więc tych regionów Polski, które Rose znał najlepiej i które badał naukowo. Są to różnego rodzaju druki (artykuły, broszury, wycinki z prasy), korespondencja z przyjaciółmi z tych stron (ostatnie listy z lat 60. XX w.; ostatnia wizyta w Polsce w 1947 r.) czy takimi postaciami jak wojewoda śląski Michał Grażyński i dyrektor Syndykatu Blachy Cynkowej w Katowicach, a wcześniej konsul generalny w Chicago Aleksander Szczepański (Pudłocki 2018).

5. Zakończenie

To, że Rose był za życia łącznikiem pomiędzy Polską i światem anglosaskim, zostało już dawno udowodnione w historiografii (Stone 1975, Zięba 1989–1991). Wydaje mi się jednak, że nie sądzono, iż pozostawił on dla badaczy historii nauki aż tyle materiałów, świadczących o bogactwie i różnorodności podejmowanych inicjatyw mających na celu przybliżenie tych dwóch światów. Spuścizna, która została zdeponowana, a obecnie jest udostępniana w archiwum Uniwersytetu Kolumbii Brytyjskiej, z pozoru dotyczy jednej osoby, a przecież jej zawartość odnosi się do wydarzeń i postaci ze świata historii, literatury, socjologii, filozofii, polityki, dziennikarstwa czy historii nauki na przestrzeni kilku dziesiątków lat XX w. Swoją zawartością może zatem zainteresować przedstawicieli wielu nauk, piszących na rozmaite tematy, a których celem jest pokazywanie wzajemnych powiązań i współpracy Polski, krajów anglosaskich oraz wielu innych państw.

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The divergent histories of Bose-Einstein statistics and the forgotten achievements of Władysław Natanson (1864–1937)

Abstract

This article investigates the forgotten achievements of Władysław Natanson (1864–1937) related to the creation of Bose-Einstein statistics.

The introductory part of the article presents considerations regarding the methodology of history and the history of exact sciences, and then the divergent research perspectives that can be taken in the description of the history of Bose-Einstein statistics, as well as the author's integrated approach to this issue, which eliminates the disadvantages of these divergent views.

This integrated approach is then used to describe the achievements of Władysław Natanson related to the creation of Bose-Einstein statistics.

These achievements are presented against the background and in the context of discussions which – relatively sporadically –

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took place among various groups of researchers: historians and philosophers of science, physicists, sociologists of scientific knowledge in the 20th and 21st centuries.

These discussions have now been reordered here. They are followed by a presentation of the complete list of Natanson's publications regarding the subject. Also shown is his strategy to quote reliably the bibliography with regard to the explanation of the distribution of blackbody radiation and related issues.

Additionally, a list of scientists who knew Natanson's publications has been supplemented in the article and the precursorship of Natanson's achievements is explained. This is followed by a rebuttal of many erroneous or simplified statements about him and his achievements.

The already well-known terminological conventions have been recalled: "Bose statistics" and "Bose-Einstein statistics", as well as recently introduced: "Planck-Bose statistics" (1984), "Natanson's statistics" (1997)", "Natanson-Bose-Einstein statistics" (2005), "Planck-Natanson-Bose-Einstein statistics" (2011), and "Natanson statistics" (2013).

New terminological conventions have been introduced: "Boltzmann-Planck-Natanson statistics" and "Boltzmann--Planck-Natanson-Bose-Einstein statistics".

A side effect of this research is a discovery that Robert K. Merton – the author of the label 'Matthew effect' – chose the name of the effect using erroneous premises and the effect should therefore be named after its actual discoverer.

The article is accompanied by four appendixes: the first presents reflections on the methodology of historiography and historiography of exact sciences, the second – a commentary on the use of the terms: "Bose statistics", "Bose-Einstein statistics", "Einstein-Bose statistics" and "Planck-Bose statistics", the third – a very important letter by Max Planck to Władysław Natanson (of 25 January 1913), and the fourth – the excerpts of two letters from Sommerfeld to Rubinowicz (of 1 October 1919 and 1 November 1919).

Keywords: *Władysław Natanson*, *Ladislav Natanson*, *distribution of blackbody radiation*, *Bose statistics*, *Bose-Einstein statistics*, *Planck-Bose statistics*, *Natanson statistics*, *Natanson-Bose-Einstein statistics*, *Planck-Natanson-Bose-Einstein statistics*, *Boltzmann-Planck-Natanson statistics*, *Boltzmann-Planck-Natanson--Bose-Einstein statistics*, *divergent histories*, *integrated approach*, *precursorship*, *Matthew effect*, *R.K. Merton effect*, *methodology of history*, *methodology of the history of exact sciences*

Rozbieżne historie statystyki Bosego-Einsteina i zapomniane osiągnięcia Władysława Natansona (1864–1937)

Abstrakt

Artykuł bada zapomniane osiągnięcia Władysława Natansona (1864–1937) związane z powstaniem statystyki Bosego-Einsteina.

W części wstępnej artykułu wskazano rozbieżne perspektywy badawcze, jakie przyjmowano w opisie historii statystyki Bosego-Einsteina, a także autorskie zintegrowane ujęcie tego zagadnienia, które eliminuje wady tych rozbieżnych perspektyw.

Wspomniane zintegrowane ujęcie zastosowano następnie do opisania osiągnięć Władysława Natansona (1864–1937), związanych z powstaniem statystyki Bosego-Einsteina.

Dokonania Natansona przedstawiono na tle i w kontekście dyskusji, jakie toczyły się (stosunkowo sporadycznie) wśród różnych grup badaczy: historyków i filozofów nauki, fizyków, socjologów wiedzy naukowej w XX i XXI w.

Dyskusje uporządkowano oraz przedstawiono kompletną listę publikacji Natansona dotyczących omawianego zagadnienia. Wskazano także strategię rzetelnego cytowania przez Natansona bibliografii dotyczącej wyjaśnienia rozkładu promieniowania ciała doskonale czarnego i pokrewnych zagadnień; uzupełniono listę naukowców, którzy znali publikacje Natansona; skorygowano wiele błędnych lub uproszczonych stwierdzeń na temat Natansona i znaczenia jego publikacji, wyjaśniono kwestię prekursorstwa jego osiągnięć etc.

Przypomniano już znane konwencje terminologiczne: „statystyka Bosego" i „statystyka Bosego-Einsteina", jak również niedawno wprowadzone: „statystyka Plancka-Bosego" (1984), „statystyka Natansona" (1997, 2013), „statystyka Natansona-Bosego-Einsteina" (2005) oraz „statystyka Plancka-Natansona-Bosego-Einsteina" (2011).

Wprowadzono nowe konwencje terminologiczne: „statystyka Boltzmanna-Plancka-Natansona" i „statystyka Boltzmanna-Plancka-Natansona-Bosego-Einsteina".

Skutkiem pobocznym tych badań jest odkrycie, iż socjolog Robert K. Merton – autor określenia „efekt św. Mateusza" –

wybrał tę nazwę, posługując się błędnymi przesłankami i dlatego należy nazywać ten efekt nazwiskiem jego faktycznego odkrywcy.

Do artykułu dołączone są cztery dodatki: pierwszy przedstawia rozważania z zakresu metodologii historii i historii nauk ścisłych, drugi – komentarz dotyczący użycia terminów: „statystyka Bosego", „statystyka Bosego-Einsteina", „statystyka Einsteina-Bosego" oraz „statystyka Plancka-Bosego, trzeci – bardzo ważny list Maxa Plancka do Władysława Natansona z 25 stycznia 1913 r., a czwarty – fragmenty dwóch listów Sommerfelda do Rubinowicza z 1 października 1919 i 1 listopada 1919 r.

Słowa kluczowe: *Władysław Natanson*, *rozkład promieniowania ciała doskonale czarnego*, *statystyka Bosego*, *statystyka Bosego-Einsteina*, *statystyka Einsteina-Bosego*, *statystyka Plancka-Bosego*, *statystyka Natansona*, *statystyka Natansona-Bosego-Einsteina*, *statystyka Plancka-Natansona-Bosego-Einsteina*, *statystyka Boltzmanna-Plancka-Natansona*, *statystyka Boltzmanna-Plancka--Natansona-Bosego-Einsteina*, *rozbieżne historie*, *zintegrowane podejście*, *prekursorstwo*, *efekt św. Mateusza*, *efekt* R.K. *Mertona*, *metodologia historii*, *metodologia historii nauk ścisłych*

1. The divergent perspectives in studying the history of Bose-Einstein statistics and a postulate for an integration of research[1]

To structuralize better our considerations on the history of Bose-Einstein statistics and Natanson's contribution to it, it is important to formulate here some remarks of a general nature.

Firstly, the scholars who researched the so-called Bose-Einstein statistics dealt with problems in physics and applied mathematics (statistics).

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[1] The subject-matter of this article was analyzed by the author in several previous works: Kokowski 2009 (in Polish, only on p. 92, and fn. 3); 2011a and 2011b (I gave a lecture in English during a Prague conference and then a summary of the lecture and a presentation were published in the proceeding of the conference, but only in the CD-ROM version; these works were not reviewed, and did not receive DOI numbers); 2015 (I gave a lecture in English, but the lecture was not published in print or online). In consequence, one cannot find these works in print or online, and my views on this subject are not known to specialists. To remedy this, I present this article, which not only systematizes, but also greatly expands my previous analyses.

Secondly, these scholars functioned in certain scientific communities and thought collectives.[2] Their achievements are dependent on their talents and participation in scholar traditions or thought styles, because nobody can achieve success in science if they do not *stand on the shoulders of giants*.[3]

Thirdly, the creativity of scientists is measured by the quality of their publications, but an evaluation of these publications is not an easy matter. We can assume safely that the historical approach can be useful in this task. However, we cannot rule out *a priori* that the tools of scientometrics (which is a measurement of the development of science using mathematical tools, including a measurement of impact of scientific publications by a simple citation counting) can be useful too.

Fourthly, while our analyses of the context of justification must play an important part in our consideration, we must not neglect the importance of the context of discovery, since both these contexts interlace with each other in everyday practice of scientists.

Fifthly, between the justification and the discovery there is a whole intermediate field to persuade recipients (other scientists as well as a broader audience), and to mediate between different scientific camps; as a result we cannot neglect the rhetorical aspect of considerations, interactions between thought collectives, different interests of scientific camps including their political views, etc.

Sixthly, the history of Bose-Einstein statistics can be interpreted from divergent points of view that originate from such diverse disciplines as, for example: the teaching of physics, the history and philosophy of physics (and, generally, of the exact sciences), the sociology of scientific knowledge, the psychology of scientific discovery and scientometrics.

I suggest that these primary divergent points of view be treated as complementary perspectives of an integrated approach. And, from my point of view, the only reasonable approach to study the problem is to assume a certain thematic hierarchy of these points of view (in other words, these points of view are not important in the same sense). Firstly, we must carry on an extensive, detailed internal analysis of the development of scientific ideas (including the so-called philosophy in science).

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[2] Fleck 1935/1979.

[3] R.K. Merton 1965 (2nd ed. 1985; 3rd ed. 1993); *Wikipedia* 2019f; Kokowski 2012, pp. 57–58.

Then, we can look for the additional so-called 'external' explanations (originating from the philosophy of science, the sociology of scientific knowledge, the scientific rhetoric, political views, psychology, general philosophy, etc.).[4]

Seventhly and finally, when we want to write about the history of Bose-Einstein statistics, we should notice the two main approaches applied by researchers: a) the canonical approach, which is based on its "history" as seen by the authors of textbooks on statistical physics, and b) the historical approach. The latter gives us possibility of studying both, the so-called internal history of science, and the external one. We can thus study the internal history of physics (linked with the internal philosophy of physics), and the external history of physics. Thereby, the external history of physics is open to questions stemming from historical contexts defined by politics and philosophy, the sociology of scientific knowledge, the scientific rhetoric, the psychology of scientific discovery, as well as scientometrics. I am an advocate of the integrated approach linking in a hierarchical way both the internal and external factors.[5]

2. The different approaches in studying the history of Bose-Einstein statistics and Natanson's achievements

2.1. The canonical approach, the university textbooks and Natanson

In the canonical interpretation of Bose-Einstein statistics there is no problem pointing to the real discoverers: they were simply Satyendra Nath Bose (1894–1974) and Albert Einstein (1879–1955), and nobody else. It is sufficient to look at the table below and compare only two formulas describing the distribution of particles over energy states for the

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[4] This integrated approach stems from the progress of history of science and philosophy of science, and sociology of scientific knowledge in the 20th century. It transcends the opposition of the ideas of "internal history of science" and "external history of science".

[5] Regarding the methodology of historiography and historiography of science assumed by the article author, see Appendix 1.

two statistcs, to quote the articles of S.N. Bose 1924a (reprinted 2009a; English transl. 2009b); 1924b (reprinted 2009c; English transl. 2009d) and A. Einstein 1924 (reprinted 2015a, Doc. 283; English transl. 2015b, Doc. 283); 1925a (reprinted 2015a, Doc. 385; English transl. 2015b, Doc. 385); 1925b (reprinted 2015a, Doc. 427; English transl. 2015b, Doc. 427), as well as to show the textbooks, e.g. L.D. Landau, E.M. Lifshitz (1937–1939; ed. 1975, §54, pp. 180–181) / (English transl. 1958, §54, pp. 153–154); F. Hund (1956, § 92); K. Huang (1963, chapter 12); R.P. Feynman (1972, chapter 1.9) or *Wikipedia* (2019b).[6]

Table 1. The distribution of particles over energy states

Bose statistics (1924)	Bose-Einstein statistics (1924–1925)
$\frac{n_i}{g_i} = \frac{1}{\exp\left(\frac{\varepsilon_i}{k_B T}\right) - 1}$	$\frac{n_i}{g_i} = \frac{1}{\exp\left(\frac{\varepsilon_i - \mu}{k_B T}\right) - 1}$
where: n_i – population number of (indistinguishable) particles with energy ε_i, g_i – number of (distinguishable) sub-levels, n_i/g_i – probability of occupation level i with energy ε_i, μ – chemical potential.[7]	

2.2. The internal history (and philosophy) of physics[7]

Changing the research perspective by using other "glasses" (i.e. other interpretative tools), which are sensitive to a detailed historical research, brings out new epistemic results. Thanks to the thoroughgoing

[6] For a genesis and description of Bose's and Einstein's works, see Chapter 14 "Satyendra Nath Bose, Bose-Einstein-Statistics, and the Quantum Theory of an Ideal Gas" in: Mehra 2001, pp. 501–545.

[7] In the canonical approach we ignore historical details and talk about "particles" irrespective of the fact whether we consider imponderable matter (quanta of light) or ponderable matter.

It is noteworthy that the formulas mentioned in the Table 1 are linked by a correspondence principle: for the limiting case, when the correspondence parameter "$\mu/k_B T$" tends to zero, numeric values (predictions) of the second formula goes to the works by, among others, M. Jammer (1966), J. Mehra, H. Rechenberg (1982–2000), S. Bergia (1987), A. Bach (1988; 1990), S. Varró (2006a, pp. 1–34; 2006b; 2007), we know that the history of the so-called Bose-Einstein statistics is very complicated and many scientists played important roles in it.

We can distinguish three subsequent main stages in this history: the first (preparatory) stage from the formulation of the laws of electrodynamics and the principles and laws of statistical physics but before formulation of the black-body radiation law; the second stage, the formulation of the black-body radiation law, and the third stage, the explanation of the black-body radiation law and the formulation of the so-called Bose statistics and then of its generalization Bose-Einstein statistics.[8]

In the first (preparatory) stage the laws of electrodynamics and the principles and laws of statistical physics were formulated, including the entropy-probability relationship found by Boltzmann (see Bach 1988; 1990, p. 2), the Maxwell-Boltzmann distribution, and the Wien-Jeans law of radiation.

In the second stage Max Planck (1900a) discovered the black-body radiation law. The law is described by the following formulas:

Table 2. The black-body radiation law

$$\varrho_{\nu}(\nu,T)=8\pi\nu^{2}c^{-3}\frac{h\nu}{\exp\left(\frac{h\nu}{kT}\right)-1},$$

numeric values (predictions) of the first formula. This is not an accidental feature and no anachronism. It is a manifestation of applying the hypothetico-deductive method of correspondence-oriented thinking by researchers of the so-called exact sciences (see Kokowski 1996; 2001; 2004; 2006; 2015c). Therefore: a) I do not agree with Jean Bricmon (2015), who – going on footnotes of Thomas S. Kuhn, Paul Feyerabend, and the sociology of scientific knowledge – declares that there is no scientific method and it is not a problem, and b) I do agree with Elliott Sober (2015), who is sure that the scientific method is not a myth and there are general normative principles that govern every science.

[8] Of course, this three-part division is only a conventional division.

$$\varrho_{\nu}(\nu,T) = 8\pi\nu^{2}c^{-3}U(\nu,T),$$

$$U(\nu,T) = \frac{h\nu}{\exp\left(\frac{h\nu}{kT}\right) - 1}$$

where:

$\varrho_{\nu}(\nu,T)$	–	the spectral energy density of radiation in cavity in thermal equilibrium at absolute temperature T per unit volume and per frequency unit;
$8\pi\nu^{2}c^{-3}d\nu$	–	the number of modes of oscillation (states) in the frequency interval $[\nu, \nu + d\nu]$ per unit volume;
$U(\nu,T)$	–	the mean energy of oscillator of frequency ν and absolute temperature T.

In the third stage, the subsequent theoretical derivations and justifications of the black-body radiation law by Max Planck (1900b; …) were criticized by other scientists: Joseph Larmor, Charles Thomson Rees Wilson, Peter Debye, Hendrik Lorentz, Władysław Natanson, Abram Fyodorovich Joffé, Paul Ehrenfest, Jun Ishiwara, Iurii Aleksandrovich Krutkov, Mieczysław Wolfke, Heike Kamerligh Onnes, Maurice de Broglie, Arthur H. Compton, Wolfgang Pauli, Viktor R. Bursian, Otto Halpern, and finally Satyendra Nath Bose in 1924, and Alfred Einstein in 1924–1925.[9] Regarding Bose's and Einstein's contributions: Bose in-

[9] This third stage was analysed by dozens of scholars, mainly physicists and historians of physics. There are two groups of such scholars. The first group, which overlooked Natanson's achievements (a majority of physicists and historians of physics); and the second group, which noticed his achievements.

To the first group belong, among others, B.I. Spasskiĭ (1964, chap. 19, §73); L.D. Landau, E.M. Lifshitz (3rd ed. 1976; Engl. transl. 1986, §37, 54, 55); F. Hund (1956, § 92); K. Huang (1963, chapter 12); H. Kangro (1970/1976); R. Feymann (1972, chapter 1.9); T.S. Kuhn (1978); M. Toda, R. Kubo, N. Saitô (1978; Engl. transl. 1983, chapter 3.1.3); A. Pais (1979, paragraph VI; 1982, repr. 2005, chapter 23); C. Domb (1995); C.A. Gearhart (2002); H. Kragh (2002); R. Fitzpatrick (2006); A. Michelangeli (2007); D. Monaldi (2009); E.P. Canals, T. Sauer (2010a); *Wikipedia* (2019b; 2019d).

To the second group belong, among others, E.T. Whittaker (1953); F. Hund (1967); A. Hermann (1969; Engl. transl. 1971); A. Kastler (1981); B. Średniawa (1985;

troduced statistics for radiation, called now Bose statistics, and Einstein, generalizing Bose's approach, introduced statistics both for imponderable matter (radiation) and of ponderable matter (material vibrators / atoms), called now Bose-Einstein statistics.[10]

However, the understanding of this entire three-part story, including the reasons why all of the authors criticized Planck's approaches, is not the aim of this article.[11] I restrict below only to illuminate the issue of the reception of Natanson's views in his times and later.

3. Natanson's achievements in focus

In this section, in order to systematize the knowledge on Natanson's achievements dispersed among different kinds of specialists, I will try to summarise the discussions about the issue held among specialists and add my own comments. To achieve this aim I will try to answer key questions regarding this issue.

3.1. Elementary issue: How many works did he write on the subject?

It is an elementary issue for a positivistic methodology of the history of science to establish the number of works that Natanson wrote about the statistics of imponderable matter (black-body radiation) and of ponderable matter (material vibrators).

As far as I know, he wrote the following list of works on the subject mentioned.

1997; 2000; 2001; 2007); A. Pais (1986); S. Bergia (1987); A. Bach (1988; 1990); O. Darrigol (1988; 1993); B. Lange (1992a; 1992b; 1997a; 1997b); L.J. Boya (2003); J. Spałek (2005; 2006; 2009); S. Varró (2006a; 2006b); A. Borrelli (2009), "R. Minamida" (N. Nagasawa) (2009a); M. Kokowski (2009; 2011a; 2011b); M. Waniek, K. Hentschel (2011); B.R. Masters (2013); N. Nagasawa (2018); K. Hentschel (2018) – this group is not homogeneous: its representatives declare a whole spectrum of views (I will explain it later).

[10] About these terms see Appendix 2.

[11] This matter is complicated and worthy a separate detailed book. For a general introduction to the history of physical ideas and the problem-situation see Mehra, Rechenberg 2001, pp. 557–578. One of the important threads of this history is to explain *a combinatorial and physical problem*: the distribution of indistinguishable particles over energy states.

 DOI: 10.4467/2543702XSHS.19.012.11018

1) O teoryi statystycznej promieniowania. On the Statistical Theory of Radiation (presented: 6 March 1911; published: circa 10 April 1911). *Bulletin International de l'Académie des Sciences de Cracovie, Classe des Sciences mathématiques et naturelles. Série A: Sciences mathématiques. Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch--Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften,* pp. 134–148. (in English) & offprint. Hereafter: Natanson 1911a.

O teoryi statystycznej promieniowania. — On the Statistical Theory of Radiation.

Mémoire

de M. ***LADISLAS NATANSON*** m. t.,

présenté dans la séance du 6 Mars 1911.

The Theory of Natural Radiation has been cultivated with great ingenuity and the results which Planck, Einstein, Jeans, Larmor, Lorentz and other physicists have obtained must be reckoned amongst the profoundest discoveries in Molecular Science.

It is to Planck that we owe the fundamental conception on which the theory in its present state essentially depends. As is well known, Planck supposes that energy is not divisible without limit, but is constituted of an aggregate of discrete elements or *units*; and that the ultimate particles of which matter consists are capable only of absorbing, containing and emitting amounts of energy which are multiples of these finite and determinate units.

§ 1. In the usual manner, we imagine a system of N independent material entities, capable only of having amounts

(1) . . . $$0e,\ 1e,\ 2e,\ \ldots\ldots\ pe$$

of energy; in general ie, where $i = 0, 1, 2, \ldots p$. For brevity we will call these entities the *receptacles* of energy in the system. If out of N receptacles under consideration, N_0 have zero energy, N_1 have energy e etc., then

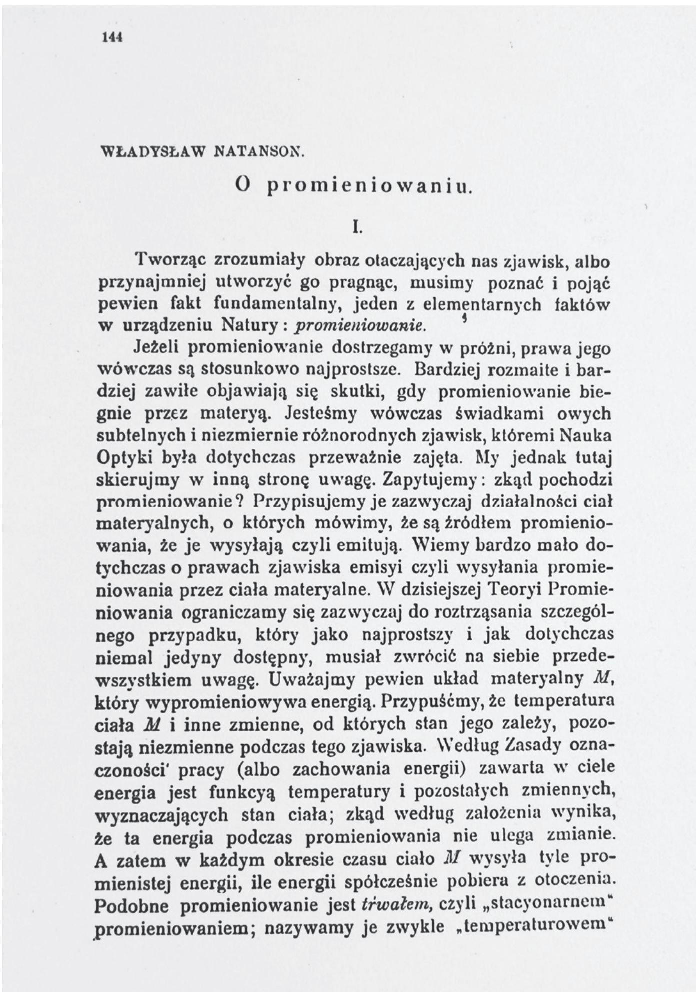
(I) . . . $$\sum_{i=0}^{i=p} N_i = N.$$

If E is the total amount of energy contained in the system, the number of available units of energy is $n = E/e$ and we obviously have

(II) . . . $$\sum_{i=0}^{i=p} iN_i = n.$$

Fig. 1. The front page of "On the Statistical Theory of Radiation" (presented: 6 March 1911; published: circa 10 April 1911). Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

2) O promieniowaniu (On Radiation) (1st vers.; 19 July 1911). [In:] *Księga pamiątkowa XI Zjazdu Lekarzy i Przyrodników Polskich w Krakowie, 18–22 lipca 1911* {*Proceedings of the 11th Congress of Polish physicians and natural scientists in Krakow, 18–22 July 1911*} (Kraków: Komitet Gospodarczy, 1911), pp. 144–160. Available online: https://jbc.bj.uj.edu.pl/dlibra/doccontent?id=278801. Hereafter: Natanson 1911b).


144

WŁADYSŁAW NATANSON.

O promieniowaniu.

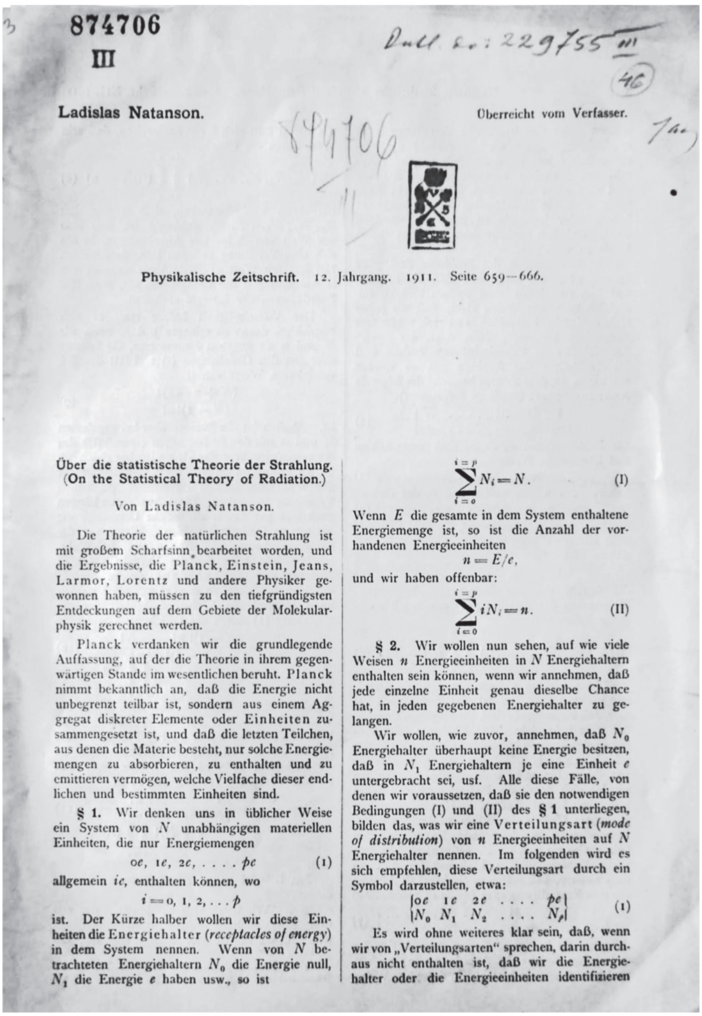
I.

Tworząc zrozumiały obraz otaczających nas zjawisk, albo przynajmniej utworzyć go pragnąc, musimy poznać i pojąć pewien fakt fundamentalny, jeden z elementarnych faktów w urządzeniu Natury: *promieniowanie.*

Jeżeli promieniowanie dostrzegamy w próżni, prawa jego wówczas są stosunkowo najprostsze. Bardziej rozmaite i bardziej zawiłe objawiają się skutki, gdy promieniowanie biegnie przez materyą. Jesteśmy wówczas świadkami owych subtelnych i niezmiernie różnorodnych zjawisk, któremi Nauka Optyki była dotychczas przeważnie zajęta. My jednak tutaj skierujmy w inną stronę uwagę. Zapytujemy: zkąd pochodzi promieniowanie? Przypisujemy je zazwyczaj działalności ciał materyalnych, o których mówimy, że są źródłem promieniowania, że je wysyłają czyli emitują. Wiemy bardzo mało dotychczas o prawach zjawiska emisyi czyli wysyłania promieniowania przez ciała materyalne. W dzisiejszej Teoryi Promieniowania ograniczamy się zazwyczaj do roztrząsania szczególnego przypadku, który jako najprostszy i jak dotychczas niemal jedyny dostępny, musiał zwrócić na siebie przedewszystkiem uwagę. Uważajmy pewien układ materyalny *M*, który wypromieniowywa energią. Przypuśćmy, że temperatura ciała *M* i inne zmienne, od których stan jego zależy, pozostają niezmienne podczas tego zjawiska. Według Zasady oznaczoności pracy (albo zachowania energii) zawarta w ciele energia jest funkcyą temperatury i pozostałych zmiennych, wyznaczających stan ciała; zkąd według założenia wynika, że ta energia podczas promieniowania nie ulega zmianie. A zatem w każdym okresie czasu ciało *M* wysyła tyle promienistej energii, ile energii spółcześnie pobiera z otoczenia. Podobne promieniowanie jest *trwałem,* czyli „stacyonarnem" promieniowaniem; nazywamy je zwykle „temperaturowem"

Fig. 2. The front page of "O promieniowaniu" (1st version, 19 July 1911).
Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

3) Über die statistische Theorie der Strahlung (received: 29 Aprill 1911; published: 15 August 1911). *Physikalische Zeitschrift* 12, pp. 659–666 {it is a translation of Natanson's first paper (1911a)}; & offprint (in German). The translation was made by Max Iklé, when the chief editor of the journal was Friedrich Krüger. Hereafter: Natanson 1911c.



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III

Ladislas Natanson.

Überreicht vom Verfasser.

Physikalische Zeitschrift. 12. Jahrgang. 1911. Seite 659—666.

Über die statistische Theorie der Strahlung.
(On the Statistical Theory of Radiation.)

Von Ladislas Natanson.

Die Theorie der natürlichen Strahlung ist mit großem Scharfsinn bearbeitet worden, und die Ergebnisse, die Planck, Einstein, Jeans, Larmor, Lorentz und andere Physiker gewonnen haben, müssen zu den tiefgründigsten Entdeckungen auf dem Gebiete der Molekularphysik gerechnet werden.

Planck verdanken wir die grundlegende Auffassung, auf der die Theorie in ihrem gegenwärtigen Stande im wesentlichen beruht. Planck nimmt bekanntlich an, daß die Energie nicht unbegrenzt teilbar ist, sondern aus einem Aggregat diskreter Elemente oder Einheiten zusammengesetzt ist, und daß die letzten Teilchen, aus denen die Materie besteht, nur solche Energiemengen zu absorbieren, zu enthalten und zu emittieren vermögen, welche Vielfache dieser endlichen und bestimmten Einheiten sind.

§ 1. Wir denken uns in üblicher Weise ein System von N unabhängigen materiellen Einheiten, die nur Energiemengen

$$0e,\ 1e,\ 2e,\ \ldots\ pe \qquad (1)$$

allgemein ie, enthalten können, wo

$$i = 0, 1, 2, \ldots p$$

ist. Der Kürze halber wollen wir diese Einheiten die Energiehalter (*receptacles of energy*) in dem System nennen. Wenn von N betrachteten Energiehaltern N_0 die Energie null, N_1 die Energie e haben usw., so ist

$$\sum_{i=0}^{i=p} N_i = N. \qquad (I)$$

Wenn E die gesamte in dem System enthaltene Energiemenge ist, so ist die Anzahl der vorhandenen Energieeinheiten

$$n = E/e,$$

und wir haben offenbar:

$$\sum_{i=0}^{i=p} iN_i = n. \qquad (II)$$

§ 2. Wir wollen nun sehen, auf wie viele Weisen n Energieeinheiten in N Energiehaltern enthalten sein können, wenn wir annehmen, daß jede einzelne Einheit genau dieselbe Chance hat, in jeden gegebenen Energiehalter zu gelangen.

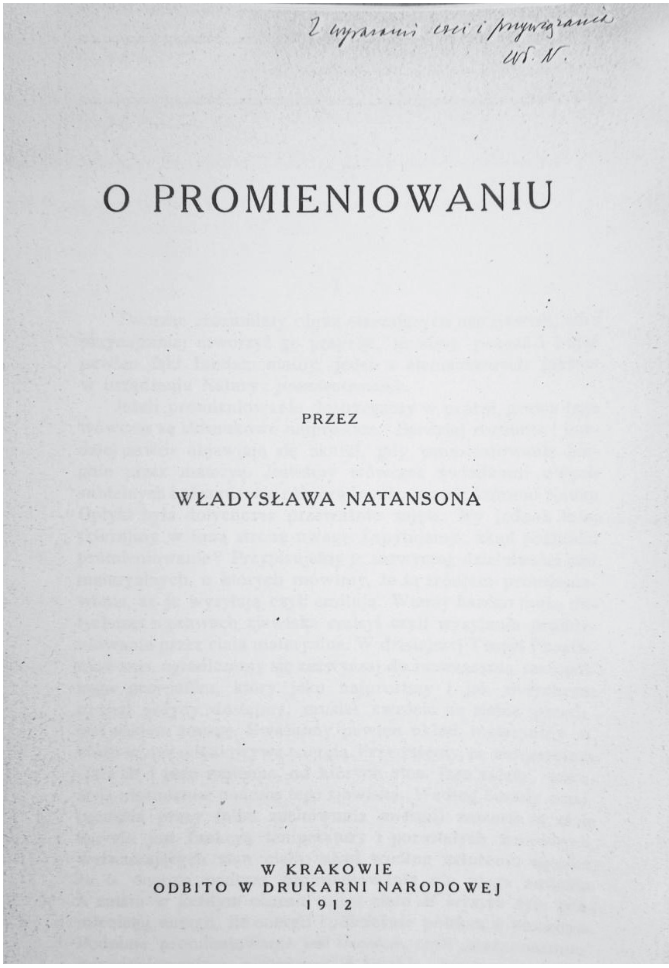
Wir wollen, wie zuvor, annehmen, daß N_0 Energiehalter überhaupt keine Energie besitzen, daß in N_1 Energiehaltern je eine Einheit e untergebracht sei, usf. Alle diese Fälle, von denen wir voraussetzen, daß sie den notwendigen Bedingungen (I) und (II) des **§ 1** unterliegen, bilden das, was wir eine Verteilungsart (*mode of distribution*) von n Energieeinheiten auf N Energiehalter nennen. Im folgenden wird es sich empfehlen, diese Verteilungsart durch ein Symbol darzustellen, etwa:

$$\begin{Bmatrix} 0e & 1e & 2e & \ldots & pe \\ N_0 & N_1 & N_2 & \ldots & N_p \end{Bmatrix} \qquad (1)$$

Es wird ohne weiteres klar sein, daß, wenn wir von „Verteilungsarten" sprechen, darin durchaus nicht enthalten ist, daß wir die Energiehalter oder die Energieeinheiten identifizieren

Fig. 3. The front page of "Über die statistische Theorie der Strahlung" (received: 29 Aprill 1911; published: 15 August 1911). Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

4) On Radiation (1st vers.) – Offprint 1912 of Natanson 1911b (in Polish). Hereafter: Natanson 1912a.



Z wyrazami czci i przywiązania
W. N.

O PROMIENIOWANIU

PRZEZ

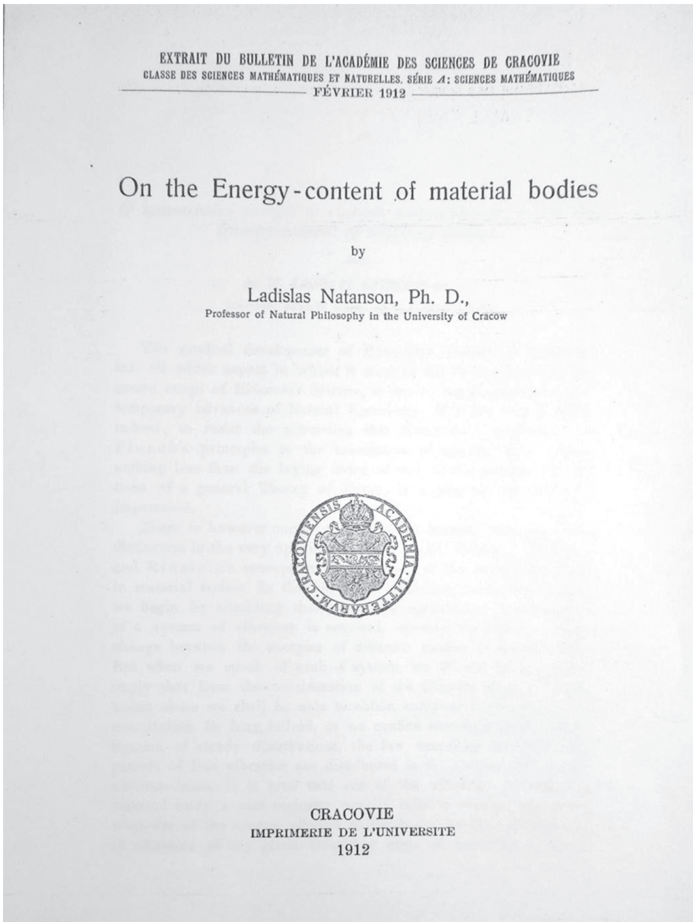
WŁADYSŁAWA NATANSONA

W KRAKOWIE
ODBITO W DRUKARNI NARODOWEJ
1912

Fig. 4. The front page of "On Radiation" (1st version) – offprint 1912 of Natanson 1911b. Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

5) O zawartości energii w ciałach materyalnych – On the Energy--content of material bodies (presented on 8 January 1912; published: April 1912). *Bulletin International de l'Académie des Sciences de Cracovie, Classe des Sciences mathématiques et naturelles. Série A: Sciences mathématiques. Anzeiger der Akademie der Wissenschaften*

in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften, pp. 95–102 & offprint (in English). Hereafter: Natanson 1912b.



EXTRAIT DU BULLETIN DE L'ACADÉMIE DES SCIENCES DE CRACOVIE
CLASSE DES SCIENCES MATHÉMATIQUES ET NATURELLES. SÉRIE A: SCIENCES MATHÉMATIQUES
FÉVRIER 1912

On the Energy-content of material bodies

by

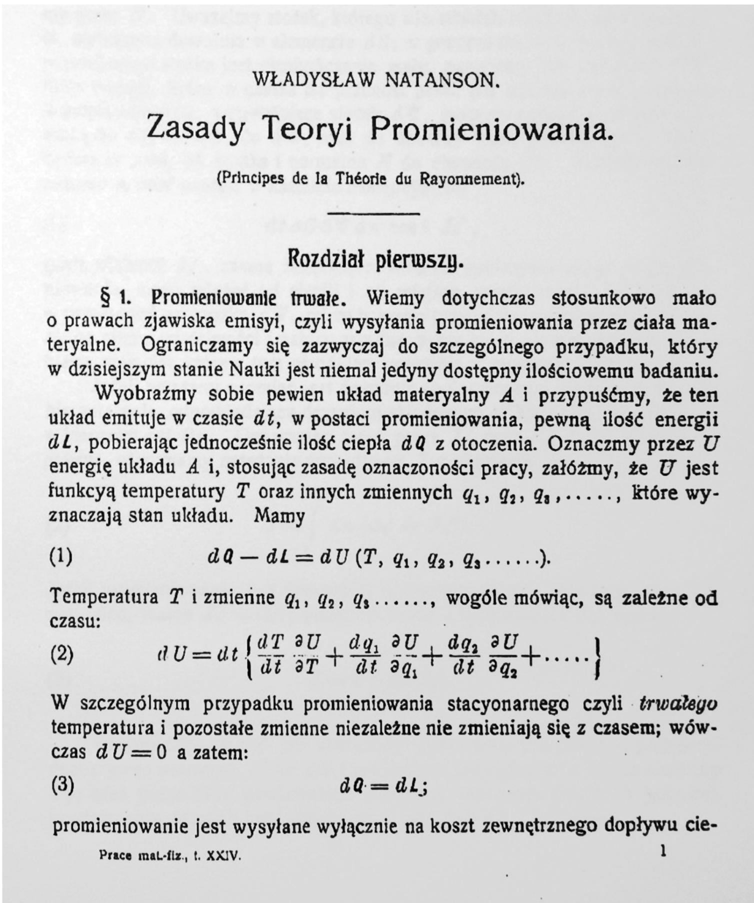
Ladislas Natanson, Ph. D.,
Professor of Natural Philosophy in the University of Cracow

CRACOVIE
IMPRIMERIE DE L'UNIVERSITE
1912

Fig. 5. The front page of "On the Energy-content of material bodies" (presented on 8 January 1912; published: April 1912). Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

6) Zasady Teoryi Promieniowania (Principes de la Théorie du Rayonnement) (in Polish). *Prace Matematyczno-Fizyczne* 24, pp. 1–88. Warszawa: Wydawnictwo Redakcji Prac Matematyczno-Fizycznych. Available online (at "Polska Biblioteka Wirtualna Nauki",

„Kolekcja Matematyczna”): http://matwbn.icm.edu.pl/ksiazki/pmf/pmf24/pmf2411.pdf. Hereafter: Natanson 1913. (*The article, though written only in Polish, is the most important Natanson’s work on the theory of radiation and related matters.*)



WŁADYSŁAW NATANSON.

Zasady Teoryi Promieniowania.

(Principes de la Théorie du Rayonnement).

Rozdział pierwszy.

§ 1. Promieniowanie trwałe. Wiemy dotychczas stosunkowo mało o prawach zjawiska emisyi, czyli wysyłania promieniowania przez ciała materyalne. Ograniczamy się zazwyczaj do szczególnego przypadku, który w dzisiejszym stanie Nauki jest niemal jedyny dostępny ilościowemu badaniu.

Wyobraźmy sobie pewien układ materyalny A i przypuśćmy, że ten układ emituje w czasie dt, w postaci promieniowania, pewną ilość energii dL, pobierając jednocześnie ilość ciepła dQ z otoczenia. Oznaczmy przez U energię układu A i, stosując zasadę oznaczoności pracy, załóżmy, że U jest funkcyą temperatury T oraz innych zmiennych $q_1, q_2, q_3, \ldots\ldots$, które wyznaczają stan układu. Mamy

(1) $$dQ - dL = dU(T, q_1, q_2, q_3 \ldots\ldots).$$

Temperatura T i zmienne $q_1, q_2, q_3 \ldots\ldots$, wogóle mówiąc, są zależne od czasu:

(2) $$dU = dt\left\{\frac{dT}{dt}\frac{\partial U}{\partial T} + \frac{dq_1}{dt}\frac{\partial U}{\partial q_1} + \frac{dq_2}{dt}\frac{\partial U}{\partial q_2} + \ldots\ldots\right\}$$

W szczególnym przypadku promieniowania stacyonarnego czyli *trwałego* temperatura i pozostałe zmienne niezależne nie zmieniają się z czasem; wówczas $dU = 0$ a zatem:

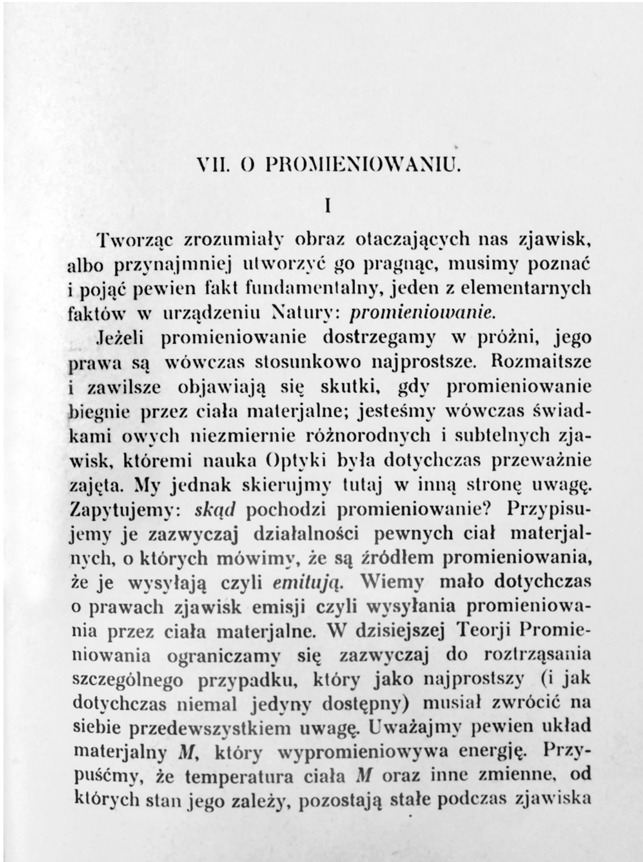
(3) $$dQ = dL;$$

promieniowanie jest wysyłane wyłącznie na koszt zewnętrznego dopływu cie-

Prace mat.-fiz., t. XXIV. 1

Fig. 6. The front page of “Zasady Teoryi Promieniowania” (“Principes de la Théorie du Rayonnement”) (in Polish). Source: © Polska Biblioteka Wirtualna Nauki, Kolekcja Matematyczna; photo: © Michał Kokowski.

7) On Radiation (*2nd vers. with changes*) (1924). [In:] Natanson 1924a, pp. 125–151 (in Polish). Hereafter: Natanson 1924b; before the publication of Bose’s first paper.



VII. O PROMIENIOWANIU.

I

Tworząc zrozumiały obraz otaczających nas zjawisk, albo przynajmniej utworzyć go pragnąc, musimy poznać i pojąć pewien fakt fundamentalny, jeden z elementarnych faktów w urządzeniu Natury: *promieniowanie*.

Jeżeli promieniowanie dostrzegamy w próżni, jego prawa są wówczas stosunkowo najprostsze. Rozmaitsze i zawilsze objawiają się skutki, gdy promieniowanie biegnie przez ciała materjalne; jesteśmy wówczas świadkami owych niezmiernie różnorodnych i subtelnych zjawisk, któremi nauka Optyki była dotychczas przeważnie zajęta. My jednak skierujmy tutaj w inną stronę uwagę. Zapytujemy: *skąd* pochodzi promieniowanie? Przypisujemy je zazwyczaj działalności pewnych ciał materjalnych, o których mówimy, że są źródłem promieniowania, że je wysyłają czyli *emitują*. Wiemy mało dotychczas o prawach zjawisk emisji czyli wysyłania promieniowania przez ciała materjalne. W dzisiejszej Teorji Promieniowania ograniczamy się zazwyczaj do roztrząsania szczególnego przypadku, który jako najprostszy (i jak dotychczas niemal jedyny dostępny) musiał zwrócić na siebie przedewszystkiem uwagę. Uważajmy pewien układ materjalny *M*, który wypromieniowywa energję. Przypuśćmy, że temperatura ciała *M* oraz inne zmienne, od których stan jego zależy, pozostają stałe podczas zjawiska

Fig. 7. The front page of "On Radiation" (2nd vers. with changes, 1924).
Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

3.2. What works does Natanson mention in the bibliography of his works?

From the current research in the science of science, which includes bibliometrics, it is known that the authors of scientific papers have often a very serious problem with the reliability of quoting the publications they use during the preparation of their own publications.[12] Therefore, it is worth examining how Natanson dealt with this problem in his times.

[12] Kokowski 2015b.

In the article L. Natanson 1911a: On statistical theory of radiation. *Bulletin International de l'Académie des Sciences de Cracovie A. Sciences mathématiques. Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften* (presented: 6 March 1911; published circa 10 April 1911), and its translation into German: Natanson, Ladislas (Władysław) 1911c: Über die statistische Theorie de Strahlung. *Physikalische Zeitschrift* (received: 29 April 1911; published: 15 August 1911). "Nach *Bulletin de l'Académie des Sciences de Cracovie* (A), pp. 134–148, 1911 (aus dem Englischen übersetzt von Max Iklé; eingegangen 29. April 1911)" Natanson mentions the following nine works:[13]

- Boltzmann, Ludwig 1872: Weitere Studien über das Wärmegleichgewicht unter Gasmolecülen. *Sitzungsberichte d. K. Akad. d. Wiss. zu Wien* II Abt, 66, pp. 275–370 (October 1872).
- Boltzmann, Ludwig 1877: Über die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung resp. den Sätzen über das Wärmegleichgewicht. *Sitzungsberrichte d. K. Akad. d. Wiss. zu Wien* II Abt, 76, pp. 373–435 (October 1877).
- Debye, Peter 1910: Der Wahrscheinlichkeitsbegriff in der Theorie der Strahlung. *Annalen der Physik* 33, pp. 1427–1434.
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[13] He used a short version of the citation, accepted by physicists (i.e. without giving the publication title, without the scope of pages of the publication).

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- Planck, Max 1906: *Vorlesungen über die Theorie der Wärmestrahlung* (1st ed.).
- Wilson, Harold A. 1910: On the Statistical Theory of Heat Radiation. *Philosophical Magazine* 20, pp. 121–125. (Available online: https://archive.org/details/londonedinburg6201910lond/page/n4).

In Natanson, Ladislas (Władysław) 1912b (presented on 8 January 1912; published: April 1912): O zawartości energii w ciałach materyalnych – On the Energy-content of material bodies. *Bulletin International de l'Académie des Sciences de Cracovie A. Sciences mathématiques. Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften*, pp. 95–102, the author mentions two theories:

- Planck's theory of radiation;
- Einstein's theory of specific heat;

and two works:

- Natanson, Ladislas (Władysław) 1911a: On statistical theory of radiation. *Bulletin International de l'Académie des Sciences de Cracovie A,* pp. 134–148;
- Duhem, Pierre 1911: *Traité d'Energétique ou de Thermodynamique Générale.*

In the work: Natanson, Ladislas (Władysław) 1913: Zasady teoryi promieniowania (Principes de la Théorie du Rayonnement). *Prace Matematyczno-Fizyczne* 24, pp. 1–88. Warszawa: Wydawnictwo Redakcji Prac Matematyczno-Fizycznych, the author mentions all possible works on the subject written by authors in German, English, French, Italian and Polish – nearly 200 references, including for example P. Ehrenfest's papers of 1906; 1911a, and T&P. Ehrenfest of 1911.[14]

The article "O promieniowaniu" (On radiation), marked as Natanson 1911b, 1912a (i.e. offprint of 1911b) and 1924b (i.e. 1924a, pp. 125–151; it is a revised version of 1911b), is a review essay. It does not only regard radiation but also theories of gases, of liquids and solid states,

[14] I will continue these considerations later in the article.

with quanta of energy as a key joining all these issues. The article was published without a bibliography, but the dates and the names of authors of main results are mentioned. In the case of the version of 1924, the name of Bose is still omitted, since Natanson wrote his article before Bose's articles (1924a;[15] 1924b[16]).

3.3. Who was the first scientist to appreciate very highly the achievements of Natanson in quantum statistics?

The received answer for this question is clear. It was Friedrich Hund (1896–1997), a German physicist and historian of physics.[17]

Fig. 8. Friedrich Hund, Werner Heisenberg and Max Born (Hund's seventieth birthday, Göttingen, 4 February 1966). Source: http://upload.wikimedia.org/wikipedia/commons/thumb/6/60/Hund_Heisenberg_Born_1966_Göttingen.jpg/800px-Hund_Heisenberg_Born_1966_Göttingen.jpg.

He stated so in his monograph of 1967: *Geschichte der Quantentheorie* (Mannheim: Bibliographisches Institut, 1967); English translation: *The history of quantum theory*. Transl. by Gordon Reece (London: Harrap, 1974); Italian translation by G. Longo: *Storia della teoria dei quanta* (Bollona: Bollati Boringhieri, 1975); Japan translation: by K. Yamazaki 1978 and Russian translation: *Istorija kvantovoj teorii* (Kiev: Naukova Dumka,

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[15] Reprinted 2009a, English transl. 2009b / English transl. 1976.

[16] Reprinted 2009c, English transl. 2009d.

[17] Cf. Hund, Hentschel, Tobies 1996; *Wikipedia* 2019c.

1980). It happened only after 56 years from the appearance of Natanson's first work (Natanson 1911a).

But why did it happen so late? And is it true that only in 1967 did Hund – as a historian of physics – first appreciate Natanson's achievements in quantum statistics? Moreover, did Natanson's contemporary scientists value his work in the 1910s and 1920s? I will try to shed some light on these problems later in this article.

3.4. What is the essence and rank of Natanson's achievements?

Researchers commenting Natanson's achievements answered for this question in three complementary ways.

Firstly, Natanson's name was linked with the name of Satyendra Nath Bose and his statistics *of "light quanta"*. This strategy was applied by such scientists as L. Infeld (1958); F. Hund (1967/1974); A. Herman (1969, 1971); A. Kastler (1981); B. Średniawa (1985; 1997; 2000, pp. 454–455; 2001, pp. 105–107; 2007); B. Lange (1992a; 1992b; 1997a; 1997b), and J. Spałek (2005; 2006; 2009).[18]

According to Leopold Infeld (1958, p. 136; 1964b, pp. 35–36):

> [Natanson] was close, remarkably close to the great scientific discoveries, such as the formulation of Bose statistics [translation — M.K.].[19]

[18] S. Bergia (1987), A. Bach (1988; 1990), J.J. Stachel (2000), and M. Kokowski (2009; 2011a; 2011b) do not belong to this list. I will explain their stances below.

[19] Of course, this side-note by Infeld, spoken within his reminiscences on his teacher, does not diminish the importance of the role of Friedrich Hund's priority (1967) in appreciating the meaning of Natanson's thought in the history of quantum physics (see above section 3.3).

Incidentally, in order to understand this Infeld's view better, it is worth sketching his attitude to his teacher Natanson. They were both Polish Jews, physicists and talented writers of popular books, however they differed considerably in social, philosophical and political terms. Infeld came from a poor and uneducated family, and Natanson had a rich and educated burgeois backgroud. The former was an ateist and socialist, and the latter (Władysław *Szeliga* Natanson) in 1900 converted from Judaism to Catholicism (cf. Mieses 1938a, pp. 144–150). In addition, Natanson did not help Infeld to get a job at university.

Unfortunately, Infeld – while writing his first famous autobiography (1941, reprint in 1980, 2006 & 2017) – repeatedly diverted from the truth. Among others, he

In contrast, according to Friedrich Hund (1967, pp. 25–26, 134, 153–154; (English transl.) 1974, pp. 30, 145; (Russian transl.) 1980, pp. 26, 123) *Natanson was the first who formulated Bose statistics of "light quanta"*:

> This method of counting events, that Natanson made, is exactly the one, which Bose later made for light quanta, and is now called Bose statistics (Hund 1974, p. 30).[20]

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created an untrue, much exaggerated, picture of Polish anti-Semitism, and an entirely misguided picture of his teacher Natanson: "The only lecturer in mathematical physics [in Kraków] was an old, completely detached professor, delighted with the smoothness and external beauty of his lectures and not really giving a damn whether he inspired anyone or not. For thirty years he had lectured in Cracow and had never had a Ph.D. student" (Infeld 1941, repr. 2006, p. 88).

In fact, Natanson had five Ph.D. students: Stanisław Loria (1907), Wacław Staszewski (1917), Lepold Infeld (1921), Józef Miczyński (1922), Stefan Szymon Rozental (1928), and eleven others students, who received a Ph.D., had been earlier peer-reviewed by him (cf. Dybiec 2009, pp. 30–38).

Natanson was also a very good teacher, which Infeld explained himself clearly in his later essay (1958, pp. 130–136), after his comeback to Poland. It appears that Infeld admired Natanson for his great intellectual culture, but he had held a grudge against him, because although he was – according to Infeld – his only scientific pupil (which was not true – see above): "he did not teach me the technique of scientific work and did not provide me with the right conditions to conduct such work" ("nie nauczył mnie techniki pracy naukowej i nie dał mi warunków do tej pracy" (Infeld 1958, p. 134). Therefore, it is not psychologically surprising that the pupil has not found the time to comment on the teacher's achievements in more detail (cf. Natanson 1933/1958, pp. 115–119; Infeld 1958, pp. 130–136).

In his new autobiographical essays written in Polish (1954; 1964; 1967), Infeld tempered his views presented in his first autobiography (1941). Nevertheless, he repeated in 1967 his sentiments to his teacher: "So far, I do not understand why Professor Natanson did not offer me a job as his assistant. Certainly no one at the university would oppose the will of one of the most important professors. Of course I was a Jew. But was that enough of a reason? Perhaps, but today I think it's not the only one. Professor Natanson must have been disgusted with the idea of having an assistant". ("Dotychczas nie rozumiem, dlaczego profesor Natanson nie zaproponował mi asystentury. Na pewno nikt na uniwersytecie nie sprzeciwiłby się woli jednego z najważniejszych profesorów. Oczywiście byłem Żydem. Ale czy to powód wystarczający? Może tak, ale dzisiaj sądzę, że nie jedyny. Profesor Natanson musiał mieć wstręt do idei posiadania asystenta" (Infeld, 1967, p. 187)).

For explaining Infeld's wrong opinions, especially the allegedly Polish radical anti-Semitism among the Polish prewar academia, cf. Wróblewski 2017, pp. 71–82. See also Hurwic 1968, reprinted 2016, pp. 405–417.

[20] "Die Abzählung, die Natanson verdeutlicht hat, ist genau die, die Bose später auf Lichtquanten anwandte und die man jetzt Bose-Statistik nennt" (Hund 1967, p. 26).

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> Bose statistics of light quanta was thus led to Planck radiation formula. This method of counting events for indistinguishable particles, which had already been perfectly recognized by Natanson in 1911, was subsequently to be called Bose statistics (Natanson's work of 1911, had of course been forgotten by 1924). It was not until some years later that the alternative possibility of the quantum statistics of the indistinguishable particle, that of Fermi statistics, was considered (Hund 1974, p. 145).

This thesis of F. Hund was accepted later by some researchers, such as A. Hermann (1971, p. 141), A. Kastler (1981), B. Średniawa (1985, pp. 89–90; 1997, pp. 14–16 (and repeated by K. Czapla 2005, p. 55); 2000, pp. 454–455; 2001, pp. 105–106; 2007, pp. 713–714), A. Bach (1990, pp. 1–2), L.J. Boya (2003, p. 110), K. Hentschel (2006, p. 15; 2018, pp. 81–86), S. Varró (2006a, pdf version, pp. 14–16; 2006b, pdf version, p. 4; 2007, pp. 161–162), B. Lange (1992a; 1992b; 1997a; 1997b), and "Roh Minamida" (2009)/N. Nagasawa (2018).

For example, according to Armin Hermann (1971, p. 141):

> Natanson (besides Max Planck, Albert Einstein and Paul Ehrenfest) was one of the first to establish a background for deeper understanding of the nature of quantum physics.[21]

According to Alfred Kastler (1981):

> Natanson discovered Bose-Einstein statistics 13 years before Bose and Einstein.

According to Bronisław Średniawa (1985, p. 89):

> In 1911 Natanson turned his scientific interest towards quantum theory. The papers *On the Statistical Theory of Radiation* [...], and its German version published in that year belong to the most important Natanson's publications. They contain the first formulation of quantum statistics, which was 13 years later rediscovered independently by Indian

[21] The statement was later cited by, among others, Średniawa 1997, p. 16, and repeated by Czapla 2005, p. 56.

> physicist Bose [...] and developed by Einstein and is today called "Bose-Einstein statistics" [my emphasis – M.K.].[22]

According to Alexander Bach:

> Natanson was the first who formulated Bose statistics of "light quanta"; the statistical assumptions of Bose are contained in a work of Natanson (1911c) (Bach 1990, pp. 1–2).

According to Bogdan Lange (1997a, p. 526):

> The analysis and comparison I conducted show that procedures employed by Natanson and Bose are identical. Therefore, Hund (1974, p. 145) was right when he said, "The Bose statistics of light quanta was thus led to Planck radiation formula. This method of counting events for indistinguishable particles, which had already been perfectly recognized by Natanson in 1911, was subsequently to be called Bose statistics (Natanson's work of 1911, had of course been forgotten by 1924) [...]".

For this reason Bogdan Lange (1997a) uses the phrase "Natanson's statistics", and Peter Mittelstaedt (2013, pp. 83–85) "Natanson statistics" (however, he does not quote Lange's article).

A separate opinion was expressed by a physicist Józef Spałek (2005; 2006; 2009)[23]:

> We should talk about Natanson-Bose-Einstein statistics, with the reservation that Natanson assigned indistinguishability to photons absorbed in "atoms of energy", but Bose assigned statistical properties to the radiation itself in this sense that he considered the number of photon states as restricted only by geometry of container (it is the accepted view today) [summarised by M.K.].

A similar thesis was formulated by two historians of science: Magdalena Waniek and Klaus Hentschel (2011, p. 42):

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[22] Those statements were later repeated by Czapla 2005, pp. 55–56.

[23] Knowing B. Średniawa's works, J. Spałek inherites some views of earlier scholars of Natanson's achievements (particularly of F. Hund's).

> Actually, the quantum statistics named nowadays after Bose and Einstein would have to be called Planck--Natanson-Bose-Einstein statistics. This confirms the first law of the history of science – namely that (almost) no scientific result is named after the people who actually discovered it first [translated by M.K.].[24]

Secondly, the researchers commenting Natanson's achievements thought that he was the first to understand the statistical foundations of Planck's law of black-body radiation. It was claimed by the following scholars: F. Hund (1967, pp. 26, 153–154), Kangro (1970/1976, p. 219, fn. 212), A. Kastler (1979; 1983), S. Bergia (1987, pp. 233–236; repr. 2009, pp. 343–346), T. You Wu (1986, p. 40), A. Bach (1990, p. 24), B. Średniawa (1985, pp. 89–90; 1997, pp. 14–16; 2000, pp. 454––455; 2001, pp. 105–106; 2007, pp. 713–714), A. Pais (1986, pp. 283, 294), A. Kojevnikov (2002, p. 198), J. Spałek (2005; 2006; 2009), S. Varró (2006a, pdf version, pp. 14–16), P. Enders (2007, p. 87), M. Waniek, K. Hentschel (2011, p. 42), B.R. Masters (2013, p. 43), O. Passon and J. Grebe-Ellis (2017, p. 7).

For example, according to Friedrich Hund (1967, pp. 153–154/1974, p. 167):

> In the course of deriving his radiation formula (1900), Planck had applied a noteworthy form of statistics for the distribution of energy quanta among the oscillators: equally probable events were the occupation numbers of the oscillators. In 1911 Natanson recognized this as containing a possible form of the statistics of indistinguishable particles. Bose applied the same form of statistics (1924) to light particles, and Einstein – to gas molecules: he showed that the fluctuations in such a gas behaved as if they were caused by both particles and waves (Hund 1974, p. 167).[25]

[24] "Eigentlich müsste die heute nach Bose und Einstein benannte Quantenstatistik somit Planck-Natanson-Bose-Einstein-Statistik heißen. Hier bestätigt sich auf's Neue der erste Hauptsatz der Wissenschaftsgeschichte – dass nämlich (fast) kein wissenschaftliches Resultat nach dem Namen seines tatsächlichen Erst-Entdeckers benannt ist" (Waniek, Hentschel 2011, p. 42).

[25] "Bei der Ableitung seiner Strahlungsformel (1900) verwandte Planck eine bemerkenswerte Statistik für die Verteilung von Energiequanten auf Oszillatoren:

According to Hans Kangro (1970/1976, p. 219, fn. 212):

> Ladislas Natanson was the first to recognize the reason why Planck's statistics must in contrast differ from "classical" statistics (Natanson 1911[c], [pp.] 663–[66]5).

According to Tau You Wu (1986, p. 40):

> Einstein's theory was criticized by P. Ehrenfest (1911––1914) [1911; 1914] and Natanson (1911) [but Natanson's article was not mentioned in bibliography], as not leading to Planck's law, but only to Wien's law. The criticism were based on the analysis of the distinction between the "indistinguishable and discrete photons" of Einstein and the "energy steps" in Planck's theory.

According to Alexander Bach (1990, pp. 24–25):

> The question concerning the statistical foundations of Planck's law left open by Lorentz was answered by Natanson [Natanson 1911c – in M.K.'s notation], who explicitly referred to the contributions of Boltzmann [...] and Lorentz [...]. Compared to the work of his predecessors (except Boltzmann) and followers, Natanson's work was distinguished by his unusual precision in terminology and by his explicit determinations of probability distributions. Because Natanson fixed, as Boltzmann did, the maximum energy of any molecule, and because he met the same difficulties as did Boltzmann in taking the limit n, d $\rightarrow \infty$, Natanson directly followed Boltzmann's method of 1877. [...] Finally, using and entropy expression which is equivalent to Boltzmann's (but inserting Planck's constant k) [...], Natanson [...] obtains, by following Planck's strategy, [...], Planck's radiation formula. [...] Natanson also provides

Gleichwahrscheinliche Fälle waren die Besetzugszahlen der Oszillatoren. Natanson sah darin (1911) eine mögliche Statistik nichtunterscheidbarer Teilchen. Bose wandte die gleiche Statistik (1924) auf Lichtteilchen an, Einstein auf Gasmolekeln: er zeigte, daß die Schwankungen in einem solchen Gase sich so verhielten, als kämen sie Teilchen und von Wellen her" (Hund 1967, pp. 153–154).

> a careful analysis of the difference between Bose-Einstein statistics and Maxwell-Boltzmann statistics [...][26].

According to Alexei Kojevnikov (2002, p. 198):

> If quantized waves can be regarded as a quantum mutation of classical waves, one could similarly try to modify

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[26] An anonymous reviewer of this article *rightly* noted that this quotation of A. Bach "says nothing about what Natanson did differently from Boltzmann and Planck. In particular, it is not explained, why Boltzmann did not obtain Planck's distribution before making the energy of the molecules to be continuous. (As a matter of fact, Planck's complexions are different from Boltzmann's complexions)".

Answering this remark, it is worth mentioning here that L. Boltzmann (1868; 1872; 1877) considers *a system of n fictious physical molecules, which is not realized in any mechanical problem*, since each of these molecules can take only a discrete velocity (0/q, 1/q , 2/q , 3/q , . . . , p/q) and a discrete alive force [i.e. twice of kinetic energy] (0ε, 1ε, 2ε, 3ε, ..., Pε). Moreover, w_0 molecules have a 0ε alive force, w_1 molecules have 2ε, …, w_P molecules have Pε, $w_0 + w_1 + w_2 + ... + w_p = n$, and the total alive force of this system is: $0w_0 + 1w_1 + 2w_2 + . . . + Pw_p = \lambda$. These assumptions are very useful to perform mathematical calculations to get the probability of state distributions of system of n such molecules. This probability is the number B of complexions in which w_0 molecules have a 0ε alive force, w_1 molecules have 2ε, etc, divided by the number of all complexions J: $B=n!/[(w_0)!(w_1)!...(w_P)!$ – it means that molecules are distinguishable], and $J = (\mu \lambda + n - 1, \lambda)$ [it is a binomial coefficient, and $\mu=\lambda\varepsilon/n$ (mean alive force of a molecule)]$= \lambda^{n-1} e^{n-1}/[(2\pi)^{1/2}(n-1)^{n-1/2}]$. Then, at a final step of calculation Boltzmann applies the limit of p, q and P to infinity, and ε to 0. Such a limit has a physical sense and leads to Maxwell-Boltzmann statistics. Cf. Boltzmann 1868 (reprinted 1909a, pp. 49–96; translated into English and commented 2014, pp. 139–142, 142–148); 1972 (reprinted 1909a, pp. 316–402); 1877 (reprinted 1909b, pp. 164–223; 2002 (English translation by Joël Le Roux)); Gallavotti 2014, pp. 178–181 (partial translation and comments by Giovanni Gallavotti); Bach (1988; 1990); Badino 2009; 2015; Nauenberg 2016, pp. 717–718.

Regarding Planck's approach, he accepted Boltzmann's combinatorical approach, but instead of fictious physical molecules, he considers material entities having discrete value of energy (0ε, 1ε, 2ε, 3ε, ..., Pε). More about the derivation of Planck's distributions, including his combinatorical considerations, see e.g. Rosenfeld 1936; 1958; Klein 1962; 1965; Kuhn 1978; Darrigol 2000, pp. 3–21/2001; 2003; Boya 2003; Badino 2009; 2012; 2015; Gearhart 2010, pp. 95–117; Nauenberg 2016; Passon, Grebe-Ellis 2017, pp. 5–6.

Regarding Natanson's approach, following the idea of Boltzmann's approach, he improved Planck's approach and explained the idea of distinguishability and indistinguishability of material entities. Cf. Natanson 1911a; 1911c; Bergia 1987, pp. 233––235 (reprinted: 2009, pp. 343–345); Lange (1992a; 1992b; 1997a; 1997b); Spałek (2005; 2006; 2009).

> somewhat the model of light quanta in order to bring it in correspondence with the Planck law. The existing contradiction, which amounted to differences in statistics, was clarified largely thanks to the efforts by Ehrenfest. He explained that statistically independent energy quanta led directly to the Wien law, while in order to obtain the Planck law, one had to assume that quanta were not independent (in the classical sense of the term, which was then the only available one) but indistinguishable objects (Ehrenfest 1911). This peculiarity of Planck's combinatorics was also understood around the same time by Ladislas Natanson (Natanson 1911 [1911c]) and a few years later explained with ultimate clarity by Ehrenfest and Kamerlingh Onnes (1915), who formulated the statistics of indistinguishable objects in comparison with the statistics of independent, or distinguishable objects in exactly the same way in which contemporary textbooks explain the difference between the Bose–Einstein and Boltzmann statistics. [...] Their understanding, however, did not immediately become part of the common knowledge in the field, which led, in particular, to further polemics in 1914, between Mieczyslaw Wolfke and Iurii Aleksandrovich Krutkov (Wolfke 1914a and 1914b, Krutkow 1914a and 1914b).

According to Peter Enders (2007, p. 87):

> Contrary to Einstein's results, Ehrenfest (1880–1933) (cf. Ehrenfest 1911) and Natanson (1864–1937) (cf. Natanson 1911a; 1911c) explained the difference between the classical and quantum radiation laws by means of different counting rules for distinguishable and indistinguishable particles (cf. Jammer 1965, §1.4; Mehra, Rechenberg 1982, vol. 1, pt. 2, sect. V.3).[27]

According to Magdalena Waniek and Klaus Hentchel (2011, p. 42):

> Natanson had consistently furthered the turnaround already indicated by Einstein in 1905 towards an investigation

[27] Cf. also Enders 2009, p. 13.

of the radiation field itself, and was the first to come across the core assumption of indistinguishability.[28]

According to Natanson's own words, this is the following idea:

> in the process of estimating probabilities, the receptacles of energy *can* be treated as distinguishable and that the energy-units, being in all respects precisely alike, *cannot* be so treated. Since it is upon this assumption that our procedure ultimately rests, it seems natural to appeal to it at once as the ground work of theory. Sufficient importance does not seem to be attached to the fact that we really have no other way of demonstrating the legitimacy of Planck's method of calculating probabilities except by appealing to the experimental evidence by which the final conclusions of the calculation are supported" (Natanson 1911a, p. 139).[29]

According to Barry R. Masters (2013, p. 43):

> From a historical perspective, the little-known work [sic! – M.K.] of Ladislas Natanson is significant. He shows that both Planck and Debye have made the tacit assumption of the indistinguishability of quanta in their derivations. Both Paul Ehrenfest and Kamerlingh Onnes reach the same conclusion.

According to Oliver Passon and Johanne Grebe-Ellis (2017, p. 7):

> The issue of indistinguishability in quantum theory has an exciting prehistory which is rarely mentioned in textbooks. Already in 1911 the Polish physicist Władysław (or latinized "Ladislas") Natanson scrutinized the statistical assumptions underlying Planck's law and anticipated this

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[28] "Natanson hatte die sich bereits bei Einstein 1905 andeutende Wende hin zu einer Untersuchung des Strahlungsfeldes selbst konsequent weitergedacht und stieß dabei als Erster auf die für die statistische Ableitung eigentlich zentrale Kernannahme der Ununterscheidbarkeit" (Waniek, Hentchel 2011, p. 42).

[29] Waniek, Hentchel (2011, p. 42) quoted the German version of Natanson's article (Natanson 1911c, p. 662).

> concept [33, i.e. Natanson 1911c]. Natanson discriminated between the situation where (i) both, the units of energy and the "receptacles of energy" can be distinguished, (ii) only the receptacles of energy can be identified (i.e., are distinguishable), or, (iii) only the units of energy are distinguishable. In either case a different combinatorics needs to be applied. Natanson claims that Planck's equation […] assumes the scenario (ii), i.e., treats the energy elements as indistinguishable. But he failed to draw a connection to light quanta. This connection was drawn by Paul Ehrenfest in 1911 but argued more convincingly in 1914 by Ehrenfest together with Heike Kamerlingh Onnes in the paper already quoted for the simple derivation of Planck's combinatorial formula [20, 34, i.e. Ehrenfest 1911; Ehrenfest, Kamerlingh Onnes 1914].

And according to Klaus Hentschel (2018, p. 81):

> In principle, it is possible to assign a number to each classical particle or to give it some other identifying marker because (theoretically at least) it is distinguishable from all the others. In the twentieth century when statistical mechanics was linked to the early quantum theory, it became evident that this no longer applies to the world of quanta. The indistinguishability of quantum particles stymies any attempt to identify or earmark them […]. A Polish physicist in Cracow, Ladislas Natanson (1864–1937), was the first to realize this […].

Thirdly, the researchers commenting Natanson's achievements thought that Natanson and Ehrenfest were the first to understand the concept identicality of physical objects. It was claimed by the following scholars: M.J. Klein (1959); A. Pais (1986, p. 283), and P. Pesic (1991; 2012).

According to Peter Pesic (2012, p. xii):

> Planck's counting exemplifies a concept of *identicality*, which joins *equality* of observable physical quantities (like mass or charge) to a radical *indistinguishability* that can confuse space-time historians (cf. Pesic 1991). This is a profound

theme of the quantum theory that Ladislas Natanson and Paul Ehrenfest were among the first to notice (1911) (cf. Pais 1986, p. 283; Klein 1959).[30]

3.5. H. Kragh's thesis and the refutation thereof

Helghe Kragh's thesis

Although the Brussels Conference on "The radiation and the Quanta" included all the key figures of quantum theory,

Fig. 9. The participants of "The radiation and quanta" Symposium. The First Solvay Conference (Brussels, 29 October – 4 November 1911). Source: http://upload.wikimedia.org/wikipedia/commons/c/ca/1911_Solvay_conference.jpg; http://en.wikipedia.org/wiki/Solvay_Conference. Seated (L-R): Walther Nernst, Marcel Brillouin, Ernest Solvay, Hendrik Lorentz, Emil Warburg, Jean Baptiste Perrin, Wilhelm Wien, Marie Curie and Henri Poincaré. Standing (L-R): Robert Goldschmidt, Max Planck, Heinrich Rubens, Arnold Sommerfeld, Frederick Lindemann, Maurice de Broglie, Martin Knudsen, Friedrich Hasenöhrl, Georges Hostelet, Edouard Herzen, James Hopwood Jeans, Ernest Rutherford, Heike Kamerlingh Onnes, Albert Einstein and Paul Langevin.

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[30] In contrast to all authors mentioned in Section 3.4, Enric Pérez Canals (2010) omits Natanson's contribution to the issue of indistinguishability in quantum theory. On the other hand, Marian Mięsowicz (1987, p. 550) overstated mistakenly that "Prof. Władysław Natanson was the first in the world to drew attention to the issue

> not all of the participants were concerned with quantum problems. Two of the reports, given by Jean Perrin and Martin Knudsen, did not deal with aspects of quantum theory (Kragh 2002, p. 71).

A refutation of H. Kragh's thesis

For the historical reason given below, it is impossible to agree with the statement that the First Solvay Conference held in 1911 "included all the key figures of quantum theory".

It suffices to repeat after A. Hermann (1971, p. 141) the list of scientists who, despite their interest in the quantum theory, were not invited to participate in the Solvay Conference. The list includes such scholars as: Arthur Erich Haas, Artur Schidlof, Ludwik Hopf, Ladislas Natanson, Peter Debye, Niels Bjerrum, Richard Gans, Pierre Weis, Emil Warburg, James Franck, Edgar Meyer, and Friedrich Paschen.

Furthermore M.J. Konieczny (2008, 2010; 2011; 2012) and N. Nagasawa ("Minamida" 2009; Nagasawa 2018) emphasize justly the fact that Ladislas Natanson was not invited to participate in the Conference, though he was at that time one of the best experts of the subject matter of "the Radiation and the Quanta".

3.6. Who, in the years 1911–1925, knew Natanson's works on Planck's theories of radiation and related issues? The results obtained so far by other researchers than the author of this article

The first researchers of this issue formulated two related theses:

- *F. Hund's thesis:* "In 1924 Natanson's arguments had been already forgotten" (Hund 1967, p. 123).
- *A. Kastler's thesis:* "A paper [Natanson 1911c – M.K.] which unfortunately remained unnoticed and unmentioned" (Kastler 1983, p. 617).

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of quantum statistics as early as 1911". („Na zagadnienie statystyk kwantowych, pierwszy w świecie zwrócił uwagę już w roku 1911 prof. Władysław Natanson".)

Then S. Bergia (1987; reprinted version 2009) made an empirical test of F. Hund's and A. Kastler's theses. He received both negative and positive results.

Negative results:

- S. Bergia (1987, p. 234; reprinted version 2009, p. 344) could not find a reference to Natanson in Planck's research papers.

Positive results:

- S. Bergia listed four scientists that read Natanson's work (1911c):
  1) M. Masius – The English translator of the 2nd ed. of Planck's *Vorlesungen über die Theorie der Wärmestrahlung* (1914).[31]
  2) George Krutkow[32] (1914a) gave a reference to Natanson's paper (1911c).[33]
  3) Mieczysław Wolfke (*although S. Bergia does not show any reference where Natanson is cited*).[34]
  4) Paul Ehrenfest and Heike Kamerlingh Onnes (*although S. Bergia does not show any reference where Natanson is cited*).[35]
  5) Louis de Broglie (*although S. Bergia does not show any reference where Natanson is cited*).[36]

Then N. Nagasawa ("Minamida" 2009; Nagasawa 2018) made a subsequent empirical test of F. Hund's, A. Kastler's and S. Bergia's theses.

Negative results:

- N. Nagasawa could not find a reference to Natanson in Einstein's research papers.

Positive results:

- N. Nagasawa listed eight scientists that read Natanson's work:
  1) Max Planck (Solvay Congress's talk, then he cited Natanson's article (1911c) in his own article published in Proceedings).[37]

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31 Bergia 1987, p. 234 (reprinted version 2009, p. 344).

32 George Krutkow this is Iurii Aleksandrovich Krutkov.

33 Bergia 1987, p. 235 (reprinted version 2009, p. 345).

34 *Ibid.*, pp. 235–236, 239–240 (reprinted version 2009, pp. 345–346, 349–350).

35 *Ibid.*, pp. 236–239 (reprinted version 2009, pp. 346–349).

36 *Ibid.*, pp. 240–243 (reprinted version 2009, pp. 350–353).

37 According to N. Nagasawa ("Minamida" 2009 / Nagasawa 2018, p. 397), it was in fact a negative citation: "These calculations [made by Planck himself] are complete

2) Arnold Sommerfeld knew Natanson's article (1911c; published: 15 August 1911) as evidenced by Sommerfeld's letter sent to Natanson from Münich, dated October 3, 1911. However, he did not cite it in his works.[38]
3) Paul Ehrenfest knew Natanson's article (1911c; published: 15 August 1911) as evidenced by Ehrenfest's letter sent to Sommerfeld from St. Petersburg, dated October 16, 1911.[39] However, he did not quote this Natanson's work in his articles.
4) M. [Morton] Masius – the English translator of the 2nd ed. of Planck's *Vorlesungen über die Theorie der Wärmestrahlung* (1914) mentions two Natanson's works (1911c; 1912b).[40]
5) Friedrich Krüger, the chief editor of the *Physikalische Zeitschrift (Danzig-Langfuhr)*, knew both the English version of Natanson's article (1911a), and the German version (1911c) as evidenced by two Krüger's letters sent to Natanson from Berlin, dated April 24, 1911 and July 22, 1911.
6) M. Iklé, who translated Natanson (1911a) for the *Physikalische Zeitschrift* (Natanson 1911c), knew also both versions of Natanson's article.

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and do not contain such uncertainty that recently Natanson described in the "Phys. Zeitschr." (Planck 1911a)». (Nagasawa repeated this translation after O. Darignol (1991, p. 254)). Moreover, according to Nagasawa: "This is the only instance that we can find of a third party referring to Natanson's paper before World War II." I will show in the further part of the article that O. Darignol and N. Nobukata Nagasawa are wrong in both of these points.

[38] Cf. Nagasawa 2018, pp. 397–398.

[39] Cf. *Ibid.*, pp. 398–399.

Moreover, Ehrenfests doesn't mention Natanson's article in their review work finished in September 1911: Ehrenfest, Ehrenfest-Afanaseva 1911 (Supplements completed in September 1911), p. 84 fn. 237 / (English translation) 1959, p. 104, fn. 245. In this context it is cited only the article of P. Ehrenfest (1911).

[40] Planck mentioned them in 1914 in Appendix II. "References", included in his English translation of Max Planck's *Vorlesungen über die Theorie der Wärmestrahlung* (2nd ed., 1913). We read here that the appendix gives "a list of the most important papers on the subject treated in this book and others closely related to them" and was created "with Professor Planck's permission" (see: Planck 1914, p. iv).

S. Bergia 1987, p. 234 (reprinted version 2009, p. 344) doubts that Max Planck did so, and thinks that Natanson's works were included in the appendix without the knowledge of Planck. However, Bergia does not show any evidence for this statement. In contrast, I do not doubt the words of M. Maius. I explain this in the further part of this article.

7) Samuel Hawksley Burbury[41] – his note #344 (1911) includes only the bibliographic records of Natanson's articles: 1911a and 1911c without a review of these works.
8) Edwin Henry Barton[42] – his article #733 (1912) is a long positively evaluated review of Natanson's article (1912b).[43]
9) Japan physicists Hantaro Nagaoka (1865–1950) and Jun Ishiwara (1881–1947) could have known and read Natanson's article (1911a), because the former had a copy of this article (and five other Natanson's works published in Kraków from 1904 to 1931), and the latter because he used to be a student of the former. Nevertheless, they do not cite the Natanson's article in their works, particularly in the article of Ishiwara (1911–1912; in German, and published in Tokyo).[44]

4. Who knew Natanson's works on Planck's theory of radiation and related issues in the years 1911–1925? The results of the author of this article

Following the footsteps of A. Kastler, S. Bergia and "R. Minamida"/ N. Nagasawa, I looked in my research for recipients of Natanson's works on the subject under discussion. First of all, I analysed once again the course and content of two conferences of 1911: the first in Kraków and the second in Brussel. Then I studied Natanson's correspondence and I sought the publications that cited Natanson's works.

4.1. 11th Congress of Polish Physicians and Natural Scientists in Kraków (18 – 22 July, 1911) – Einstein, Natanson, Smoluchowski, and Olszewski

At the beginning of 1911 Albert Einstein intended to participate in the 11th Congress of Polish Physicians and Natural Scientists in Kraków

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[41] Samuel Hawksley Burbury (1831–1911), a British mathematician and physicist. He died on 18 August 1911. According to "Minamida" 2009 / Nagasawa 2018 (pp. 407–408) it was the reason that Natanson's work was not reviewed by Burbury. I think it is a very probable explanation.

[42] Edwin Henry Barton (1859–1925) was professor of experimental physics at University College, Nottingham.

[43] Cf. "Minamida" 2009/Nagasawa 2018, p. 408.

[44] Cf. *ibid.*, pp. 399–402.

(18–22 July, 1911) but he finally decided against this idea. We know that from the draft letter written by Einstein in Prague before 21 July 1911 and sent to the Institute of Physics of the University of Kraków directed by nobody else but the protagonist of this paper Władysław Natanson[45]

The received message was paraphrased in *Dziennik XI. Zjazdu Lekarzy i Przyrodników Polskich w Krakowie* ("The Daily Proceedings of Polish Physicians and Natural Scientists in Kraków"), No. 4, 21 July 1911, p. 4 (see below).

> Znakomity uczony prof. Einstein z Pragi nadesłał bardzo serdeczną depeszę od [do – M.K.] sekcyi fizycznej.
>
> The illustrious scientist professor Einstein sent from Prague a heartfelt message to the physical section {of the 11th Congress of Polish Physicians and Natural Scientists} [translation – M.K.].

During this 11th Congress of Polish Physicians and Natural Scientists, on 19 June, 1911, the 2nd General Session of the Section of Exact Sciences held talks of two scholars: M. Smoluchowski, "Atomistyka współczesna" ("Contemporary atomics"), W. Natanson, "O promieniowaniu" ("On radiation"). After these talks Karol Olszewski gave an additional lecture on cryogenic instruments combined with an exhibition of these instruments.

We know that all these three speakers were eminent scientists,[46] and that all the three talks were highly evaluated by over 100 participants of the section of exact sciences. It is evidenced by the report from these events in *Dziennik XI. Zjazdu Lekarzy i Przyrodników Polskich* 1911, No. 3, p. 1:

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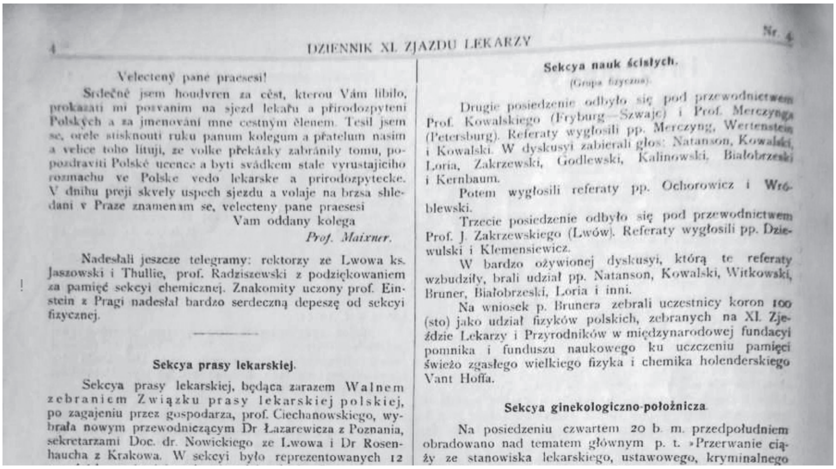
[45] See document 273 in German, in: *The Collected Papers of Albert Einstein*, vol. 5, *The Swiss Years: Correspondence, 1902–1914*, p. 306; and its English translation by Anna Beck in: Einstein 1995, p. 195. That fact was discussed by Nobukata Nagasawa ("Roh Minamida" 2009, p. 3), however without mentioning the source of the information; he did so later (Nagasawa 2018, pp. 396–397).

[46] Smoluchowski deserved the Nobel Prizes in chemistry and physics, but he did not receive them, because he died in 1917. He should have received it in 1925 in chemistry together with an Austrian of Hungarian origin, Richard Zsigmondy, professor at the University of Göttingen, and / or in 1926, together with Teodor Svedberg, Swedish professor at the University of Uppsala, whose experimental work on colloids was closely


4 DZIENNIK XI. ZJAZDU LEKARZY Nr. 4

Velecteny pane praesesi!

Srdečné jsem houdvren za cést, kterou Vám libilo, prokazati mi pozvanim na sjezd lekařu a přirodozpyteni Polských a za jmenování mne cestným členem. Tesil jsem se, orele stisknouti ruku panum kolegum a přatelum nasim a velice toho lituji, ze volke překážky zabránily tomu, po-pozdraviti Polské ucence a byti svádkem stale vyrustajiciho rozmachu ve Polske vedo lekarske a prirodozpytecke. V dnihu preji skvely uspech sjezdu a volaje na brzsa shledani v Praze znamenam se, velecteny pane praesesi

Vam oddany kolega

Prof. Maixner.

Nadesłali jeszcze telegramy: rektorzy ze Lwowa ks. Jaszowski i Thullie, prof. Radziszewski z podziękowaniem za pamięć sekcyi chemicznej. Znakomity uczony prof. Einstein z Pragi nadesłał bardzo serdeczną depeszę od sekcyi fizycznej.

Sekcya prasy lekarskiej.

Sekcya prasy lekarskiej, będąca zarazem Walnem zebraniem Związku prasy lekarskiej polskiej, po zagajeniu przez gospodarza, prof. Ciechanowskiego, wybrała nowym przewodniczącym Dr Łazarewicza z Poznania, sekretarzami Doc. dr. Nowickiego ze Lwowa i Dr Rosenhaucha z Krakowa. W sekcyi było reprezentowanych 12

Sekcya nauk ścisłych.

(Grupa fizyczna).

Drugie posiedzenie odbyło się pod przewodnictwem Prof. Kowalskiego (Fryburg—Szwajc) i Prof. Merczynga (Petersburg). Referaty wygłosili pp. Merczyng, Wertenstein i Kowalski. W dyskusyi zabierali głos: Natanson, Kowalski, Loria, Zakrzewski, Godlewski, Kalinowski, Białobrzeski i Kernbaum.

Potem wygłosili referaty pp. Ochorowicz i Wróblewski.

Trzecie posiedzenie odbyło się pod przewodnictwem Prof. J. Zakrzewskiego (Lwów). Referaty wygłosili pp. Dziewulski i Klemensiewicz.

W bardzo ożywionej dyskusyi, którą te referaty wzbudziły, brali udział pp. Natanson, Kowalski, Witkowski, Bruner, Białobrzeski, Loria i inni.

Na wniosek p. Brunera zebrali uczestnicy koron 100 (sto) jako udział fizyków polskich, zebranych na XI. Zjeździe Lekarzy i Przyrodników w międzynarodowej fundacyi pomnika i funduszu naukowego ku uczczeniu pamięci świeżo zgasłego wielkiego fizyka i chemika holenderskiego Vant Hoffa.

Sekcya ginekologiczno-położnicza.

Na posiedzeniu czwartem 20 b. m. przedpołudniem obradowano nad tematem głównym p. t. »Przerwanie ciąży ze stanowiska lekarskiego, ustawowego, kryminalnego

Fig. 10. *Daily Proceedings of the Congress of Polish Physicians and Natural Scientists in Cracow*, No. 4, 21 July 1911, p. 4. Source: © Biblioteka Jagiellońska; photo: © Michał Kokowski.

The meeting was held under the chairmanship of prof. Dickstein from Warsaw with a large audience (over 100 people). The talks of prof. Smoluchowski from Lviv and

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related to Smoluchowski's theoretical work. He should have also received it in 1926 in physics, together with Jean Perrin, French professor at the University of Sorbonne in Paris for experimental work on the Brownian motion, confirming the validity of the molecular theory of Einstein-Smoluchowski. Cf. Zsigmondy 1926; Perin 1926; Svedberg 1927; Crawford, Heilbron, Ulrich 1987; Wróblewski 2012; The Nobel Prize 2019.

Olszewski was nominated for the Nobel Prize in physics twice: in 1904 (nominator: Nikolay Umov from Moscow State University) and in 1913 (nominators: Ladislav Natanson, August Witkowski, Constantin Zakrzewski, Maurycy Rudzki, all from the Jagiellonian University in Kraków), and once for the Nobel Prize in chemistry in 1913 (nominator: Karl Dziewonski from the Jagiellonian University in Kraków) – cf. Crawford, Heilbron, Ulrich 1987; Wróblewski 2012; The Nobel Prize 2019. Why Olszewski did not receive this award is a mystery to me, which can be explained on the basis of the analysis of external factors in science (i.e. international politics and sociology).

Natanson was also a very good physicist. In this article I focus on showing the reception of Natanson's achievements in statistics of radiation and related themes. Regarding the general description of Natanson's achievements in physics, cf. Weyssenhoff 1937 (pp. 289–294 (in English)); 1958 (pp. 120–124 (in Polish)); Średniawa 1985, pp. 89–90, 116–117; 2000, pp. 447–458; 2001, pp. 104–107. Regarding Natanson's achievements in theory of irreversible and reversible phenomena, cf. Kokowski 1993; 1994; 1997.

Fig. 11. Marian Smoluchowski, circa 1914. Source: http://web.archive.org/web/20051227102418/http://www.zwoje-scrolls.com/zwoje35/portret-krakow.jpg

Fig. 12. Władysław Natanson, circa 1910. Source: *Władysław Natanson 1864–1937* (2009), illustration 14.

Fig. 13. Karol Olszewski, "A king of low temperatures", circa 1900. Source: *Wikipedia* (https://commons.wikimedia.org/wiki/File:Karol_Olszewski.jpg).

> Natanson from Kraków, touching on the deepest issues of contemporary physics and chemistry, made a deep impression on the listeners and met with the warmest of receptions.
>
> The meeting was continued at the First Chemical Laboratory of the Jagiellonian University, where prof. Olszewski arranged an exhibition of equipment used to liquefy gases, mostly of their own construction. In his lecture, the lecturer gave an outline of his classical research and, in addition, demonstrated his latest instruments for separating oxygen and nitrogen from liquefied air, as well as a device for simultaneous condensation of air and hydrogen.
>
> The exhibition arranged by prof. Olszewski with a great deal of work casts a bright light on the development of these studies, whose fruits played such an outstanding role in the development of science.[47]

Hence, there is no doubt that over 100 Polish physicists and chemists participating on 19 June, 1911 in Natanson's talk during the 11th Congress of Physicians and Naturalists knew at least one Natanson's publication on the statistics of radiation (Natanson 1911b).

Moreover, we know from Natanson's correspondence that long before this congress, on 22 December 1910, Natanson shared with Smoluchowski his view on problem of Planck's theory of radiation:

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[47] "Posiedzenie odbyło się pod przewodnictwem prof. Dicksteina z Warszawy przy bardzo licznym udziale słuchaczy (przeszło 100). Odczyty prof. Smoluchowskiego ze Lwowa i Natansona z Krakowa, poruszające najgłębsze zagadnienia współczesnej fizyki i chemii wywarły na słuchaczach głębokie wrażenie i spotkały się z gorącem uznaniem.

Dalszy ciąg posiedzenia miał miejsce w I. pracowni chemicznej uniwersytetu, gdzie prof. Olszewski urządził wystawę aparatów, przeważnie własnej konstrukcji, służących do skraplania gazów. W swym referacie, podał prelegent historyczny zarys swych klasycznych badań a oprócz tego zademonstrował najnowsze swe przyrządy do oddzielania tlenu i azotu ze skroplonego powietrza, jako też przyrząd do równoczesnego skraplania powietrza i wodoru.

Wystawa urządzona przez prof. Olszewskiego z wielkim nakładem pracy, rzuca jasne światło na rozwój tych badań, których owoce tak wybitną rolę odegrały w rozwoju nauki" (*Dziennik XI. Zjazdu Lekarzy i Przyrodników Polskich* 1911, No. 3, p. 1).

> Dear Sir, […] I look […] forward to your announced Congress speech [“Contemporary Atomics”]. Doctor Zakrzewski insists on me to speak of “atomics in energetics” in a similar manner and I almost agreed; but perhaps I will be relieved of the promise if “quanta” disappear as quickly as it has been in science so far; until July, there may not be any atomics in energetics and I will have nothing to talk about. It is a beautiful theory, the one of radiation; it's just a shame that what is solid and perfectly justified, i.e. equipartition, does not agree with the facts, and Planck, who agrees, seems to have no basis for that at all! It seems to me that Planck, even in his own way, should have received $R\nu\lambda^{-4}$ (Rayleigh-Jeans law) and I cannot understand why his formula is true. These are very difficult *problemata* [translated by M.K.].[48]

In another letter of 26 April 1911 r. Natanson writes to Smoluchowski:

> We have already been thinking for a few months with Mr. Zakrzewski about applying to metals – instead of the usual equipartition Maxw. [Maxwellian] theory of electron motion – the Einsteinian-Planckian theory of (eε/kυ – 1) etc. Well, it's so difficult to concentrate. There is still a lot to do in the matter itself; we are not quite ready yet[49] [translated by M.K.].

[48] “Cieszę się […] z zapowiedzianego referatu Szan[ownego] Pana na Zjeździe. Dr. Zakrzewski nalega na mnie, abym mówił podobnie o «atomistyce w energetyce» i niemal że [niemalże] się zgodziłem; lecz może będę zwolniony zobietnicy [z obietnicy], jeżeli «quanta» będą znikały równie prędko, jak dotych czas [dotychczas] z Nauki; do Lipca [lipca] może Atomistyki w Energetyce już wcale nie będzie i nie będę miał o czem mówić. Przepiękna to jest teoria, ta Promieniowania; tylko szkoda, że to, co jest mocne i znakomicie uzasadnione, ekwipartycya [ekwipartycja], nie zgadza się z faktami, a Planck, który się zgadza, zdaje mi się, że nie ma wcale podstaw! Tak mi się wydaje, że Planck, nawet na swojej <u>własnej</u> drodze powinien był otrzymać $R\nu\lambda^{-4}$ (p. [prawo] Rayleigha-Jeansa), i nie mogę zrozumieć, czemu jego wzór jest prawdziwy. Bardzo są to trudne problemata” (A letter from Władysław Natanson to Marian Smoluchowski, Kraków, 22 December, 1910; transcription – M.K.; see Natanson 1910 (*archival document*), folium 60 verso).

[49] “Z p. Zakrzewskim już od kilku miesięcy myślimy o tem [tym], że do metali zastosować, zamiast zwykłej ekwipartycyjnej Maxw. [Maxwellowskiej] teoryi ruchu elek-

Hence, it is certain that Marian Smoluchowski knew Natanson's views on Planck's theory of radiation and the related Einstein's issues also directly from their private correspondence.

Moreover, because Albert Einstein (1879–1955) was to be a participant of the 11th Congress for Polish Physicians and Natural Scientists (Kraków, 18–22 July 1911), he was probably informed about the planned lectures of Smoluchowski, Natanson, and Olszewski, as well as on Natanson's first article (1911a; published circa 10 April 1911). However, so far, there are no historical documents that would prove this hypothesis.

4.2. The First Solvay Conference (Brussels, 29 October – 4 November 1911) – Planck, Natanson, and Einstein

During the First Solvay Conference held "The radiation and quanta" Symposium. Among participants of this meeting were also Max Planck, Arnold Sommerfeld, Paul Ehrenfest and Albert Einstein. Planck had a talk entitled: "La loi du rayonnement noir et l'hypothèse des quantités élémentaires d'action" ("The law of black radiation and the hypothesis of elementary quantities of action").[50]

At that time or a bit later Planck not only knew Natanson's article (1911c), but also understood its essence, as evidenced by his remark in his article (1912) published in the Proceeding of this conference:

> Ce calcul ne prête à aucune ambiguïté et ne renferme en particulier plus rien de l'indétermination dont L. Natanson a récemment parlé dans le *Phys. Zeitschr.*, t. XII, 1911 [i.e. Natanson 1911c], p. 659 (Planck 1912, p. 104, fn. 1).[51]

To avoid doubts, let us to cite its equivalent German and English translations:

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tronów – Einstein Plankowską teoryę z ($e^{\varepsilon/k\upsilon}$ – 1) etc. Cóż kiedy tak tudno o możność skupienia się. W samej kwestyi, jest jeszcze dużo do zrobienia; nie jesteśmy jeszcze w zupełnym porządku" (Natanson 1910 (*archival document*), folium 71 recto).

[50] Cf. Solvay et al. 1912, pp. 93–114, 115–132.

[51] To my knowledge, it is the first citation of a German version of Natanson article (1911c), and the second citation after Zakrzewski (1911, p. 329 fn. 1), who cited the English version of Natanson's article (1911a).

> Diese Berechnung ist vollkommen eindeutig und enthält insbesondere nichts mehr von der Unbestimmtheit, welche L. Natanson neuerdings mit Recht zur Sprache gebracht hat (translated by Arnold Eucken 1914; cited by Lange 1992, p. 22, and by Straumann 2011, p. 12).
>
> This calculation is completely unambivalent and in particular no longer contains the indefiniteness about which L. Natanson has recently spoken with justification (firstly translated by Stahel 2000, p. 246; repeated by Straumann 2011, p. 12).[52]

Hence, it is certain that Planck appreciated Natanson's article (1911c),[53] and many scientists, including Einstein, could read Planck's article and learned about the existence of Natanson's article.

4.3. List of the scientists who knew and appreciated Natanson's works (author's results)

1) Marian Smoluchowski (1872–1917), professor of the Lviv University (1900–1912) and professor of the Jagiellonian University (1912–1917), a friend of Natanson, knew about his first objections regarding Planck's considerations[54] and that Natanson together with Zakrzewski planned to extend "the Einstein-Planck theory ($e^{\varepsilon/k\upsilon}$ – 1) etc. to metal"[55]; Smoluchowski knew

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[52] This English translation correctly reflects the content of the French original and its German translation – please compare them with the English translation provided by Nobukata Nagasawa ("Minamida" 2009 / Nagasawa 2018, p. 397) – see fn. 37.

More about the first conference, cf. Mehra 1975, pp. 12–72 (pp. 24–40, about Planck lecture).

[53] Moreover, it was Max Planck, who supported the nomination of Władysław Natanson to the Deutscher Physikalischen Gesellschaft at the end of January 1913, see Appendix 3. It is obvious that he would not have done it, if he had not valued Natanson's scientific achievements.

Therefore, in contrast to S. Bergia (1987, p. 234; reprinted version 2009, p. 344) – cf. fn. 40 above – I think that M. Maius included in the Appendix II two Natanson's works (1911c, 1912b) with the knowledge of Planck himself.

[54] The letter of 22 December 1910 from Władysław Natanson to Marian Smoluchowski (see Natanson 1910 (*archival document*), folium 60 verso).

[55] The letter of 26 April 1911 from Władysław Natanson to Marian Smoluchowski (see Natanson 1911c (*archival document*), folium 71 recto). Natanson considered this issue in his article of 1912 and Zakrzewski in his article of 1911.

Natanson's articles: 1911a; 1911b; 1911c; and probably also other Natanson's works excluding article of 1924b (since he died in 1917).[56]

2) Władysław Gorczyński (1879–1953), a Polish meteorologist from the Meteorological Office at the Museum in Warsaw, knew at least the first Natanson's article (Natanson 1911a).[57]
3) Konstanty Zakrzewski (1876–1948), professor of the Jagiellonian University (1911–1913, and since 1917) and professor of the Lviv University (1913–1917), Natanson's college, a co-organizer of the Section of Exact Sciences during the 11th Congress for Polish Physicians and Natural Scientists (Kraków, 18––22 July, 1911),[58] knew at least six works (Natanson 1911a; 1911b; 1911c; 1912a; 1912b; 1913); he quoted the first Natanson's work (1911a) in his own article (Zakrzewski 1911, received: 3 April 1911, p. 329 fn. 1)[59]; he co-operated with Natanson and planned to extend the Einstein-Planck theory to metal;[60] in a letter on 23 January 1916, he discussed with Natanson a plea of plagiarism against George Jaffé's article from *Annalen der Physik* (1914) about optical properties of metals that repeated many formulas published earlier in Zakrzewski's article in German (1911a), which also cited Natanson (1911a; received: 6 March 1911; published: circa 6 April 1911);[61] and then he

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[56] In the letter of 30 March 1911 (see Natanson 191b (*archival document*), folium 69 verso), Natanson informed Smoluchowski that he would send in a week or ten days the article on Radiation from the *Bulletin* (i.e. 1911a). On 19 June 1911 in Kraków, Smoluchowski attended Natanson's lecture on radiation – cf. above Section 4.1, so he most likely knew also at least the text of the lecture (Natanson 1911b) and possibly also its reprint (1912a).

[57] In the letter of 10 April 1911 (Gorczyński 1911 (*archival document*)), he thanked Natanson for sending a copy of this article. About Gorczyński's career, see Środka 1983.

[58] Cf. *Dziennik XI. Zjazdu Lekarzy i Przyrodników Polskich w Krakowie* (*Daily Proceedings of Polish Physicians and Natural Scientists in Cracow*), No. 1, 21 July 1911, p. 10.

[59] To my knowledge, it is the first citation of Natanson's article (1911a) in literature, but according to N. Nagasawa only Max Planck cited in 1912 Natanson's work on radiation and related matters (1911c) – cf. fn. 37 above.

[60] See fn. 55.

[61] Zakrzewski 1916 (*archival document*).

published this objection in *Annalen der Physik* (Zakrzewski 1916, received: 10 February 1916).

4) A group of over 100 Polish physicists and chemists (including Smoluchowski, Zakrzewski) that on 19 June, 1911 participated at the 2nd General Session of the Section of Exact Sciences during the 11th Congress for Polish Physicians and Natural Scientists which was held between the 18th and the 22nd of July in 1911 in Kraków. They heard the Natanson's lecture entitled "O promieniowaniu" ("On radiation"), published as Natanson 1911b, and also the Smoluchowski's lecture "Atomistyka współczesna" ("Contemporary atomics"), published as Smoluchowski 1911.[62]
5) Jean Becquerel (1878–1953) could knew Natanson 1911a – he received this work from Natanson himself on which informs the handy annotation of Natanson.[63]
6) Hugo von Seeliger (1849–1924) informed about Natanson 1911a, 1912b in his notes in *Jahrbuch über die Fortschritte der Mathematik* (1912, 1913).
7) Arnold Sommerfeld knew Natanson's article (1911c; published: 15 August 1911) as evidenced by Sommerfeld's letter sent to Natanson from Münich, dated October 3, 1911 – see Sommerfeld 1911 (*archival document*).[64]
   However, he did not cite it in his works, including Sommerfeld 1911a (a paper given on 25 September 1911 in Karlsruhe during

[62] See Section 4.1. It is linked with an evaluation of quality of Polish physics in 1910s.

[63] The work belonged to the Library of Becquerel family. It could be bought in 2011 in one antiquarian bookstore.

[64] The fact was noticed by N. Nagasawa 2018, who quoted and translated an appropriate excerpt of this letter into English: "[...] Ich bin Ihnen aufrichtig dankbar, dass Sie mir regelmässig Ihre sehr interessanten Arbeiten zusenden, die ich stets genau verfolge; ich werde mich bald mit meinem Carlsruher Vortrag über Quantentheorie und einigen Anderen revangieren. […]" (cited by Nagasawa 2018, p. 397). In English translation: "[…] I sincerely appreciate that you regularly send me your very interesting works, which I always thoroughly follow; I will soon return the favour with my Karlsruhe lecture on quantum theory, and some other papers. […]" (Nagasawa 2018, p. 398).

On A. Sommerfeld's scientific biography, cf. Eckert 2013a (German) / 2013b (English).

the 83rd Meeting of the German Natural Scientists and Medical Doctors Association) and Sommerfeld 1911b.[65]

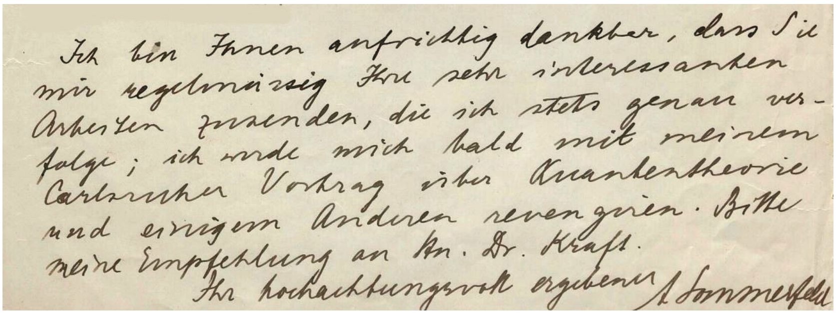
Ich bin Ihnen aufrichtig dankbar, dass Sie mir regelmässig Ihre sehr interessanten Arbeiten zusenden, die ich stets genau verfolge; ich werde mich bald mit meinem Carlsruher Vortrag über Quantentheorie und einigem Anderen revanchieren. Bitte meine Empfehlung an Hn. Dr. Kraft.
Ihr hochachtungsvoll ergebener A. Sommerfeld

Fig. 14. The excerpt of Sommerfeld's letter sent to Natanson from Münich, dated October 3, 1911. Source: Sommerfeld 1911 (*archival document*), folium 107 recto. Photo: © Michal Kokowski.

8) Paul Ehrenfest (1880–1933), who worked in Saint Petersburg Polytechnic Institute between the summer of 1907 and September 1912, and then succeeded Hendrik Antoon Lorentz (1853–1928) in the chair of theoretical physics at the University of Leiden in 1912, knew Natanson's article (1911c) and discussed about it with Sommerfeld in the letter of 16 October 1911.[66]

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[65] On the ground of rational and ethical argumentation, I do not understand why A. Sommerfeld did not cite Natanson's works. Perhaps other, irrational reasons had to decide on their attitude to Natanson. Cf. Sections 5 and 9.

[66] Ehrenfest sent from St. Persburg a letter dated 16 October, 1911 to Sommerfeld (Ehrenfest 1911b (*archival document*)) that showed that both knew Natanson article. The fact was noticed by "Minamida" 2009 / Nagasawa 2018, pp. 398–399, who quoted an appropriate excerpt of the letter and translated it into English.

Archiv: München, DM (Archiv HS 1977-28/A,76): "Die Bemerkungen, die kürzlich Nathanson [sic!] über die combinatorischen Grundlagen der Planckschen Theorie publiziert hatte ich ebenfalls gefunden und vor dem Erscheinen der Arbeit von Nathanson in der hiesigen physikalischen Gesellschaft vorgetragen. Aber Nathanson hat die Lösung der Schweierigkeit nicht gefunden: er hat eben nicht bemerkt, daß die Planckschen und Einsteinsche Hypothese total verschieden sind" (cited by Nagasawa 2018, p. 398). In English translation: "Remark: I had also found the recent publication of Nathanson on the combinatorial foundations of Planck's theory. I had presented [the idea] at the local physical society before Nathanson's paper was published.

However, he did not quote this Natanson's article or other works of the author in his own works.[67]

9) Max Planck (1858–1947) not only knew Natanson's article (1911c) and understood its essence, but also appreciated it. Planck stated so clearly in his paper in the Proceedings of the First Solvay Congress (held from 30 October to 3 November 1911) – cf. Planck 1912, p. 104, fn. 1. So, the other participants of the First Solvay Congress, including Albert Einstein, Arnold Sommefeld, and Paul Ehrenfest, had to know about Natanson's article (1911c). Moreover, at the end of January 1913, it was Planck who supported Natanson's efforts to become a member of the German Physical Society and recommended his candidature.[68]
10) Iurii A. Krutkov (1890–1952), a student of Paul Ehrenfest in Saint Petersburg Polytechnic Institute (i.e. 1908–1912), and his co-worker in Leiden University (from the summer months of 1913 to the beginnings of March of 1914),[69] published two papers: Krutkow[70] 1914a (received 6 January 1914) and 1914b (received 8 March 1914). The former article mentions Natanson 1911c on pp. 134 and 136, and the latter – Natanson 1911a and 1911c on p. 363.[71] Moreover, the former article states that

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But Nathanson did not find the solution to the difficulty: he did not notice that Planck's and Einstein's hypotheses are totally different" (translated by Nagasawa 2018, pp. 398–399).

On P. Ehrenfest scientific biography, cf. Klein 1985; Huijnen, Kox 2007. Regarding Ehrenfest's views on statistics of radiation and related matters, cf. also Navarro, Pérez 2004.

[67] This is the same behavior as in the case of A. Sommerfeld. Hence, fn. 64 should be referred also to P. Ehrenfest.

[68] Cf. Appendix 3.

[69] On Krutkov's biography cf. Frenkel' 1970; *Encyclopedia.com* 2019.

[70] Please note that I.A. Krutkov's works are mentioned in the bibliography under the name "George Krutkow" because he used this form in his articles published in the German journals.

[71] To my current knowledge and in contrast to N. Nagasawa (cf. fn. 37 above), it is the second (after Planck 1912) citation of the German version of Natanson's article (1911c); and the second citation (after Zakrzewski 1911) of the English version (1911a).

Konstanty Zakrzewski informed Natanson about Krutkov's article (1914a) in the letter on 10 February 1914 (Zakrzewski 1916 (*archival document*), folium 52 verso –

Planck's assumption of independent light quanta leads to Wien's radiation formula, and the latter that Wolfke's assumption of the light atoms leads to the same Wien's radiation formula, and the basis of these conclusions were combinatorial and statistical considerations made earlier by Ehrenfest 1911a (received: 8 July 1911) and Natanson 1911a (presented: 6 March 1911; published circa 10 April 1911); 1911c (received: 29 Aprill 1911; published: 15 August 1911).

11) Krutkov's articles were well-known by scientists, and Ehrenfest contributed himself to this: he firstly informed Hendrik Antoon Lorentz (1853–1928) about Krutkov's article (1914a).[72] Then together with Kamerlingh published an article (Ehrenfest, Kamerlingh 1914; communicated in the meeting of 31 October 1914; published on December 31, 1914; English translation 1914) where two Krutkov's works are cited (1914a; 1914b).

12) It is very probable that Ehrenfest, who had known Natanson's article (1911c), contributed to that his student Krutkov knew Natanson's articles (1911c; 1911a). In the opposite, and less probable case, the second Krutkov's article (1914b) resulted in Eherenfest also knowing about the existence of the primary English version of Natanson's article (1911a).[73] Regardless of

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folium 53 recto): "Przerzucałem natomiast w ostatnim «Physik. Zeitsch.» rozprawę Krutkowa [1914a] i cieszę się, że Pańska praca o promieniowaniu znalazła uznanie. Choć doprawdy od promieniowania i quantów głowa już puchnie: coraz to coś nowego, a stare wątpliwości wcale się nie zmniejszają". In an English translation: "I was browsing Krutkov's article in the last «Physik. Zeitsch.» [1914a] and I am glad that your work about radiation has been appreciated. Though indeed radiation and quanta already give me headaches: there is more and more of the new, and old doubts are not reduced at all" [transl. – M.K.].

[72] Cf. the letter from Ehrenfest to Lorentz (24 January 1914), cited by Darrigol 1991, p. 255, fn. 21, and see also Lorentz 2018, Doc. 155, pp. 392–395.

O. Darrigol described Krutkov as "a student of Einstein's in Leiden." However we know that Krutkov was an assistant of Ehrenfest already in St. Petersburg, and he was in Leiden in 1913/1914 (cf. Lorentz 2018, p. 395, fn. 5; Frenkel' 1970; van Lunteren 2003).

[73] Ehrenfest had a copy of Krutkov's work (1914b), since on its back he wrote a draft of a letter to Albert Einstein (regarding his answer to Einstein's comment about rotating electrons in a magnetic field) – see: Einstein 1998a, p. 15, Doc. 4 (English translation) Einstein 1998b, p. 11, Doc. 4.

that Ehrenfest did not quote Natanson's publications on radiation statistics and related issues in his own publications.[74]

13) Mieczysław Wolfke, a privat-docent in the ETH (1913–1914), a docent in the University of Zurich (1914–1922), and then a professor at Warsaw Politechnic, and a college of Einstein from ETH (where Einstein was a professor), did not cite Natanson's works in a series of his articles from 1913 to 1914.[75] Nevertheless he knew about the existence of Natanson's works on radiation (and related subjects), at least indirectly by reading the articles of Krutkov (1914a; 1914b – this is a polemic with Wolfke). Moreover, Wolfke knew also a revised version of Natanson's article 1911b, i.e. 1924b because in the letter of 3 September 1924, he thanked Natanson for sending him Natanson's book (1924a) which included the article – see Wolfke 1924 (*archival document*).[76]
14) Max Born (1882–1970)[77] and Theodore von Kármán (1881–1963) not only knew Natanson's article (1912b), but they

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[74] It is evidently a negative behaviour. I will consider this problem later.

[75] It is a contrary view to S. Bergia (1987, p. 235; repr. 2009, p. 345).

[76] About Wolfke's views on "atoms of light", see Mehra, Rechenberg 1982, p. 559; Bergia 1987, pp. 235–236, 239–240 (reprinted version 2009, pp. 345–346, 349–350); Howard 1990, pp. 76–78.

[77] Max Born was a German Jew, born in Breslau (Wrocław), converted to Lutheranism in 1913 (cf. G.V.R. Born 2002) and, according to some of his biographers, until 1933 (or a bit earlier) he was an advocate of German nationalism or Pan-German chauvinism (cf. Wolff 2003, pp. 337–338; Bromberg 2006). One example of such attitude of 23 November 1914 and 23 February 1915: "The *Physikalische Zeitschrift* printed the names of colleagues fighting at the front, and of those who had been decorated, wounded, or killed. Photographs framed in black accompanied obituaries of the fallen. One of the journal's editors, Max Born, explained that it wanted to demonstrate to foreign countries that «physics too is at one with the fatherland in this time of peril and danger»" (Wolff 2003, pp. 337–338).

However, later he was a member of "Vereinigung Gleichgesinnter" (Association of People with the Same Opinion) founded on 8 June 1916. It was "a confidential discussion group of pacifist intellectuals, whose aim was to make the theoretical discussions concerning internal and foreign politics [...] in the spirit of overcoming unethical nationalism and of replacing power politics by politics based on ethics, and [...] to influence the press and to reestablish international relations with scientists abroad as soon as this might become possible again" (Goenner, Castagnetti 1996, pp. 26–27). Then, with the development of Nazi anti-Semitism in Germany, the loss mentioned this fact in their paper (1913a) in the context of priority of the solution to the problem of specific heats of crystal solids by Natanson, Debye, and Born and von Kármán.[78] Hence, Max Born and Theodore von Kármán knew Natanson's work (1912b), as well as they could know his earlier articles (1911a and 1911c).

15) Czesław Białobrzeski (1878–1953), a privat-docent in the University of Kiev (1907–1913), a docent in the University of Kiev (1913–1919),[79] a profesor of University of Kraków (1919–1920); professor of theoretical physics in University of Warsaw (1920–1953), in the letter of 1–14 September 1912, sent from Kiev, he thanked Natanson for sending the dissertation "Zasady Teoryi Promieniowania" ("The Principles of Theory of Radiation", published in 1913 – cf. Natanson 1913), which he intended to use at

of professorship at the University of Göttingen in January 1933 (for racial reason, since he was treated as a Jew) and the emigration to England this year, he changed his primary views and became a socialist who was convinced that one had to "fight nationalism in whatever form it appears" (Greenspan 2005, p. 261) "including that of the Jews" (letter from Max Born to Albert Einstein, 22 May 1948 – cf. Born, Max 1936 (*published archival document*)).

[78] "Shortly before the appearance of our communication of April 1912 [Born, von Kármán1912] Mr. Debye has, as he told us afterwards, reported his results in the March meeting of the Swiss Physical Society and published a short note in the *Archives de Genève*, March 1912, p. 256 [Debye 1912]. Further, Mr. Natanson claims – based on a communication to the February session of the Cracow Academy [i.e. Natanson 1912b] – that he first stated the idea which lies at the basis of the treatments of Debye and ourselves. It seems to us that the priority for giving a formulation and an approximate solution of the problem belongs to Mr. Dybye by several days" (Born, von Kármán 1913a, p. 15 fn. 1 in the right column; translated by Mehra, Rechenberg 2001, p. 143).

In fact, Natanson (1912b) presented his work in Cracow Academy on 8 January 1912 (and not in February). Moreover, Born and von Kármán (1913a) accept that Natanson was the first to state the idea which lies at the basis of the treatments of Debye (9 March, 1912), and Born, von Kármán (April 1912). Therefore, the priority of this idea belonged to Natanson. However, in his biography (cf. Born 1978, pp. 141–142), Born neglects this aspect and mentions only Debye's contribution.

Hence, to my current knowledge and in contrast to N. Nagasawa (cf. fn. 37 above), this citation of Born's and von Kármán's (1913a) is the first citation of Natanson's fourth article (i.e. 1912b).

[79] Regarding the scientific degrees in the Tsarist Russia, cf. Flin, Panko 2015.

the University of Kiev in the spring semester of 1913 during the lectures on theoretical physics of "Theory of radiation".[80]

16) Kazimierz Fajans (1887–1975), a physico-chemist (from 1911 an assistant, from 1913 an associate professor at University of Karlsruhe; from 1917 a professor of University of München) knew and appreciated Natanson's monograph on radiation and related matters (1913).[81]
17) Emil Lampe (1840–1918) informed about Natanson's work of 1913 in his note in *Jahrbuch über die Fortschritte der Mathematik* (1914). (However, it was only a bibliographic record of this work and keywords).
18) Stanisław Loria (1883–1955), who was a Ph.D. student of Natanson in 1907, informed about Natanson's article of 1913 in his note in *Jahrbuch über die Fortschritte der Mathematik* (1914). It is quite probable that he knew all the other Natanson's works on the subject under discussion, since he corresponded with Natanson and appreciated him as a very good physicist[82].

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[80] Cf. the letter from Czesław Białobrzeski to Władysław Natanson, Leningrad, 1–14 September 1912 (see Białobrzeski 1912 (*archival document*)). About Białobrzeski's scientific career cf. Ścisłowski 1954; 1979; Wróblewski 2016, pp. 335–339. He is, among other things, a precursor of research on the thermodynamic equilibrium of a star, modeled as a free gas sphere (he did this before Arthur Stanley Eddington's research) – cf. Bialobjeski [Białobrzeski] 1913 (the work was presented by Natanson on 5 May 1913 at the session of the Academy of Arts and Sciences in Kraków).

[81] Cf. the letter from Kazimierz Fajans to Władysław Natanson, Karlsruhe, 4 November 1913 (see Fajans 1913 (*archival document*), folium 3 recto). About Fajans's scientific career, cf. Hurwic 1988; 2000a; 2000b. Fajans was nominated 6 times for a Nobel Prize in Chemistry: in 1928 by Fritz Arndt, Heinrich Biltz, Walter Herz, Ernst Koenigs, J. Meyer, and in 1934 by Mieczyslaw Centnerszwer.

[82] In his recollections from his stay in University of Breslau (1907–1908), Max Born makes the following description of Stanisław Loria and formulates the following opinion on Loria's teacher Ladislas Natanson: "The last in our group, Stanislaus Loria, was a Pole from Cracow, hence at that time a subject of the Austro-Hungarian monarchy. [To this group belonged also Max Born (1882–1970), Rudolf Ladenburg (1882–1952) and Fritz Reiche (1883–1969) – M.K.]. But he was a great Polish patriot and hated the Austrians. [...] He was an enthusiastic physicist and a most charming young man, with fine features and perfect manners. We got on very well. And the only point of friction between us was his exaggerated devotion to his professor, Nathanson [sic! – it should be Natanson], in Cracow, whom he declared to be one of the greatest physicists.

19) Leopold Infeld (1898–1968), who was the Ph.D. student of Natanson in 1921 and corresponded then with his teacher, knew Natanson's works of 1913[83] and 1924b, i.e. 1924a, pp. 125–151, and his other works.[84]

We considered this to be a nationalistic overstatement [sic! – M.K.], and contradicted him; but on the other hand this admiration for his master was rather touching, and we soon ceased our objections" (Born 1978, pp. 124–125).

Unfortunately, Born does not explain why he thought that Loria's admiration for Natanson was "to be a nationalistic overstatement". To date, however, there are no historical sources that would support Born's statement.

As a side note: Born learned from Fritz Reiche and Stanislaus Loria about Einstein's work on special relativity (1905): "[…] When I later (1907–1908) tried to develop my experimental skills at the Institute presided over by Lummer and Pringsheim in my home town of Breslau, I joined an active group of young physicists, including Rudolf Ladenburg, Fritz Reiche and Stanislaus Loria. We studied the more recent physics literature and reported on what we had read. When I mentioned Minkowsky's contributions to the seminars in Gottingen, which already contained the germ of his four-dimension representation of the electromagnetic field, published in 1907–8, Reiche and Loria told me about Einstein's paper and suggested that I should study it. This I did, and was immediately deeply impressed. We were all aware that a genius of the first order had emerged. But nobody knew anything about his personality or his life, except that he was a civil servant at the Swiss Patent Office in Berne. Then Ladenburg decided to look him up during a holiday trip, and his account was the first I heard of Einstein the man. Even then he was as he appeared later: completely unpretentious, simple and modest in his habits, kind and friendly, yet witty and humorous. Ladenburg was enthusiastic and made us curious about the great unknown" (Born, Einstein 1971, p. 1).

There is another description of this fact given by Leopold Infeld: "My friend Professor Loria told me how his teacher, Professor [August] Witkowski (and a very great teacher he was!), read Einstein's paper [1905; on special relativity] and exclaimed to Loria: 'A new Copernicus has been born! Read Einstein's paper'. Later, when Professor Loria met Max Born at a physics meeting [in fact, it was in 1907–1908 at the University of Breslau, Loria was then a doctor], he told him about Einstein and asked Born if he had read the paper. It turned out that neither Born nor anyone else had heard about Einstein. They went to the library, took from the bookshelves the seventeenth volume of *Annalen der Physik* and started to read Einstein's article. Immediately Born recognized its greatness and also the necessity for formal generalizations" (Infeld 1950, p. 44; cited by Wróblewski 2014, p. 262).

[83] Infeld 1958, p. 134.

[84] He wrote a review of the book *Oblicze natury* and mentioned there a paper "On radiation" (=Natanson 1924b). Cf. letter from Leopold Infeld to Władysław Natanson (Warszawa, 9 February 1926) (see Infeld 19126a (*archival document*), folium

20) The offprint of Natanson's article of 1912 (1912b) was in possession of the Department of Theoretical Physics, University of Uppsala and it is stamped: "Mek. Sem. Uppsala".[85]
21) Otto Halpern (1899–1982), an assistant of professor Hans Thirring (1888–1976) of University in Wien, and Albert Einstein (1899–1982) have known about the existence of Natanson's article (1911c), because Halpern informed Einstein about Krutkov's article (1914a) in the letter of 26 August 1924, which criticized a derivation of Planck's radiation formula with Bose's assumption of independent light quanta in the agreement and on the basis of Ehrenfest 1911a and Krutkov 1914a.[86]
22) Witold Jacyna, a Polish physicist from Institute of Metrology, Leningrad, U.S.S.R. and an author of articles in thermodynamics at the *Physikalische Zeitschrift* and the *Physical Review*, knew the second version of Natanson's article "O promieniowaniu" ("On radiation" – Natanson 1924b, i.e. Natanson 1924a, pp. 125–151), however, he was not an advocate of quantum ideas.[87]

4–5; Infeld 1926c, p. 5 (a review). Morever he knew the other Natanson works on statistics of radiation and related issues, because in the letter to Władysław Natanson (from Piaseczno, on 26 July 1926) Infeld mentioned as example of great works Natanson's works about radiation (see Infeld 19126b (*archival document*), folium 8 recto and verso).

[85] Andersson 2018. It is still to buy at the Antikvariat Thomas Andersson at Uppsala, Sweden.

[86] See Halpern 1924a (*published archival document*). Moreover, Albert Einstein, during his stay in Leyden from 4 to 24 October 1924, discussed this objection with Ehrenfest, which is followed by Einstein's statement from his article: "Mr. Ehrenfest and other colleagues have faulted Bose's theory of radiation and my analogous one for ideal gases for not treating quanta, or molecules, as statistically mutually independent structures, without specifically pointing out this circumstance in our paper" (Einstein 1925a (dated: December 1924; presented: 8 January1925; published: 9 February 1925) / (reprinted) 2015a, Doc. 385; (English translation) 2015b, Doc. 385 (cf. also its fn. 6).

[87] Moreover, he thought that all Natanson's monographs known to him (1908; 1924a; 1928; 1929–1930; 1934) were very well written, including especially his monograph entitled *Pierwsze Zasady Mechaniki Undulacyjnej* (*Principes fondamentaux de la Mécanique Ondulatoire*) (1929–1930), and worth to translate them into foreign languages. He even tried himself to make their translations into Russian, but he did not realize this aim finally. He wanted also to bring to translate into Russian Natanson's monograph entitled

23) Leon Lichtenstein (1879–1933), a Polish mathematician, professor of mathematics of Technical University of Charlottenburg, University of Münster, and University of Lipsk, one of the founders and the first editor-in-chief (1918–1933) of the journal *Mathematische Zeitschrift* (founded in 1918), and an editor of *Jahrbuch über die Fortschritte der Mathematik* (1919–1927), appreciated very much Natanson's *Oblicze natury* (1924a; including the second version of Natanson's article "O promieniowaniu" ("On radiation" – Natanson 1924b, i.e. Natanson 1924a, pp. 125–151), and also "Zasady teoryi promieniowania" (1913) (The Principles of Theory of Radiation).[88]
24) Wacław Dziewulski, a physicist, from October 1919 worked in Stefan Batory University in Wilno, knew the second version of Natanson's article "O promieniowaniu" ("On radiation" – Natanson 1924b, i.e. Natanson 1924a, pp. 125–151).[89]
25) Walther Gerlach (1889–1979) and Alfred Landé (1888–1976) not only knew Natanson's article (1911c) – cf. Landé 1925 (letter to Natanson, 18 November 1925), but also cited it in their article (Gerlach, Landé 1926, p. 170, fn. 2; reprinted in Landé 1988, p. 245, fn. 2).[90]

Hence, it is evidenced that many Polish scientists (physicists, chemists, and mathematicians) appreciated Natanson's works on statistical theory of radiation and related matters. They did so, not only in private

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Pierwsze Zasady Mechaniki Undulacyjnej (*Principes fondamentaux de la Mécanique Ondulatoire*; 1929–1930), and "he had a semi-formal pledge as to the favorable resolution of the question, and a very positive response to this request from [I.A.] Krutkov", but "the matter has stopped somehow", cf. the letter from Jacyna to Natanson from Leningrad on 11 January 1936 – see Jacyna 1936 (*archival document*).

[88] Cf. two letters from Lenon Lichtenstein to Władysław Natanson from Lipsk on 29 May 1927, and from Wildungen on 19 August 1928 – see Lichtenstein 1927 (*archival document*); 1928 (*archival document*). On Lichtenstein cf. Przeworska-Rolewicz 1979; Gittel 2014; *Wikipedia* 2019h.

[89] Cf. letter from Wacław Dziewulski to Władysław Natanson from Wilno on 29 December 1923, folium 110, where he thanked for sending him *Oblicze Natury* (1924a).

[90] To my current knowledge and in contrast to N. Nagasawa (cf. fn. 37 above), it was the fifth citation of this article.

letters, but also in reviews of Natanson's works (cf. Loria 1914, Infeld 1926c, p. 5) and in their own scientific articles (cf. Zakrzewski 1911).

From this entire survey (cf. Sections 4.2–4.3) it is also evidenced that Natanson's works on quantum statistics and Natanson himself were known to many scientists of the 1910s, including those who focused their research on the problem of the black-body radiation, the quanta and new (quantum) theories of gases, liquids and solid bodies, especially Zakrzewski (1911) – who was the first to cite the English version of Natanson's article (1911a); Planck (1911; 1912) – who was the first to cite the German version of Natanson's article (1911c); Sommerfeld and Ehrenfest (letter from Ehrenfest to Sommerfled, October 16, 1911) – who knew the German version of Natanson's article (1911c); Born, von Kármán (1913a) – who was the first to mention Natanson's article (1912b); Krutkow (1914a; 1914b) – who knew both English and German versions of Natanson's article (1911a; 1911c); Landé (letter to Natanson, 18 November 1925); and Gerlach, Landé (1926/reprinted 1988) – who cited the German version of Natanson's article (1911c).[91]

In other words, A. Kastler's thesis (cf. Section 4.6) that Natanson's article (1911c) "unfortunately remained unnoticed and unmentioned" (Kastler 1983, p. 617) is wrong since a part of scientists, including eminent and prominent ones, knew this article and valued it highly.[92]

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[91] Hence, the received view that it was only in 1967 that Friedrich Hund as the first scientist appreciated Natanson's works on Planck's theories of radiation and related issues (cf. Section 4.3) is wrong.

[92] Moreover, since Natanson was used to send his publications to many scientists (it is evidenced by his large scientific correspondence), and this happened also in the case of his correspondence concerning the issues of radiation theory and related issues, it is very probable that his works were known to many other scientists. Since in the case of his two articles published in the *Bulletin International de l'Académie des Sciences de Cracovie, Classe des Sciences mathématiques et naturelles. Série A: Sciences mathématiques / Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften* he had on disposal even 100 copies in each case. This number of copies was mentioned by a Polish physicist, professor of electrotechnics and mechanics in the Institute of Engineers of Communication in St Petersburg, and a historian Henryk Merczyng (1860–1916) in his letter from St. Petersburg on 21 August 1912 to Władysław Natanson – see Merczyng 1912 (*archival document*). On H. Merczyng's scientific biography, cf. Zuzga 1987.

On the other hand, F. Hund's thesis (cf. Section 4.6), according to which "in 1924 Natanson's arguments had been already forgotten" (Hund 1967, p. 123), is right in the case of Bose and Einstein, since it is true that in 1924–1925 they overlooked subtleties of the issue of indistinguishability of physical objects known to Natanson. However, F. Hund's thesis is wrong in the case of Paul Ehrenfest and their colleagues, including Viktor R. Bursian, Yuri A. Krutkow and Otto Halpern, who did not forget Natanson's considerations, and caused that Einstein was informed on these subtleties of the issue of indistinguishability of physical objects (this issue will be explained in Section 7).

5. Why Natanson's achievements were unnoticed and forgotten by eminent and prominent scientists? The external factors in the reception of the publications[93]

Four explanations of this fact have been proposed:

A "geographical isolation" explanation by L. Navarro, E. Pérez (2004)

> His [Ehrenfest] geographical isolation thus may have contributed to the neglect of his 1911 paper even though it was published in the widely read *Annalen der Physik*. The work of Ioffé, Natanson, and Ishiwara suffered a similar fate. We are not in position, however, to go into this question further in this time (Navarro, Pérez 2004, p. 137).

This interpretation is defective because "the geographical isolation" does not explain the lack of reception of the publication, which appeared in the leading physical journal in a country (Germany) that at the time "was not geographically isolated".

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[93] The basis of this section is A. Kestler's thesis, that Natanson's article (1911c) "unfortunately remained unnoticed and unmentioned" (Kastler 1983, p. 617), which was accepted by some interpreters of Natanson's achievements. Though the previous section showed that this thesis is wrong, in this section the views of several interpreters of Natanson's achievements who accepted it and explained it in different manners are analysed. The section focuses on the lacks of these explanations.

However, this kind of isolation of German science started only with WWI[94] and then the foundation of the International Research Council in 1919.[95] Therefore, L. Navarro's and E, Pérez's term: "geographical isolation" conceals the real lack of openness to the achievements of scientists from such territories as Russia (Ehrenfest and Ioffé worked at the Saint Petersburg Academy), the Austrian governed part of Poland (Natanson from Kraków) and Japan (Ishiwara from Tokyo).[96]

A psychological and sociological explanation by J. Spałek (2006)

> Natanson visited Einstein in Berlin 1914 [sic! – M.K.][97] and probably did not mention his own work, as he was a shy person [sic! – M.K.] (Spałek 2006).
>
> He worked in an isolated, local scientific environment and published his works in little known journals [sic! – M.K.] (Spałek 2009).

However, having an in-depth knowledge of the biography of Władysław Natanson, it is certain that he was not a shy person!

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[94] During the World War I (from 1914 to 1918) many German scientists took active part in the so-called "Der Krieg der Geister" ("The War with spiritual weapons"). They created, among others, two important documents: the *Aufruf 'An die Kulturwelt!* ("A Call to the Civilized World") of 4 October 1914, signed by ninety-three German scientists (among others, Walther H. Nernst, Wilhelm Röntgen, Wilhelm Wien, Max Planck, and Arnold Sommerfeld), and the *Erklärung der Hochschullehrer* des Deutschen Reiches (Declaration of University Teachers of the German Empire) of 16 October 1914, signed by over four thousands teachers. It caused an isolation of German scientists from the international community. Cf. Wolff 2003 (English)/2007 (German); Somsen 2008; *Wikipedia* 2009; Kleinert 2010; von Ungern-Sternberg 2014.

[95] I will refer to this issue below in the context of Natanson himself.

[96] Today's equivalent of this problem is the overrepresentation of English-language journals in Scopus and WoS, and the widespread habit of not quoting publications from "geographically isolated countries". Of course, nowadays, in the age of the Internet, there are no geographical barriers that will limit the dissemination of scientific knowledge. The only barriers are mental barriers existing in human minds. They create scientific ghettos and world temples of knowledge.

[97] Natanson with his family went for a summer holiday 1914 to Belgium (Westende, near Ostende). He planned to stay there from August to the first half of September.

Moreover, his works could be understood by a broad international community of physicists, because he published his major works in English, French and German (and also in Polish). He was known well by this international community, because he was a member of the Physial Society of London (from 1886), a member of the Deutscher Physikalischen Gesellschaft (from 1913)[98], the Council of the Société Française de Physique (from 1914), the Polish delegate to the General Assembly of the International Research Council (from 1919), a co-founder of the International Union of Pure and Applied Physics (1922) as a representative of Poland and Vicepresident of this union (1925–1931). He corresponded with many leading scholars of his times such as Max Planck, Albert Einstein, Hendrik Antoon Lorentz, Arnold Sommerfeld, Maria Skłodowska-Curie, etc.[99]

The outbreak of WWI forced him to stay in this country, and then from 24 November 1914 until the first days of September (at least 3rd September) 1915 – they spent in Berlin (letters on 29 June 1914, 17 November 1914, and 11 September 1915 from Władysław Natanson to Marian Smoluchowski – see Natanson 1914a (*archival document*), folium 151 verso; 1914b (*archival document*), folium 153 recto; 1915 (*archival document*), folium 176 verso).

During this stay Einstein met Natanson personally (and his family) and became friends (cf. Średniawa 1996, pp. 76–77; 2006, p. 260). Their first meeting was at the beginning of January 1915. Einstein informed his friend Heinrich Zangger about this event on 11 January 1915: "In recent days I made the acquaintance of our colleague Natanson from Cracow, a fine theoretical mind. […] He is a Polish Jew and grew up in Russia, now 50 years of age. I quickly took a liking to him as I rarely do with people; blood runs thicker than water!" (Einstein 1998a, Doc. 45a, p. 28 / English translation: Einstein 1998b, Doc. 45a, pp. 15–16; see also Konieczny 2011, p. 255).

The last meetings were before 4 September 1915. Natanson informed his friend Smoluchowski about these events: "In Berlin, before leaving, [I had] yet a few more nice and beautiful conversations with prof. Einstein. He promised to come to us to Krakow" („W Berlinie z prof. Einsteinem jeszcze kilka nader miłych i ślicznych rozmów, przed wyjazdem. Obiecał, że przyjedzie do nas do Krakowa" (Letter on 11 September 1915 from Władysław Natanson to Marian Smoluchowski – see Natanson 1915 (*archival document*), folium 177 verso).

[98] Cf. Planck 1913 (*archival document*) – see below, Appendix 3 for its transcription and translation into English.

[99] Cf. Natanson 1933/1958; Biblioteka Jagiellońska, Natanson's correspondence; Konieczny 2011, p. 255.

For example: he was well known by Hendrik Antoon Lorentz, who – in the letter of 20 August 1914 to Pieter Zeeman – wrote:

> From foreign friends and acquitainces I hear hardly anything. Incidently I heard that Stark was inducted in the Feldsturm, and that Natanson is in Westende near Ostende with wife and children and does not know how and when he will be coming home again (Lorentz 1914 (*published archival document*), in: Lorentz 2018, Doc. 160, p. 416; English translation: p. 417).

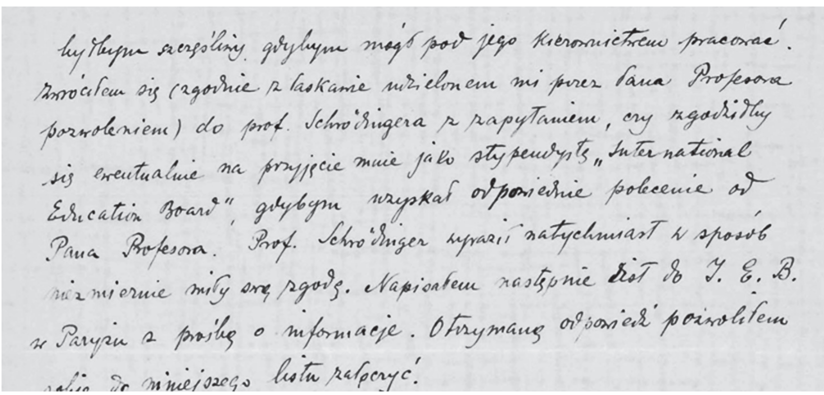
byłbym szczęśliwy, gdybym mógł pod jego kierownictwem pracować.
Zwróciłem się (zgodnie z łaskawie udzielonem mi przez Pana Profesora
pozwoleniem) do prof. Schrödingera z zapytaniem, czy zgodziłby
się ewentualnie na przyjęcie mnie jako stypendysty „International
Education Board", gdybym uzyskał odpowiednie polecenie od
Pana Profesora. Prof. Schrödinger wyraził natychmiast w sposób
niezmiernie miły swą zgodę. Napisałem następnie list do I. E. B.
w Paryżu z prośbą o informacje. Otrzymaną odpowiedź pozwoliłem
sobie do niniejszego listu załączyć.

Fig. 15. The excerpt of Infeld's letter sent to Natanson from Warszawa, dated 5 October 1928. Source: Infeld 1928 (*archival document*), folium 11 verso. Photo: © Michał Kokowski.

Moreover, he was appreciated by Erwin Schrödinger as evidenced by letter from Leopold Infeld to Władysław Natanson (Warszawa, 5 October 1928):

> Zwróciłem się (zgodnie z łaskawie udzielonem mi przez Pana Profesora pozwoleniem) do profesora Schrödingera z zapytaniem, czy zgodziłby się ewentualnie na przyjęcie mnie jako stypendysty „International Education Board", gdybym uzyskał odpowiednie polecenie od Pana Profesora. Prof. Schrödinger wyraził natychmiast w sposób niezmiernie miły swą zgodę (Infeld 1928 (*archival document*), folium 11 verso).
>
> I have turned to Professor Schrödinger (according to the permission granted me by you, dear Professor) asking

> if he would agree to take me as a holder of the "International Education Board" scholarship if I received the appropriate instruction from you, dear Professor. Professor Schrödinger was exceptionally kind and immediately gave his me consent [translation – M.K.].

Hence, there is no doubt that though Natanson lived in Poland and was active in a little Polish local scientific environment, he was well known in the international community of physicists.[100]

Moreover, it appears that there is another explanation of the enigmatic behavior of Natanson regarding Einstein. Some light is shed on this by a similar event that took place in 1931, fourteen years after the death of Marian Smoluchowski. In the letter of 10 July 1931 to Tadeusz Smoluchowski (the brother of Marian Smoluchowski), Natanson commented on E. Arendt's remarks (1931) on Natanson's work (1888a/1888b) related to the later analysis of the problem of Brownian motions by Smoluchowski in 1900–1910s and stated:

> [Marian Smoluchowski] did not know about this work; I wanted to tell him about it a few times, but I always postponed it in order not to allow him to think that I wanted to revindicate a part of his discoveries for myself. I had so much devotion and attachment for him, the most cordial, almost fraternal, that I did not want to put a question *de priorité* between us [...] [translation – M.K.].[101]

We will understand these wordings even better when we add a statement here from a letter of 15 September 1933 from Arkadiusz Piekara (then in Paris) to Władysław Natanson:

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[100] I develop in this paragraph the thought firstly expressed by Matthew Konieczny (2012, p. 74/75). In 2010/2011, I was his scientific supervisor. He stayed then in Kraków as a scholarship holder of the Fulbright Foundation (he got the Fulbright Junior Research Award).

[101] "[Marian] nie wiedział o tej pracy; parę razy chciałem Mu o niej powiedzieć, ale odkładałem zawsze, żeby nie nasuwać Mu myśli, jakobym chciał cząstkę Jego odkryć rewindykować dla siebie. Miałem dla Niego tyle czci i przywiązania tyle, najgorętszego, prawie że braterskiego, iż nie chciałem kwestyj *de priorité* pomiędzy nami […]" (cited by Teske 1970a, p. 143).

> I am reminded of the words of the Venerable Rector [i.e. Natanson], spoken a year ago in Warsaw. "Science is not our life's goal, it is only its color" [translation – M.K.].[102]

Hence, Władysław Natanson was not a shy person, but a very friendly and polite one, and physics was not so important to him to risk weakening his friendship with Marian Smoluchowski.

Therefore, perhaps, it was for the same reason that Natanson did not mention his works of 1911–1913 during his talks with Einstein in Berlin in 1915. Nevertheless, from my own perspective – I agree in this point with Józef Spałek – such interpretation seems unlikely, since in 1915 these subjects were still very interesting and "hot" to discuss with Albert Einstein. However, this "unlikeliness" is only my guess not based on any historical source, so it can be wrong.

Moreover it is not true that Natanson worked in an *isolated*, local scientific environment and published his works in little known journals. First of all, this local scientific environment was not isolated from international science as evidenced by the same level of scientific works of, for instance, Smoluchowski, Olszewski and Natanson. Then, Natanson's articles appeared, among others, in *Wiedemann's Annales* (1885/1886), *Zeitschrift für Physikalische Chemie* (1892; 1894; 1895; 1896; 1902; 1903), *Comptes Rendus de l'Académie Française des Sciences* (1893), *Philosophical Magazine* (1895; 1901; 1919, 1933), *Annalen der Naturphilosophie* (1901), *Journal de Physique Théorique et Appliquée* (1903), *Journal of Physical Chemistry* (1903), *Journal de Physique* (1909), *Physikalische Zeitschrift* (1911c), and also *Bulletin International de l'Académie des Sciences de Cracovie, Classe des Sciences mathématiques et naturelles. Série A: Sciences mathématiques / Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften* (1893; 1895; 1897; 1898; 1899; 1901; 1902; 1911a; 1912b).[103] The latter journal published works in German, French, English or Polish (so it promoted international communication in then science), and these works were mentioned and reviewed by the *Science Abstracts* and the *Jahrbuch über*

[102] "Przypominają mi się słowa Czcig. Pana Rektora, wypowiedziane przed rokiem w Warszawie. Nauka nie jest celem życia, jest tylko jej barwą" (Piekara, Arkadiusz 1933 (*archival document*), folium 257 verso).

[103] Cf. the list of Natanson's publications Natanson 1879–1937 (pp. 300–307).

die Fortschritte der Mathematik (contemporary analogies of present-day Scopus and Clarivative databases).

Hence, it is not legitimate to claim that non-Polish journals mentioned above – including e.g. the *Physikalische Zeitschrift*, a leading journal on an international scale – or the *Bulletin International de l'Académie des Sciences de Cracovie… Série A…* were isolated in any sense from international audience. Since in the case of the reception of Natanson's scientific publications, neither the language nor the place where he published his works were barriers, they had to be mental and political barriers existing in the minds of scientists.

An epistemological explanation by Konieczny (2010)

> Despite advanced work on the very issues at stake at Solvay in 1911, and extensive contact with Western European institutions and practitioners of physics, why were no scientists working at universities in the peripheries of East-Central European empires invited? Ultimately, I argue that their exclusion was not based on an ignorance of their work or a prejudice against scientific workers on the periphery per se, but rather that Eastern European physicists approached the issues at stake at Solvay from fundamentally different epistemological and ontological dispositions, which rendered their work beyond the bounds of the dominant scientific discourse and thus intellectually incompatible (Konieczny 2010).

I think that this thesis is too bold. It is better to say in this context that the nationalist ideology that spread in 1910s Europe made a free flow of information and a free reception of ideas very difficult or even impossible. Natanson was not a nationalist but a Renaissance person. He was not only a physicist, but also a historian and a philosopher of science, and a talented humanist, a philosophical literary critic, the author of literary portraits of world-famous writers (Shakespeare, Shelley, Dostoyevsky, …), a biographer of outstanding physicists (Newton, Faraday, Maxwell, Smoluchowski, …),[104] who received the gold medal of

[104] Cf. Natanson 1908; 1924a; 1928; 1934; 1937; 1977.

"Wawrzyn Akademicki" (Academic Laurel) of the Polish Academy of Literature in 1936 "for outstanding critical-literary, scientific and journalistic work in the field of belles-lettres".[105] And being such a creative man, he did not fit the nationalistic *Weltanschauung* or *Zeitgeist* that dominated the Europe of these times.

A political explanation by M.J. Konieczny (2011; 2012)

The so-called "Western science" at the turn of the 19th and 20th centuries was limited by the nationalistic approach, especially with regard to French and German science.[106] In contrast, Władysław Natanson was a true European scientist, that is, being free of nationalism, he appreciated the value of works of authors irrespective of their nation or state of origin; his works on quantum statistics were not accepted, since he did not belong to any "scientific party".

I agree with this interpretation partially: it is true that all his education and scientific activities, including publications, prove that he drew on the achievements of various linguistic cultures (especially British, French, German, and American) and he did not belong to any "scientific party" nor to any "nationalist scientific camp" (British, French, German or American).[107] In support of this thesis, I will quote Natanson's autobiographical statement of 1933:

> Throughout my life, I have tried with all my strength to learn from the most esteemed masters, even if they had not been alive for a long time. How much have I learned from Newton, from Lagrange, Kelvin, Clausius, from J.W. Gibbs, from G.G. Stokes, from Lord Rayleigh, P. Duhem, and H.A. Lorentz. My first beloved role-model and commander has always been J. Clerk-Maxwell. Communing with the works of genius creators leaves in the mind

[105] Cf. "Eine" 2007; Kokowski 2009; *Wikipedia* 2019g, and first of all his brilliant essays written in Polish – Natanson 1924; 1928; 1934; 1937.

[106] It is well known problem – cf. also Kleinert 1978; Wolff, Stefan L. 2003 (English)/2007 (German); Somsen 2008; Eckert 2013a/2013b; Gordin 2015; Fox 2016.

[107] The existence of "national scientific camps" in science causes the existence of serious incoherency between different national histories of science. Cf. Wróblewski (2006) regarding incoherency of national histories of science, and Kokowski (1993; 1994; 1997; 2009; 2010) regarding Natanson's views.

> and soul traces and effects which, in my opinion, no lectures – be it seminars or foreign ones – can bring about [translation – M.K.][108].

On the other hand, I think that M.J. Konieczny did not provide solid empirical evidence – i.e. explicit statements in archival or printed correspondence or articles and books of the scientists of his epoch – that Natanson's "works on quantum statistics were not accepted, since he did not belong to any «scientific party»" (but see below).

A political explanations by N. Nagasawa ("Minamida" 2009; Nagasawa 2018)

According to N. Nagasawa ("Minamida" 2009; Nagasawa 2018, pp. 404–407), one of the reasons, why Natanson's paper was forgotten in 1924 (when Bose's article appeared) within the physics community was a nationalist climate guarding the European physics community, and especially the German one (he referred in this point to S.L. Wolff's work (2003)).

An expression of this attitude was to be Arnold Sommerfeld's letter dated 1 November, 1919 to Adalbert (Wojciech) Rubinowicz (a Polish scientist), a former assistant of Sommerfeld. Rubinowicz was asking for Sommerfeld's help to look for the academic position. Sommerfeld advised him:

> It would be very difficult to find positions in Poland. Natanson is not reliable because his activity is limited within his domestic community [sic! – M.K.]. You should contact M. [Maria – M.K.] Curie. She is not poisoned by chauvinism (Nagasawa 2018, p. 407; quoted also in "Minamida" 2009).

However, still according to N. Nagasawa, being familiar with the biography of Władysław Natanson (cf. Klecki 1938), we should not think that he was a chauvinist (Nagasawa 2018, p. 406). Therefore, Sommerfeld

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108 "Przez całe życie starałem się ze wszystkich sił uczyć od najwyższych Mistrzów, chociażby już Ich dawno nie było na ziemi. Ileż nauczyłem się od Newtona, od Lagrange'a, Kelwina, Clausiusa, od J.W. Gibbsa, od G.G. Stokesa, od Lorda Rayleigh, P. Duhema, H.A. Lorentza. Najpierwszym, umiłowanym wzorem i wodzem był zawsze J. Clerk-Maxwell. Obcowanie z dziełami genialnych twórców pozostawia w umyśle i w duszy ślady i skutki, których mym zdaniem, żadne wykłady – seminaryjne, zagraniczne – wydać nie mogą" (Natanson 1933/1958, p. 115).

– who was immersed in the toxic climate of European chauvinisms (German, English, French, etc.) and had a nationalist bias against Natanson – was probably not eager to inform Bose or Einstein about Natanson's work of 1911 (Nagasawa 2018, p. 407).

I have the following comments to N. Nagasawa's interpretation.

Firstly, if the sentences cited above by Nagasawa are a faithful translation of Sommerfeld's sentences, the conjunction of these sentences allows two interpretations: the first – weaker – that Natanson is not reliable or even the second – stronger – that he was not reliable since he was a chauvinist.[109]

Secondly, I can indicate solid arguments in favour of Nagasawa's guess that Natanson was not a chauvinist (see below).

And finally, I do not agree that Arnold Sommerfeld wrote the quoted above statements. These are only Nagasawa's guesses of the real essence of these statements, expressed in two letters from Sommerfeld to Rubinowicz (Stockholm, 1 October 1919; München, 1 November 1919), which is not easy to determine (it is caused by the very style of Sommerfeld's handwriting). Upon reading these two Sommerfeld's letters (see Appendix 4), on the one hand, it is not clear if Sommerfeld thought that Natanson was a chauvinist, and, on the other hand, it is clear that though Sommerfeld did not know "how strong the chauvinism of the Poles was", nevertheless, in his own opinion, perhaps the Poles, like the French, "were crazy" (in their chauvinism), and perhaps Marie Curie's stance was not entirely chauvinistic.[110]

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[109] Regarding the latter interpretation: It follows from the second quoted sentence that Natanson was not reliable since his activities were limited only to the domestic Polish community; and the second and the fourth sentences – that in contrast to Natanson – M. [Marie] Curie (i.e. Maria Skłodowska-Curie) was not a chauvinist. Hence, Natanson was a chauvinist.

[110] As it is well known from the biography of Arnold Sommerfeld, he had rasist or nationalistic, or chauvinistic episodes in his life. E.g., in 1907 in a private letter to Einstein, Sommerfeld stated that perhaps in the theory of relativity "the abstract-conceptual nature of the Semite" was expressed (however, he accepted later the theory of relativity and was not a supporter of the *Deutsche Physik* or *Arische Physik*) – cf. Kleinert 1985, p. 1985; Jansen 2009, p. 148. Then, in 1914 he was one of ninety-three signatories of the *Aufruf 'An die Kulturwelt!* ("A Call to the Civilized World") of 4 October 1914 (but he wanted this document to be known only to the Germans) – see Wolff 2003 (English) / 2007 (German). Moreover, when in 1917 he tried to get a job at the University of Vienna, two scholars were selected to take up the professorship

However, irrespective of these Sommerfeld's views, it is certain that Natanson cannot be characterized in this way. This is proved by his entire activities in the Polish and international scientific communities,[111] by his life-long intellectual fascinations with the Jewish, British, Russian, French, Italian, old Islamic (Sufism) and Polish cultures, by his voluminous correspondence, both scientific (with many famous scientists) and private,[112] and finally by the recollections of his friends, participants of the famous intellectual inter- and multi-disciplinary "Symposium" (from Greek: συμπόσιον, symposion) organized by Natanson in his home for many years.[113] To support these statements, it is worth mentioning Natanson's own words from only three documents of 1918, 1919 and 1933.

The first document is a draft of Natanson's talk (in English) of 1918 from his welcome of the past president of USA Thomas Woodrow Wilson, for the award ceremony of a doctor honoris causa of philosophy in the Jagiellonian University.

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post: Sommerfeld himself and Smoluchowski. On 14 March, 1917, a mathematician Wilhelm Wirtinger, the then dean of the Philosophy Department and chairman of the Commission, and his colleague from Felix Klein seminary at Göttingen, tried to make Sommerfeld the only candidate for this position: "Wirtinger had entered a petition «to put you [Sommerfeld] unico loco on the list; Smoluchowski on the other hand, not at all.» The majority of the committee had accepted this petition, although in a minority vote the physicists had insisted on Smoluchowski. Just why Wirtinger, along with the majority of his colleagues, was against Smoluchowski, he explained by writing, «that Sm. [Smoluchowski] is a Pole, and declares himself as such. I can imagine that in the German Reich there is no true appreciation of what this means to us [in the Austro-Hungarian Empire]. For you can always count on your government's being German, whereas for us «Germanness» constitutes a chip the government bargains with in various difficult circumstances to accommodate the other nationalities, so that we have to protect it ourselves.»" (Eckert 2013b, pp. 215–216).

111 He was Chairman of the Faculty of Mathematics and Natural Sciences at the Polish Academy of Arts and Sciences, the first President of the Polish Physical Society (1920–1923), Rector of the Jagiellonian University in 1922/1923; a co-founder of the International Union of Pure and Applied Physics in 1922 (as a representative of Poland) and its Vice-president (1925–1931), etc. – regarding the latter issue, see Ossipyan, Yamaguchi 1992.

112 Cf. Natanson's correspondence at the Library of the Jagiellonian University, and at the Archive of the Jagiellonian University.

113 Cf. e.g. Michalski 1937a (pp. 308–316; in French); 1937b (in Polish); Klecki 1938, p. 32; Kokowski 2009.

> I am very glad that it is my duty to welcome you, Mr President, in the name of the our old university. It has often been said that Science is of no nationality; Science indeed is international and gives light to Humanity. Let us remember, however, that Science is also the highest product of national character and national virtue. Let us remember, that Science paves the way for a better understanding of a men and nations. […] What is now spent on armament will in the future be devoted to the advancement of Knowledge; nations will not seek supremacy over nations, they will seek supremacy over Nature (Draft of speech 1918, Archives of the Jagiellonian University, Spuścizna 10/24).[114]

We can label Natanson's political stance regarding science as *Olympic internationalism* (in Geert J. Somsen's terminology).[115] Hence, Natanson was not a chauvinist. This is confirmed additionally by the *Interim Report on the course of the Constitutional Assembly of the International Research Council held at Brussels, from 18 to 28 July 1919* (*Sprawozdanie tymczasowe z przebiegu Zgromadzenia Konstytucyjnego Rady Międzynarodowej Badań Naukowych odbytego w Brukselli, w dniach od 18. do 28. lipca 1919 roku*) that Natanson (1919) prepared as the only Polish representative at this meeting.[116]

> To a peaceful and unbiased observer it is clear that as a result of the war the scientific world will remain for a long time divided into two camps, separated from each other and mutually averse. Whether we approve of it, or whether we condemn it, whether we consider it justified or

114 Cited by Konieczny 2008, pp. 127–128, who thought mistakenly that it was a welcome in 1924 of scientists from Europe and USA.

115 Regarding the meaning of this term see Somsen 2008, pp. 365–367.

116 To understand well Natanson's comments on Constitutional Assembly of the International Research Council one should add that the aim of the foundation of this institution was the reconstruction of international scientific arrangements after the WWI, but without Germany. During the meeting in Brussels, the representatives of western countries (including President of the Assembly, Professor Alfred Lacroix (France), Minister Alphonse Harmignie (Belgium), and Professor Auguste Gravis, Director of the Class of Sciences of the Belgian Academy) spoke openly for the boycott of German science. This was accompanied by overt hostility towards the domination of the German language in science and the influence of German researchers on the development of science (cf. also Onghena 2011, pp. 283–284; Cock 1983).

> incomprehensible, we are dealing here with a mighty and massive phenomenon that we will have to reckon with for years to come. We are connected with the West with our strivings and feelings, our history and culture, our views and needs; we can no longer choose a camp where we want to be. But if we should belong to the Western civilization, then the consequences resulting from it are serious and impose great obligations on us. From among the comments that arise here, I will cite only some. […] The tendency of the Assembly, many times expressed in public, was "to be separated from the Germans; in all areas of Knowledge, do without the Germans."[117]

Referring to this, Natanson pointed out:

> Such resolutions, feelings and intentions are not isolated. I inform about them to Akademia [Academy of Arts and Sciences in Kraków], but I do not judge them. I do not make a judgment here, I quote the facts.[118]

Continuing this thought, Natanson noted that if the authorities of the (Polish) Academy of Arts and Sciences in Kraków and the Polish government adopted such a policy, the number of publications in English and French should be increased, because there is a demand for it in countries belonging to the International Research Council, and this also

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[117] "Dla spokojnego i nieuprzedzonego obserwatora jest rzeczą widoczną, że świat naukowy, w następstwie wojny, będzie przez długi czas podzielony na dwa obozy, odgrodzone od siebie i niechętne sobie. Czy je pochwalamy, czy też ganimy, czy uważamy je za uzasadnione lub niezrozumiałe, mamy tu do czynienia z potężnem, z tłumnem zjawiskiem, z którym przez lata będziemy musieli się liczyć. Jesteśmy związani z Zachodem dążeniami i uczuciami, historją i kulturą, poglądami i potrzebami; nie możemy już dzisiaj wybierać obozu, w którym pragniemy się znaleźć. Lecz skoro powinniśmy należeć do cywilizacji zachodniej, przeto następstwa stąd wynikające są poważne i nakładają na nas wielkie zobowiązania. Z pomiędzy nasuwających się tu uwag przytoczę tylko niektóre […]. Tendencją Zgromadzenia, wielokroć razy wyrażoną publicznie, było «odgrodzić się od Niemców; we wszystkich zakresach Wiedzy obejść się bez Niemców»" (Natanson 1919, p. 26).

[118] "Uchwały, uczucia i zamiary podobne nie są odosobnione. Donoszę o nich Akademji, lecz ich nie oceniam. Nie wypowiadam tutaj sądu, przytaczam fakta" (Natanson 1919, p. 27).

should be applied to the journal *Bulletin International*, issued by the Academy of Arts and Sciences, in which the German language prevails.[119]

Consequently, Natanson's statements above prove again that he was not a chauvinist. This is confirmed also by his autobiography of 1933:

> I have never belonged to political parties; I do not belong to any party today either. I was averse and reluctant to political fights. I always wanted to do something, create as much as I could, leave something after myself; fights, polemics disgusted me. In my opinion, what is weak and poorwill fall, perish, vanish. Creativity is the best criticism there is.[120]

Finally, it is worth recalling here that during Natanson's stay in Berlin in 1915, Albert Einstein – who for his pacifistic convictions was boycotted during WWI by the nationalistically oriented scientific community of German scientists[121] – found in Władysław Natanson a non-nationalistic, non-chauvinistic soulmate.[122]

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[119] Natanson 1919, pp. 27–28. It should be noted that the full name of this journal was: *Bulletin International de l'Académie des Sciences de Cracovie, Classe des Sciences mathématiques et naturelles. Série A: Sciences mathématiques / Anzeiger der Akademie der Wissenschaften in Krakau. Mathematisch-Naturwissenschaftliche Klasse. Reihe A: Mathematische Wissenschaften*. Hence, the journal of the Academy of Arts and Sciences in Kraków (after 1919, the Polish Academy of Arts and Sciences) was open to two then antagonistic camps of French science and German one.

[120] "Do stronnictw politycznych nigdy nie należałem; nie należę też dziś do żadnego stronnictwa. Walkom politycznym czułem się obcy, niechętny. Pragnąłem zawsze coś czynić, w miarę sił tworzyć, po sobie zostawić; walki, polemiki, budziły we mnie niesmak. Co jest słabe i liche, moim zdaniem, samo upada, zginie, przepadnie. Najlepszą krytyką jest twórczość" (Natanson 1933/1958, p. 119).

[121] "Einstein was one of only four signatories of the pacifist declaration «Manifesto to the Europeans» ("Aufruf an die Europäer"), written by Georg F. Nicolai (1874–1964), [e]xtraordinary [p]rofessor of [m]edicine and [p]hysiology at the University of Berlin, in response to the manifesto «To the Civilized World» ("An die Kulturwelt")" (Einstein 1915a (*published archival document*). [In:] Einstein 1998a, Doc. 45a, p. 29, fn. 7. Cf. also Wolff 2003, pp. 343–344 (English) / 2007, p. 46 (German).

[122] Cf. the following excerpts of two letters of 29 December 1915 and 14 September 1917 from Albert Einstein to Władysław Natanson.

"Solange Sie da waren, sind Sie mir der liebste Berliner gewesen; der gemütliche Verkehr mit Ihnen geht mir jetzt sehr ab." ("As long as you were here, you had been my favorite Berliner; now I miss our relaxed relations very much) (Einstein 1998a, Doc. 175, p. 231; English translation: Einstein 1998b, Doc. 175, p. 169.

Considering the above comments I agree with N. Nagasawa, that it cannot be ruled out that Arnold Sommerfeld was not eager to promote Natanson's work in 1924 for political prejudices.

6. Who discovered Bose statistics and Bose-Einstein statistics?

As it is already stated in Section 3.4: a) F. Hund (1967) formulated thesis that *Natanson was the first who formulated Bose statistics of "light quanta"*, b) this thesis was accepted by some later commentators of Natanson's works (A. Hermann, A. Kastler, B. Średniawa), A. Bach, Boya, Hentschel, S. Varró, B. Lange, and "Roh Minamida" / N. Nagasawa, c) it is B. Lange's thesis that we should talk about Natanson's statistics (1997a) or, according to P. Mittelstaedt (2013), about "Natanson statistics", d) it is J. Spałek's thesis that we should talk about "Natanson-Bose-Einstein statistics" (2005), and e) it is M. Waniek's and K. Hentschel's thesis that we should talk about "Planck-Natanson-Bose-Einstein statistics" (2011).

Contrary to the above-mentioned scholars I think that – without any exaggeration – W. Natanson or M. Planck may be considered a precursor[123] of Bose statistics and Bose-Einstein statistics, but the idea that each of them was the author or a co-author of these statistics is too far-reaching. Regarding W. Natanson, I share the opinion of such researchers as L. Infeld, and S. Bergia with regards to this issue.

The former (Infeld 1958, p. 136; 1964b, pp. 35–36) noted about Natanson that:

"Man fühlt sich immer fremder in dieser harten Welt. Aber man freut sich der wohlwollenden Gesinnungs brüder, wenn sie auch ferne in Krakau sitzen! Hoffentlich führt uns das Schicksal bald einmal wieder zusammen." ("One feels increasingly alienated at this hard world. But we appreciate our good-willed brothers of like mind, even if they are located far away in Cracow! I hope fate soon brings us together once again") (Einstein 1998a, Doc. 380, p. 514; English translation: Einstein 1998b, Doc. 380, p. 373).

Five letters from Einstein are preserved in the correspondence of Natanson kept in the Jagiellonian Library. Regarding these letters cf. Średniawa 1996, pp. 76–77; 2006, p. 260.

[123] „Precursors serve a social function in the construction of the collective memory of a community by creating links that provide continuity in time and between successive generations of scientists" (Gingras 2007, p. 371). Cf. also entire section entitled "The Social Function of 'Precursors' in Collective Memory" (pp. 371–372).

> he was close, remarkably close to the great scientific discoveries, such as the formulation of Bose statistics.

And according to the latter (Bergia 1987, p. 234; reprinted in: Bergia 2009, p. 344):

> It must be stressed that Natanson does not suggest a physical interpretation in terms of objects: photons did not exist in 1911.[124] Still, he is certainly to be considered as a forerunner of Bose statistics.

On the other hand, I cannot agree with the bold thesis stated by A. Bach (1988; 1990; 2008):

> That what we now call Bose-Einstein statistics actually had been introduced by Boltzmann in 1877 in the context of establishing the entropy–probability relationship (Bach 1990, p. 2).
>
> BE [Bose-Einstein] statistics was introduced by Boltzmann in the period 1868–1877 as a discrete scheme to derive the exponential distribution (Boltzmann distribution) and to establish the entropy–probability relationship (Bach 2008, p. 3).

The reason is quite simple, since L. Boltzmann and later M. Planck and W. Natanson were not authors of the quantum density of states discovered by Bose in 1924,[125] and Boltzmann's combinatoric approach is based on an imaginary fiction not realized in the real world of mechanical objects.[126]

However, I do agree with the advocates of the second and third theses mentioned in Section 3.5 that Natanson was the first to understand the statistical foundations of Planck's law of black-body radiation, and that Natanson and Ehrenfest were the first to understand the concept identicality of physical objects.

[124] There existed, of course, since Einstein's paper of 1905, the light quantum, but most physicists (see, for instance, Klein 1970a) did not give much credit to the idea.

[125] However before S.N. Bose's article (1924a), several authors already published articles contributed to explain the issue of the phase space elementary cell (h^3). M. Planck was also among them – cf. Ishiwara (1911), Sackur (1911; 1912a; 1912b); Tetrode (1912a; 1912b), Planck (1916), L. de Broglie (1923; 1924). Moreover see: Quarati, Lissia 2013; Abiko 2015; Ebeling; Pöschel 2019, p. 7.

[126] Cf. fn. 26.

Taking all these into account, I can now answer the question: "Who discovered the so-called Bose-Einstein statistics?"

From the perspective determined by using of the integrated historiographic approach described in the introduction (cf. Section 1), there is no simple answer to such a simple-sounding but very difficult question. The final answer depends on the specific context in which we should formulate it. If we are not interested in the history of physics, our answer is quite simple and clear – they did it, Bose and Einstein in 1924–1925, and nobody else. However, if we take history seriously, our answer should be different. I outline its idea below.

L. Boltzmann formulated "Bose-Einstein statistics" in 1868–1877 (see Bach 2008, p. 3), but it was based on a mathematical trick of *"quant" of speed* or vis viva (twice kinetic energy) without a physical sense. However, this statistics acquired physical meaning only in a limit case, when the number of quanta of vis viva tends to infinity, and the magnitude of vis viva tends to zero.[127] This limit case is named the Maxwell-Boltzmann distribution, because Maxwell found it before Boltzmann in an independent way. Then the Wien-Jeans law of radiation and Planck's law of black-body radiation were discovered. Planck gave a theoretical explanation of black-body radiation. However, his explanation was not perfect, what was criticized by many authors. Then, Natanson (1911a; 1919c), following in the footsteps of earlier scholars (starting of Boltzmann), understood the statistical assumptions of "Bose statistics" as early as December 2010 (cf. his letter to Smoluchowski of 22 December 1910) – 6 March 1911 (date of presentation of his paper at the meeting of the Academy of Arts and Sciences in Kraków), but he did not give an appropriate physical model of the problem being considered and did not give a quantum derivation of the formula for the density of states what are the merits of Bose.

Einstein, in his earlier papers on the quantum theory of ideal gases, between 1916 and 1924, and Bose in his first paper in 1924, overlooked that the quanta, respectively molecules, should be treated as statistically independent entities. This was noticed by Paul Ehrenfest and "other colleagues" such as Viktor R. Bursian, Iurii A. Krutkov and Otto Halpern (see Cannals, Sauer 2010a, pp. 10, 13, 14) which was indicated by Einstein himself in 1925 in his second paper on Bose-Einstein statistics (Einstein 1925a/2015a, Doc. 385; (Eng. transl.) 2015b, Doc. 385).

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[127] Cf. Bach 1988; 1990; 2008; Nauenberg 2016, pp. 717–718.

Moreover, Einstein, P. Ehrenfest and "other colleagues" did not realize that the key to understand a difference between particles obeying quantum and classical statistics is not the indistinguishability of particles but the single-particle states: quantum states are discrete and classical states are dense.[128] However, the problem was well understood by Natanson as early as December 1910 – 6 March 1911, and "scholar colleagues" around the world knew his works on radiation and specific heat (1911a; 1911c; 1912b), because he sent these works to many of them (they had to know at least his work on radiation published in the *Physikalische Zeitschrift* (1911c), the leading journal of those days).

There is also no doubt that Boltzmann and Planck had great merits in formulating the new quantum statistics of radiation, though they didn't discover it.

Therefore, to emphasize the key achievements of Boltzmann, Planck, Natanson, Bose and Einstein, it is worth introducing a new convention and talking about *Boltzmann-Planck-Natanson statistics* (for the old radiation theory) and using the terms *Bose statistics* (for the new quantum theory of radiation) and *Bose-Einstein statistics* (for radiation and matter, i.e. for particles with a spin of 0, 1, 2, ...), taking into account the improvements introduced by Natanson, Ehrenfest, ...). It is also useful to introduce another convention and talk about *Boltzmann-Planck-Natanson-Bose-Einstein statistics* – in this case we do not pay much attention to the physical mechanisms or postulated quasi-entitites of theories and we generalize the terms "Planck-Bose statistics", introduced by John Hendry (1984); "Natanson-Bose-Einsten statistics", introduced by Józef Spałek (2005), and "Planck-Natanson-Bose-Einstein statistics", introduced by Magdalena Waniek and Klaus Hentschel (2011). Nevertheless we should remember that such terms are only conventions, which conceal and omit a number of additional co-authors.

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[128] Cf. Winterbon 1988, p. 334 (to be clear, Winterbon considered the problem of understanding the differences between the quantum and classical statistics and the indistinguishability of particles, and he did not evaluate the views of the scientists mentioned by me); Saunders 2006, p. 17.

7. Which historian of physics was the first to note the achievement of Natanson?

It is the received view that Friedrich Hund (1896–1997) was the first historian of physics who noted achievements of Natanson in quantum statistics. However, it appears that in this respect, the priority belongs to Edmund Taylor Whittaker (1873–1956), an English mathematician, physicist, historian and philosopher of exact sciences, who at his *A History of the Theories of Aether and Eelectricity*. Vol. 2. *The Modern Theories 1900–1926* (1953) on pp. 88–89 states what follows:

> Planck regarded the quantum property as belonging essentially to the interaction between radiation and matter: free radiation he supposed to consist of electromagnetic waves, in accordance with Maxwell's theory. Einstein in this paper put forward the hypothesis that parcels of radiant energy of frequency v and amount hv occur not only in emission and absorption, but that they have an independent existence in the aether. It was shown by P. Ehrenfest[1] of Leiden, by A. Joffe[2] of St Petersburg, by L. Natanson[3] of Cracow and by G. Krutkow[4] of Leiden that Einstein's hypothesis leads not to Planck's law of radiation but to Wien's, at any rate if we assume that each of the light-quanta or photons of frequency v has energy hv and that they are completely independent of each other. In order to obtain Planck's formula it is necessary to assume that the elementary photons of energy hv form aggregates, or photo--molecules as we may call them, of energies 2hv, 3hv, ... , respectively, and that the total energy of radiation is distributed, on the average, in a regular manner between the photons and the different kinds of photo-molecules.
> 1. *Ann. d. Phys*. xxxvi (1911), p. 91 [Ehrenfest 1911]. 2. Ibid, p. 534 [Joffe 1911]. 3. *Phys. ZS* xii (1911), p. 659 [Natanson 1911c]. 4. *Phys. ZS* xv (1914), p. 133 [Krutkow 1914a].

Hence, in Whittaker's opinion, the essence of Natanson's achievement and of the other physicists: Ehrenfest, Joffe, and Krutkow was not discovery of Bose-Einstein statistics, but the fact they understood that

Einstein's hypothesis of independent photons led to Wien's law, and the "aggregates of photons" or "photo-molecules" led to Planck's law.

On the other hand, it is still valid thesis that only after Friedrich Hund monograph (1967) many researchers of history of quantum statistics discovered in this field the achievements of Natanson.

8. How many Natanson's works are known by the scholars writing about Natanson's contributions to the so-called Bose-Einstein statistics?

Nearly all scholars writing about Natanson's contributions to the so--called Bose-Einstein statistics know only the third paper on the list of Natanson's works given in Section 3.1 (Natanson 1911c), that is a German translation "Über die statistische Theorie der Strahlung" (published: 15 August 1911); they do not know of the first paper on this list (Natanson 1911a), that is his English paper "On statistical theory of radiation" (published: circa 10 April 1911).

The German translation was known and quoted, among others, by the following historians of physics: F. Hund (1967, p. 35 fn. 10; English transl. 1974; and Russian transl. 1980, p. 226 fn. 10), A. Hermann (1971, p. 28 fn. 34), J. Hendry (1980, p. 73 fn. 83), A. Kastler (1983, p. 623 fn. 9), O. Darrigol (1984, p. 659; 1988; 1993), A. Bach (1988; 1990), S.K. Das, S. Sengupta (1995), A. Kojevnikov (2002, pp. 198 & 227), L. Navarro, E. Pérez (2004, p. 141), E. Garfield (2004), S. French, D. Krause (2006, pp. 91 & 404), S. Saunders (2006, pdf version, p. 21; 2009, p. 304), M. Badino (2009, pdf version, p. 26), A. Borrelli (2009, p. 77), O. Passon, J. Grebe-Ellis (2017, p. 7), etc.

This is also cited in the realistic interpretation of quantum mechanics proposed by A. Jabs (1996, p. 82, fn. 261).

Nevertheless, even this paper is omitted by several historians of physics writing about the genesis of the so-called Bose-Einstein statistics, such as H. Kangro (1970; English translation 1976), T.S. Kuhn (1978), and H. Kragh (2002).

However, the English and German versions of this paper are listed by M. Jammer (1966, p. 51 fn. 205; Russian translation: 1985, p. 60 fn. 205), M. Paty (2001, p. 22), J. Stachel (2000, pp. 245, 246, 251), S. Varró (2006a, pdf version, p. 33; 2006b, pdf version, p. 19; 2007, p. 169) and P. Enders (2007, p. 87; 2009, p. 18).

These three papers (Natanson 1911a; 1911c; 1912b) are known also by the American historian of science (of the Polish origin) M.J. Konieczny (2008, 2010, 2011, 2012), and the Japanese historian of physics N. Nagasawa ("Roh Minamida" 2009; Nagasawa 2018).

The Polish scholars who write about Natanson's contribution to the so-called Bose-Einstein statistics, such as B. Średniawa (1985, pp. 89–90; 1997, pp. 14–16; 2000, pp. 454–455; 2001, pp. 105–107; 2007, pp. 713–714, 721) and after him J. Spałek (2005; 2006; 2009), know his other work, mentioned above in Section 4.1 and listed as the fifth position on this list – Natanson 1913: Zasady Teoryi Promieniowania (The Principles of Theory of Radiation). *Prace Matematyczno-Fizyczne* 24, pp. 1–88. Warszawa: Wydawnictwo Redakcji Prac Matematyczno-Fizycznych. It is an extensive review article on the theory of radiation.[129] This rule does not apply to previous Polish researchers: J. Weyssenhoff (1958) and A. Teske (1981) did not mention any of these three works; K. Szymborski (1980, p. 66) mentioned both English and German versions of Natanson's article (1911a; 1911c); Bogdan Lange (1992a; 1992b; 1997a; 1997b), K. Czapla (2005, p. 55, fn. 27) and A.K. Wróblewski (2014, pp. 267, 273) only the German version (1911c).

In June 2011, the author of this article found out that another of Natanson's paper dealing with our subject exists. It is titled "O promieniowaniu (On radiation)". It was presented on the 19th of July in 1911 in Kraków during "XI Zjazd lekarzy i przyrodników polskich" (The 11th Congress for Polish Physicians and Natural Scientists), which was held between the 18th and the 22nd of July in 1911 (Natanson 1911b). In 1912 it was published as a reprint (Natanson 1912a). A revised version of this paper appeared in 1924 in Władysław Natanson's book, *Oblicze*

[129] However, in the case of articles marked by me as "Natanson 1912b" (i.e. "On the Energy-content of material bodies") and "Natanson 1913" (i.e. "Zasady Teoryi Promieniowania" ("The Principles of Theory of Radiation")), B. Średniawa provides incorrect information on several occasions. In the case of the first article, among others, the incorrect year 1911 of publication is consistently given. In the case of the second article, the incorrect year 1912 of publication and / or the number of pages are given, e.g.: "W. Natanson: Zasady teorii promieniowania. Wyd. Prac Matematyczno-Fizycznych. Warszawa 1912, 88 s." (Średniawa 1997, p. 20) [*88 pp. is the correct number*]; "W. Natanson, Prace Mat.-Fiz., t. 24 (1912), s. 1–352" (Średniawa 2001, p. 107) or "W. Natanson, *Mathematical and Physical Letters* 29, 1–232, Jagiellonian University 1912 (in Polish)" (Średniawa 2007, p. 721).

natury: odczyty, przemówienia i szkice (*The face of nature: lectures, speeches and essays*). Kraków: Krakowska Spółka Wydawnicza, 1924, pp. 125–153 (Natanson 1924b, i.e. Natanson 1924, pp. 125–153).

9. Conclusion: Why the name of Władysław Natanson was neglected for many years in the context of the so-called Bose-Einstein statistics?

Regarding the considerations made so far, I think that two matters worked here. Firstly, it was the lack of sufficient knowledge about primary sources, both letters and articles (it was the basis of the assumed view that Natanson's works on theory of radiation and related issues were entirely neglected and forgotten in 1910s and 1920s), and the second was the *Robert K. Merton effect,* that

> [consisted] in the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark (R.K. Merton 1968, p. 3).[130]

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[130] However, as it is commonly known, R.K. Merton named it 'Matthew effect'. I think that this term was incorrectly chosen by this author, who while studying the *Parable of the Talents* of (Matthew 25:14–30), confused the divine perspective and the sphere of *sacrum* with the temporal perspective and the sphere of *profanum*, and formulated the principle according to which the "rich man" would only get richer (gain new talents), and the poor would be even more impoverished (lose talents). But, in Christian spiritual life, this rule simply does not apply, because (taking into account other passages from the Bible) this kind of "richness" seen by a human eye can appear to be the biggest poverty of a man. It suffices only to recall another biblical passage also from Matthew (19: 24): "It is easier for a camel to go through the eye of a needle, than for a rich man to enter into the kingdom of God" (cf. *Hebrew New Testament Studies* 2003). Thus the *Parable of the Talents* is about something different: because God knows each heart, He passes a just judgment on our real intentions and activities and warns us against the consequences of bad deeds – cf. a Trapist monk Thomas Merton 1955 (2005, p. 173); 1969, p. 16; Whelchel 2013; *Wikipedia* 2019d.

In contrast, the readers of scientific works (papers, articles, books) – not possessing divine knowledge – do not know the real merits of various authors and co-authors and therefore judge them by appearances (unless the readers make a very detailed analysis of these achievements, *but in practice it is very difficult*). That's why they often pass sentences about these works and their authors that are far from the truth, e.g. exaggerated achievements of already famous scholars...

It needs to be highlighted that the effect joins three issues: a) the intellectual thefts and unreliability in quoting which unfortunately take place too often in science,[131] b) the still dominant naïve interpretations of the history and philosophy of exact sciences based on the idea that a more famous scientist by definition and at once means more than important thinkers and their greatest merits and contributions in co-authored works,[132] and c) the small or negligible final impact of authors caused by a lack of affiliation to a dominant scientific centre, a dominant science school, a dominant political faction or even a dominant nation.[133]

At the same time, I do agree with the motto of the articles of Nagasawa ("Minamida" 2009; Nagasawa 2018) about the neglected achievement of Ladislas (Władysław) Natanson:

> Citation is not only a working technique, but also an ethics, the acknowledgement of obligations and a respect for truth (Wolff 2003, p. 349).

Unlike many of his contemporary physicists, Natanson did not have any problems with this ethical and epistemological view.

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[131] This aspect is well known from bibliometric studies – cf. Kokowski 2015b, pp. 155–169.

[132] Nobel Prize winners from various disciplines paid great attention to this aspect in interviews conducted by Zuckerman 1965; 1972; 1977. And this finding was used by R.K. Merton to formulate the term 'Matthew effect'. It is worth remembering Merton's own words on the dependence of his view on Zuckerman works: "This is occasion for repeating what I have noted in reprinting the original «Matthew Effect in Science» [R.K. Merton 1968 – M.K.]: «It is now [1973] belatedly evident to me that I drew upon the interview and other materials of the Zuckerman study to such an extent that, clearly, the paper should have appeared under joint authorship.»" (R.K. Merton 1988, p. 607 fn. 2).

[133] As regards point c), it is the reason that there is no common history of physics (or science), but there are many national histories of physics (or science) – cf. A.K. Wróblewski's plenary lecture given during the 2nd International Conference of the European Society for the History of Science held in Cracow in 2006 entitled "Are we ready for common history of science?" (Wróblewski 2006).

Appendix 1. *Preliminary methodological considerations about the historical method*

Our comprehension of historical concepts, events and processes is possible thanks to the use of the historical method.[134]

To explain this method I use the ideas of 'understanding' and 'decoding' of sources by using a deliberately chosen 'hermeneutics' i.e. 'interpretative tools', and the analogies of 'a lens', 'a microscope" or 'a telescope', and I say briefly:

Our understanding of historical sources consists in decoding their content using a specific hermeneutics (i.e. certain consciously chosen interpretative tools)[135]. The aim of the historical method is to create certain narratives based on facts evidenced by historical sources and auxiliary studies. The reliability of these narratives depends on whether they 'save historical phenomena' (i.e. they are consistent with relevant selected historical events supported by the evidence in historical sources), and they are consistent also with the state of knowledge about the whole culture or some of its privileged parts.[136]

The empirical basis of such narratives is found by analysing sources of historical information (handwritten documents, printed documents etc.). In order to read these documents with a proper understanding, we must be able to recognize handwritten characters (if we have to study

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[134] For a description of this method see, for example, Droysen 1858; Freeman 1886; Bernheim 1889 (1st ed.; 1908 5th ed.); Langlois, Seignobos 1898 (ed. of 1992); Garraghan 1946; Gottschalk 1950; Butterfield 1955 (reissued 1969); (about German historicism) Iggers 1968 (rev. ed. 1983);Topolski1968 (3rd corr. ed. 1984); 1978; 1983; 1998; Shafer 1974; McCullagh 1984; Firat 1987; Bentley (ed.) 1997; Bentley 1999; Phillips 2000; Howell, Prevenier 2001; Rüsen (ed.) 2006; Wood 2008; Edwards Education Blog 2013; Porra, Hirschheim; Parks 2014; Levitin 2015; Janssen 2017; Morgan 2017; Dagg 2019.

[135] For details see Kokowski 1999; 2001, pp. 5–9; 2007.

[136] I apply here an analogical reasoning – I mean the Hellenistic expression used in philosophy of nature 'Ζωζειν τα Φαινομενα' („to save phenomena"), and the ideas of 'external confirmation' and 'inner perfection' of a physical theory by Einstein; see Duhem 1908/1969 (reprinted 1985), and Einstein 1949, pp. 20–25. On the margin: this approach is versatile and more universal than the approach assumed in 'general history', 'philosophical history', 'conjectural history' (the latter developed by Montesquieu and Scottish scholars), and annals, memoirs, biography, and literary history – cf. Phillips 2000.

handwritten documents) and printed characters, as well as to recognize a linguistic dimension of the documents studied (the content of documents) we must possess proper linguistic abilities (in national languages, and languages of scientific disciplines).

In the historical approach, we can use a whole spectrum of different strategies (from antiquarianism[137] to presentism[138]) and apply these strategies to analyse concepts (their content, genesis, and reception including a generation of new conceptual contents), events or historical processes from past to present times, and *vice versa* (I mean progressive studies or regressive ones) or at a particular time in the history.

However, the more we move away from the historical context of our times, learning about past events becomes more and more difficult. This is because they are mediated by the way these events were understood by direct witnesses, and subsequently how various categories of interpreters understood the witness statements on these events (including translators / linguists), and at the end, by ourselves, who, immersed in some thought collectives, are also interpreters of the past.[139]

Moreover, our comprehension of historical events or processes, i.e. the result of applying the historical method, is always hypothetical, as it is based on many hypothetical premises. In researching and writing

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[137] See Sweet 2008; Levitin 2015; Janssen 2017; the genesis of this approach is described in Momigliano 1990, chap. 3. "The rise of Antiquarian research", pp. 54–79. "The essence of antiquarianism is a focus on the empirical evidence of the past, and is perhaps best encapsulated in the motto adopted by the 18th-century antiquary Sir Richard Colt Hoare, «We speak from facts, not theory.» Today the term is often used in a pejorative sense, to refer to an excessively narrow focus on factual historical trivia, to the exclusion of a sense of historical context or process" (*Wikipedia* 2019a).

[138] Presentism is an anachronistic interpretation of the past from the point of view of the present-day ideas. See, for example, Fischer 1970, pp. 135–140; Syrjamaki 2011, pp. 20–49. However, in the strict sense of the word, one cannot completely avoid such a research perspective – see, for example, Fendler 2008; Spoerhase 2008.

[139] "The whole modern method of historical research is founded upon the distinction between original and derivative authorities. By original authorities we mean either statements by eye-witnesses, or documents, and other material remains, that are contemporary with the events which they attest. By derivative authorities we mean historians or chroniclers who relate and discuss events which they have not witnessed but which they have heard of or inferred directly or indirectly from original authorities. We praise original authorities – or sources – for being reliable, but we praise non-contemporary historians – or derivative authorities – for displaying sound judgment in the interpretation and evaluation of the original sources" (Momigliano 1950, p. 286).

about the historical past, apart from determining things, i.e. material documents of the epoch, dates and places of events or processes, it is also important to determine the names of people who played important roles (not necessarily major) in those events or processes. They are all needed to build a reliable story, written by a historian, which must persuade the audience to the picture described in such a story.[140]

In the case of the historiography of sciences, we must also know documents (archival documents, published works, instruments, buildings, etc.), dates and places. As regards to scientists, we need to find not only names of discoverers, but also of their precursors and epigones. Regarding the story written by a historian of science, it must also be well-composed to persuade the audience to the picture described in such a story.[141]

This is all linked with several important issues that have been discussed for a long time by sociologists, historians of science, philosophers of science and intellectual historians. They include: the priority of discovery,[142] multiple discoveries,[143] scientific precursors,[144] anachronism[145] including the Whig or Whiggish history of science and its overcoming by a detailed contextual research,[146] the still existing intriguing discrepancies in establishing by historians of science in different linguistic circles a coherent list of names of important scientific discoverers,[147] and finally the methodology of the historiography of sciences, which joins the methodology of historiography with the methodology of sciences.

While generalizing the problem of the historical narrative (both in historiography and historiography of sciences), it is worth distinguishing in every text (including a scientific text) three elements or strata: "*the form of the text* (the literary form of the text), *the hermeneutics of the text*

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[140] Writing this story is linked with a more general problem of constructing the historical narrative by a historian – cf. Munz 1997; Topolski 1998.

[141] About the history of the historiography of sciences cf. Agassi 1963; Kragh 1987; Markova 1987; Catana 2011. On Duhem's return to sources, see Le Roux, Krasnodębski 2017, pp. 37–41.

[142] R.K. Merton 1957.

[143] R.K. Merton 1961a; 1961b; 1963; Bikard 2013.

[144] Duhem 1908; 1913–1959; Langlois 1994, p. 1.

[145] Skinner 1969; Prudovsky 1997; Jardine 2000; Spoerhase 2008; Špelda 2012.

[146] Butterfield 1931; 1949 (2nd ed. with corrections 1957); Hyman 1988; Mayr 1990; Jardine 2003.

[147] Wróblewski 2007.

(that is all means applied explicitly or implicitly in the text to interpret the subject under study) and *the rhetoric of the text* (that is all means serving to convince the reader to the expounded theses".[148]

Regarding the methodology of the so-called exact sciences, we know that every theory belonging to these sciences is composed of a mathematical model and of quasi-entities defined in the context of this model. We also know that the hypothetico-deductive method of correspondence-oriented thinking (*Korespondenzdenken*) is a very useful tool for understanding the development of such sciences. The created new theories are often linked with earlier theories by correspondence principles and it is not accidental – it is a result of the application of the postulate of correspondence of subsequent theories by scientists. Every perfect theory must be logically and mathematically coherent and save phenomena. The latter is realized by using measuring instruments and correspondence rules linking observables and their representations postulated in the context of the theory.[149]

Historians of science – in their investigations of the past science – have the freedom to choose any reasonable research hermeneutics. It can stem from the methodology of the so-called exact sciences, philosophy of science, sociology of scientific knowledge, etc. The main demand is epistemic coherence and fruitfulness of this approach, tested by decoding the previously encoded content of scientific publications.

Appendix 2. *Comments regarding terminology*: "Bose statistics", "Bose-Einstein statistics", "Einstein-Bose statistics" and "Planck-Bose statistics"

Shortly after the release of S.N. Bose's articles: 1924a (reprinted 2009a; English transl. 2009b); 1924b (reprinted 2009c; English transl. 2009d), and then A. Einstein's articles: 1924 (reprinted 2015a, Doc. 283; Eng. transl. 2015b, Doc. 283); 1925a (reprinted 2015a, Doc. 385; Eng transl. 2015b, Doc. 385); 1925b (reprinted 2015a, Doc. 427; Eng. transl. 2015b, Doc. 427), the terms "Bose statistics", "Bose-Einstein statistics" or "Einstein-Bose statistics" (emphasizing the primary role of Einstein in

[148] About this understanding of the methodology of historiography of sciences cf. Kokowski 1999; 2001, pp. 5–9, 317; 2007.

[149] More about these aspects cf. Kokowski 1996; 2012; 2015c.

its genesis) and their equivalents in other languages than English were introduced in the recognition of the achievements of the authors of these works.

The term "Bose statistics" refers to the statistics that Bose introduced in the case of blackbody radiation (the quanta of light, called photons after 1926),[150] and the terms "Bose-Einstein statistics" or "Einstein-Bose statistics" to the new quantum statistics, which was generalized by Einstein also for the case of ordinary matter particles (perfect gas).

1) "Bosesche Statistik": Adolf Smekal to Albert Einstein (Vienna, 5 February 1925; cf. Einstein 2015a, Doc. 434, p. 644); Max Born to Albert Einstein (Göttingen, 15 July 1925; cf. Einstein 2018, Document 23, pp. 70–72, here p. 70); Pascual Jordan to Albert Einstein (Göttingen, 27 October 1925; cf. Einstein 2018, Document 98, p. 177); Smekal 1926, p. 319; Jordan 1927, p. 637; Jordam, Wigner 1928, p. 635/Wigner 2013, p. 113; Haas 1928, pp. 114, 119, 120, 121, 122, 124, 126, 129; Heitler, Herzberg 1929; Fierz 1939, pp. 3, 28.
2) "Bose statistics": Weisskopf 1939, pp. 72, 75, 82 / 1958, pp. 68, 71, 78; Pauli 1940, p. 13; Feynman 1949, pp. 773, 782; Feynman 1950, p. 452 / 1958, p. 269; Kubo et al. 1965, p. 29. fn. †; Sudarshan 1968, p. 379; Sudarshan 1974/1975, p. 70.
3) "Статистика Бозе": Leontovich 1944, pp. 174, 175, 176; Landau, Lifshitz 1937–1939 (ed. 1975, p. 180) / (English transl.) 1958, p. 153).
4) "Bose-Einsteinischen Statistik": Smekal 1925, p. 613; Born, Heisenberg, Jordan 1926 (received 16 November 1925), p. 609 [reprinted in: Heisenberg 1985, p. 449]; Albert Einstein to Paul Ehrenfest (Berlin, 24 November 1926; cf. Einstein 2018, Document 420, pp. 644, 645); Jordan 1927, p. 637; Wigner, Witmer 1928, p. 868 / Wigner 2013, p. 176; Schaefer 1937, pp. 30, 405 fn. 411; Schäfer 1950, pp. 208, 209.
5) "Statystyka Bosego-Einsteina": Skłodowska-Curie 1939, p. 98.
6) "Статистика Бозе – Эйнштейна": Leontovich 1944, pp. 174, 176; Frenkel' 1948, p. 637, 643, 644.

[150] According to the current views, the term "photon" originated in 1916, but gained acceptance after the article by Gilbert N. Lewis (1926). On the genesis of this term and its meaning see Kragh 2014 and Hentschel 2018.

7) “Bose-Einstein statistics”: Feynman 1950, p. 454 / 1958, p. 271.
8) “Einstein-Bose statistics”: Wigner 1926, p. 492 / 2013, p. 34; Dirac 1927, pp. 245, 247, 250, 251, 253, 255, 260 / 1958, p. 3, 5, 8, 9, 11, 13, 18; Fowler 1929, pp. 537, 543, 553, 556, 557, 559; Rasetti 1929, p. 516; Kemmer 1938, p. 127; Pauli 1940, p. 13; Lindsay 1941 (6th priting 1962, p. 193).
9) “La statistique de Einstein-Bose”: Born, Heisenberg, Langevin, Kramers, Dirac (cf. Institut international de physique Solvay 1928, pp. 175, 176, 269, 270, 271, 272; Pauli 1936).

Moreover, some researchers assumed that the terms “Bose statistics” and “Bose-Einstein statistics” are only different names for the same statistics called “Bose statistics” or “Bose-Einstein statistics” – Landau, Lifshitz 1937–1939 (ed. 1975, p. 180) / English transl. 1958, p. 153; Leontovich 1944, pp. 174, 175, 176); Kubo et al. 1965, p. 29 fn. † / Russian transl. 1967, p. 43 fn. 2.

After the article by Wolfgang Pauli (1940) it was known that the statistics of BE describes particles with spin 0, 1, 2, ... Taking this into account and acknowledging S.N. Bose’s merits, P. Dirac (1945 [cited after Farmelo 2009, p. 331, n. 64], 1947) named particles with spin 0, 1, 2, ... bosons.

> The new statistics was first studied by Bose, so we shall call particles for which only symmetrical states occur in nature bosons. […] We can see the difference of Bose statistics from the usual statistics by considering a special case – that of only two particles and only two independent states a and b for a particle. […] Thus with Bose statistics the probability of two particles being in the same state is greater than with classical statistics. Bose statistics differ from classical statistics in the opposite direction to Fermi statistics, for which the probability of two particles being in the same state is zero. […] Planck’s law of radiation shows us that photons are bosons, as only the Bose statistics for photons will lead to Planck’s law (Dirac 1947, 3rd rev. ed., pp. 210–211).

However, in the first edition of Dirac’s monograph of 1930 he applied a more general term: “Einstein-Bose statistics”:

> This statistical mechanics is known as the Einstein-Bose statistics, as it was first introduced by Bose and Einstein before the arrival of the modern quantum mechanics (Dirac 1930, p. 201; cf. also p. 219).

Currently, the dominant term is the term "Bose-Einstein statistics" (Google: 153,000) and its equivalents in other languages: "Статистика Бозе-Эйнштейна" (58,600), "Statistique de Bose-Einstein" (21,000), "Bose-Einsteinschen Statistik" (201), "Statystyka Bosego-Einsteina" (470). The term replaced the term "Einstein-Bose statistics" (Google: 3,460) and its language equivalents: "statistique de Einstein-Bose" (81), "Einstein-Bosesche Statistik" (7), "statystyka Einsteina-Bosego" (1).

The term "Bose statistics" is still in use (Google: 26,700) and its language equivalents: "Статистика Бозе" (2,430) "statistique de Bose" (1,050), "statystyka Bosego" (86), "Bosesche Statistik" (15).

Moreover, John Hendry (1984, p. 70) uses the term "Planck-Bose statistics" instead of the term "Bose statistics" (only two cases in Google linked with Hendry's book).

Appendix 3. Letter from Max Planck to Władysław Natanson (25 January 1913)

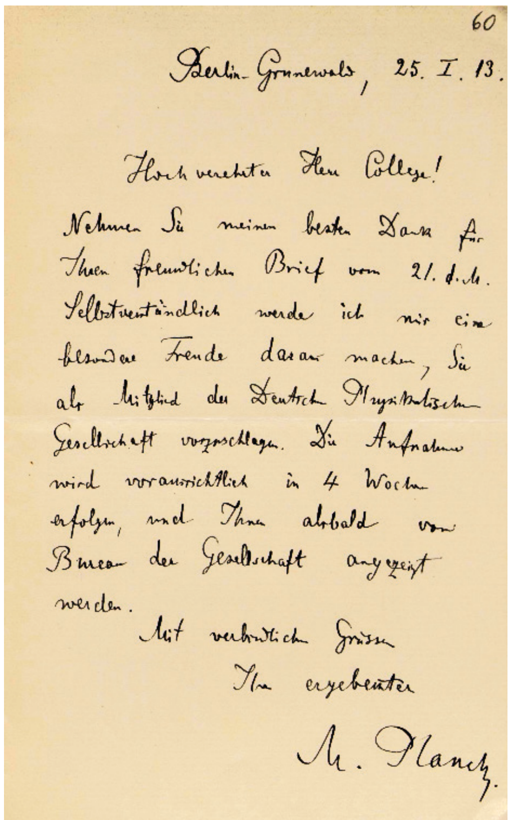
There is very interesting evidence from the beginning of 1913 that Planck appreciated Natanson as a physicist. This is Sommerfeld's letter from 25 January 1913 sent from Berlin-Grunewald to Natanson in Kraków:

I attach below a transcription of this letter with its translation into English.

> Berlin-Grunewald, 25.I.13
>
> Hoch verehrter Herr College!
>
> Nehmen Sie meinen besten Dank für Ihnen freundlichen Brief von 21. […] Selbstverständlich werde ich mir eine besondere Freude daran machen, Sie als Mitglied der Deutscher Physikalischen Gesellschaft vorzsuchlagen. Die Aufnahme wird voraussichtlich in 4 Wochen erfolgen,

und Ihnen alsbald von Bureau der Gesellschaft angezeigt werden.

Mit verbindlichen Grüssen
Ihr ergebenster
M. Planck



60

Berlin-Grunewald, 25. I. 13.

Hochverehrter Herr College!

Nehmen Sie meinen besten Dank für Ihren freundlichen Brief vom 21. d. M. Selbstverständlich werde ich mir eine besondere Freude daraus machen, Sie als Mitglied der Deutschen Physikalischen Gesellschaft vorzuschlagen. Die Aufnahme wird voraussichtlich in 4 Wochen erfolgen, und Ihnen alsbald vom Bureau der Gesellschaft angezeigt werden.

Mit verbindlichen Grüssen
Ihr ergebenster
M. Planck.

Fig. 16. Sommerfeld's letter to Natanson (25 January 1913). Source: © Biblioteka Jagiellońska, photo: © Michał Kokowski.

Berlin-Grunewald, 25 Jaunary, 1913

Esteemed Colleague!

Please receive my great thanks for your kind letter of 21. [illegible] It goes without saying that it will be a great joy to recommend you as a member of the German Physical Society. The admission is expected to take 4 weeks, and will be announced to you by the Society's Bureau.

With binding regards
Your most devoted
M. Planck

Appendix 4. Excerpts of two letters from Sommerfeld to Rubinowicz (Stockholm, 1 October 1919; München, on 1 November 1919). Transcription and translation

In the letter from Stockholm, on 1 October 1919 – see Sommerfeld 1919a (*archival document*) – we read on p. 1/2 as follows:

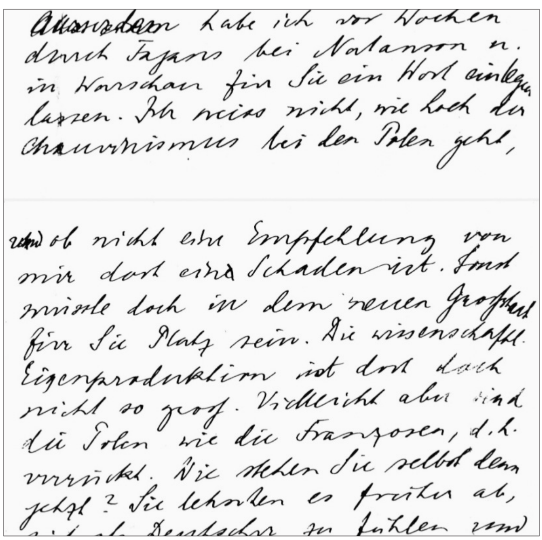

Ausserdem habe ich vor Wochen
durch Fajans bei Natanson u.
in Warschau für Sie ein Wort einlegen
lassen. Ich weiss nicht, wie hoch der
Chauvinismus bei den Polen geht,
und ob nicht eine Empfehlung von
mir dort ein Schaden ist. Und
müsste doch in dem neuen Grossstaat
für Sie Platz sein. Die wissenschaftl.
Eigenproduktion ist dort doch
nicht so gross. Vielleicht aber sind
die Polen wie die Franzosen, d.h.
verrückt. Wie stehen Sie selbst dem
jetzt? Sie lehnten es früher ab,
sich als Deutscher zu fühlen und

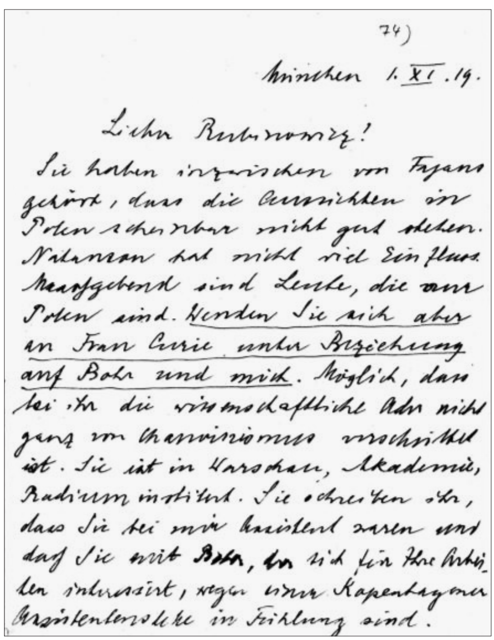
Fig. 17. The excerpt of the letter from Sommerfeld to Rubinowicz (Stockholm, 1 October 1919). Source: Sommerfeld 1919a.

Ausserdem habe ich vor Wochen durch Fajans bei Natanson u.[nd] in Warschau für Sie ein Wort einlegen lassen. Ich weiss nicht, wie hoch der Chauvinismus bei den Polen geht, und ob nicht eine Empfehlung von mir dort ein

Schaden ist. Sonst würde doch in dem neuen Grossstadt für Sie Platz sein. Die wissenchaftl.[iche] Eigenproduktion ist dort doch nicht so gross. Vielleicht aber sind die Polen wie die Franzosen, d.h. [das heißt] verrückt. Wie stehen Sie selbst denn jetzt? [Sommerfeld 1919c (*archival document*), p. 1; transcription – M.K.; F.K.].

Additionally, a few weeks ago I left a word for you via Fajans at Natanson's as well as in Warsaw. I do not know how much chauvinism the Poles hold, and whether a recommendation from me would not cause a problem. Otherwise, there would be a place for you in a new big city. Their own scientific production is not so large. Yet, perhaps the Poles are like the French, that is crazy. What would be your stance now? [translation – M.K.].

In the letter of München, on 1 November 1919 – see Sommerfeld 1919c (*archival document*) – we read on p. 1 as follows:


74)
München 1.XI.19.

Lieber Rubinowicz!
Sie haben inzwischen von Fajans gehört, dass die Aussichten in Polen scheinbar nicht gut stehen. Natanson hat nicht viel Einfluss. Maassgebend sind Leute, die von Polen sind. Wenden Sie sich aber an Frau Curie unter Beziehung auf Bohr und mich. Möglich, dass bei ihr die wissenschaftliche Ader nicht ganz von Chauvinismus verschüttet ist. Sie ist in Warschau, Akademie, Radiuminstitut. Sie schreiben ihr, dass Sie bei mir Assistent waren und daß Sie mit Bohr, der sich für Ihre Arbeiten interessirt, wegen einer Kopenhagener Assistentenstelle im Frühling sind.

Fig. 18. The excerpt of Sommerfeld's letter to Rubinowicz (München, on 1 November 1919). Source: Sommerfeld 1919c (*archival document*).

> Liber Rubinowicz,
>
> Sie haben inzwischen von Fajans gehört, dass die Aussichten in Polen scheinbar nicht gut stehen. Natanson hat nicht viel Einfluss. Massgebend sind Leute, die nur Polen sind. Wenden Sie sich aber an Frau Curie unter Beziehung auf Bohr und mich. Möglich, dass bei ihr die wissenschaftliche Adr [Adresse] nicht ganz von Chauvinismus verschriftet ist. Sie ist in Warschau, Akademie, Radiuminstitut [sic! – in fact, she was in Paris]. Sie schreiben ihr, dass Sie bei mir Assistent waren und dass Sie mit Bohr, der sich für Ihre Arbeiten interessiert, wegen einer Kopenhagener Assistentenstelle in Fühlung sind [Sommerfeld 1919c (*archival document*), p. 1; transcription – M.K.; F.K.].
>
> Dear Rubinowicz,
>
> In the meantime you have already heard from Fajans that the prospects for a job in Poland are not good. Natanson does not have much influence. Only people who are Polish have some authority. However, contact Ms. Curie and refer her to Bohr and me. It is possible that her scientific stance is not entirely chauvinistic. She is in Warsaw, Academy, Radium Institute [sic! – in fact, she was in Paris]. Write to her that you worked as an assistant with me and that you are in touch with Bohr, who is interested in your work regarding an assistant position in Copenhagen [translation – M.K.].

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State of competition: conceptual shoehorning behind priority on calcitonin precursor biosynthesis

Abstract

Until the 1950s, the first results in the studies of calcitonin-thyrocalcitonin were ignored in the accepted research scheme. However, it was José Fernández Nonídez from the Spanish School of Histology, died in Augusta (Georgia, USA) in 1947, whose expertise in the parafollicular cells of the mammalian thyroid had led him to an advanced understanding of this separate endocrine organ, which secretes calcitonin. The antecedent of the secretion was present in the cytoplasm of these cells, which Nonídez explained in a paper published in 1932.

In 1973, a Spanish group from the Instituto Gregorio Marañón (Madrid) leading the research into the ectopic production of calcitonin identified the precursor responsible for its biosynthesis.

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Nonetheless, given the informal power in connection with the communication between the scientists, this significative contribution was absolutely discarded in terms of acknowledgment within their social circle. The services responsible for dissemination of scientific knowledge considered that priority should be given to another group of young scientists dedicated to pro-calcitonin evidence.

The nature and extent of informal communication are highlighted in countries with different measures to guarantee the autonomy and independence of their state powers. Irrespectively of political circumstances, the paper is focused on the competition between two different approaches in science particularly important for progress in medicine: the perspective presented by experimental studies in basic sciences (in animals) and the models developed in clinical sciences.

Keywords: *anatomy, thyroid, endocrinology, Madrid, calcitonin, thyrocalcitonin, pro-calcitonin*, *Instituto Gregorio Marañón*, *Jose Fernández Nonídez, conceptual shoehorning, thought collective*

Stan rywalizacji: priorytetowe znaczenie biosyntezy prekursora kalcytoniny i zniekształcenia pojęciowe

Abstrakt

Do lat 50. XX w. nie uwzględniano w przyjętym schemacie badawczym pierwszych dowodów przemawiających za istnieniem kalcytoniny-tyrokalcytoniny. Jednakże, znajomość przez José Fernándeza Nonídeza (z hiszpańskiej szkoły histologii, który zmarł w Augusta (Georgia, USA) w 1947 r.) komórek pęcherzykowych tarczycy ssaków doprowadziła go do zaawansowanego rozumienia tarczycy jako odrębnego narządu endokrynnego, wydzielającego kalcytoninę. W artykule opublikowanym w 1932 r. Nonídez wyjaśnił, że prekursor wydzielania znajdował się w cytoplazmie komórek tarczycy.

W 1973 r. hiszpańska grupa z Instituto Gregorio Marañón (Madryt), prowadząca badania nad ektopowym wydzielaniem kalcytoniny, zidentyfikowała prekursora odpowiedzialnego za

biosyntezę kalcytoniny. Niemniej jednak, biorąc pod uwagę nieformalną władzę związaną z komunikacją między naukowcami, znaczący wkład Nonídeza został absolutnie odrzucony i nie zdobył uznania w tej grupie społecznej. Upowszechnianie usług wiedzy naukowej spowodowało, że przyznano priorytet odkrycia grupie młodych naukowców zajmujących się badaniami pro-kalcytoniny.

Podkreślono charakter i zakres nieformalnej komunikacji w krajach, które wykorzystują różne środki dla zagwarantowania autonomii i niezależności uprawnień państwowych.

Niezależnie od uwarunkowań politycznych artykuł koncentruje się na konkurencji między dwoma różnymi podejściami w nauce, szczególnie ważnymi dla postępu w medycynie: perspektywą przedstawioną w badaniach eksperymentalnych w naukach podstawowych (u zwierząt) i modelach opracowanych w naukach klinicznych.

Słowa kluczowe: *anatomia*, *tarczyca*, *endokrynologia*, *Madryt*, *kalcytonina*, *tyrokalcytonina*, *pro-kalcytonina*, *Instituto Gregorio Marañón*, *Jose Fernández Nonídez*, *zniekształcenia pojęciowe*, *kolektywy myślowe*

1. Introduction

Despite Ortega y Gasset's words that "perhaps, science and government are the two most contradictory human activities" (Ortega 1969),[1] when reading the history of Spain in the 20th century as marked by a relationship between "new science" and government, a historian (as well as a sociologist) should examine the generational singularity, the particular features of biographies. The facts are plausible, as Baltazar Gracián used to say, as far as they are fully described and justified (Gracián 1685).[2] This

[1] "Rather, it can be learned from history that intellectuals are one thing in politics: obstacles. Perhaps, science and government are the two most contradictory human activities. The intellectual who knows what his destiny implies, instead of asking the politician to make him a member of the Parliament, he should ask him to read his books. If he is successful, he would have shaped public policy as far as his influence could reach." This text was originally published in the Madrid newspaper El Sol on July 10, 1922 (Ortega y Gasset 1969).

[2] "In matters that concern judgment, it is dangerous [novelty], because it runs upon paradoxes; in knacks of subtlety it is laudable: and if novelty and invention jump well together, they are plausible" (Gracian 1685, maxime 283).

trust can and should be analyzed in a broader and even openly political domain, like in history when it focuses on competition as an underlying theme to bridge contexts of discovery and justification (Allchin 1994).

The case of disagreement among researchers on the precursor of calcitonin, a hormone that maintains the constant level of calcium in the blood, provides an example. The available historical information on this case indicates a pattern where relevant facts are omitted, emphasis is misplaced, and qualifications are abandoned. This dominance of conceptual shoehorning reflects a deliberate distortion or a conscious manipulation of historical facts and could be used to help students learn how to do science (Allchin 2003; 2004).[3] In the early fascist period in Spain, a well-known example of these errors of historical shoehorns resulted in obsolete scientific theories like reticular theory in neurobiology regaining attention when the Cajal Institute was re-founded by CSIC (the Spanish Research Council) (Jabonero *et al.* 1951). At the time of Cajal's posthumous publication (1934), a paucity of new evidence in support of the synaptic discontinuity led to a political requirement being made with regard to the neural theory. The neuron theory with its insistence on the anatomical discontinuity of all neurons and theories based on chemical mediation across hypothetical gaps was said to require a major revision. Eminent neurohistologists in Nazi Germany (Philipp Stöhr (Bonn), Karl August Reiser (Bonn)), and a number of contributors (Jabonero *et al.* 1951; Jabonero 1952) from the histologist in Spain, Jabonero, discussed the idea of a terminal reticulum in the autonomic nervous system. Nonídez, who went into exile as a teacher of anatomy at Cornell University in Ithaca, New York (Pinar 2001), presented a sober, midway account of the struggle (Nonídez 1944).

We are dealing in this communication with a chronicle where only one key is used to open two different locks. These two different locks

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[3] It is the argument of this contribution that the history of biomedical experiments on calcitonin is an important example to indicate a purely political affair behind scientific competition. Following Fleck, all knowledge expresses the forces that makes it intelligible; "instead of what separates, it will point to that which will be common to all, and which brings them closer together" (Fleck 1947/1936). Collectively, the structure of the relevant thought is stylized and such stylized solution is called truth (Fleck 1935/1979). This proposed Fleckian framework provides a way of considering the historical shoehorn as a finding way to explore the potential for distortions studied here.

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are the breath of the Spanish exile molding science in Franco's Spain, and the disappointed absence of competitiveness faced by those scientists when confronted with the march of invention abroad. The key to open these two doors is the purely political problem of control of scientific work on the international scene to abolish aggression. Always with an emphasis on the fact that in a particular context a combination of careful measurements and sensitive and specific experiments makes the discovery possible (Gervais, Weber 2015),[4] the determination of the problem for the study attracted our attention because of its precarious intellectual discussion, previously retained in the context of the history of biology.[5] Consistently drawn on competition as an underlying theme, the rapid development of the C cells research, as indicated by the publication rate, compels to its study.[6]

2. Scope determination

In a certain sense, this research question has taken us aside, as an interaction between two worlds. Clinical practice in Spanish endocrinology gained in importance with the exile of the scientific community. And one of the key elements of the history of experiments on procalcitonin (PCT) is its use in basic and clinical research as a biomarker

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[4] High levels of procalcitonin are observed in cancer patients (making the neuroendocrine cells of the lungs a probable site of its production), burnt patients with severe lung injuries brought about by the inhalation of toxic gases, and pediatric patients with a signaling increased level in bacterial and not in viral infections (e.g. young patients with meningitis). A few months before the Gulf War in 1991, a group of French army physicians while healing burnt patients revealed the relationship between procalcitonin and sepsis (Bohuon 2000). Positive results of the procalcitonin test in infections associated with a severe systemic response resulted in an increase in research into the prognostic value of the measurement of its release into the blood (by the spleen, the liver, the testes, or the brain).

[5] As the founder of the Seville department of genetics, Enrique Cerdá Olmedo, has asked for concrete relevance or benefits of biotechnologies for different sets of audiences (Ávila *et al.* 2003).

[6] Pearse showed that calcitonin was localized in the parafollicular cells (Bussolati 2014); these cells are now commonly called the C cells, but they have also been called the cells of Nonídez (Goormaghtigh, Thomas 1934), and light cells of the thyroid (this last name was introduced in preference to parafollicular cell because it did not imply any restriction of the cells to any particular topographical site).

(Bohoun 2000).[7] So the models to understand the production of science through the computational methods that characterize the science of science had a pending thorough study on the relative efficacy of the alternative portfolios in the basic vs clinical competition for the space between theory and praxis. The procalcitonin case could be an interesting historical reference, by tying its discovery to the framework of the latter (Markoš, Švorcová 2009), by a scientist and a medical professional distinguished in the defense of democracy in Spain and forgotten in the US, who first understood the relevance of C cells in the thyroid gland.[8]

A kind of symbiosis between the two worlds, the process of which led to the discovery of a large precursor to calcitonin – this is a real live-science history which comes out of journals, but journals are tough to master (Price 1981). Because of this it can be discussed as a political history of a research community through the microanalytic strategies relevant to invisible colleges (Ogurtsov 2001). This is a strategy that focuses attention on the power of personal and informal contacts to shape scientific careers, and generally is identified with the history of society as a course of interpersonal ways of communication, forms, entities, institutions and practices.

3. Models for the study of thyroid in different basic and clinical contexts

What passed unperceived and no attempt at a remedial action was made, was divide between the first period of discoveries on thyroid (1930–1938), which is characterized by the activity (at Cornell University) of José Fernández Nonídez from Spain (Pinar 2001), and the research to find the precursor of calcitonin that embraced new biotechnologies (in the seventies). Simply put, what was involved was the continuity of those who supported Spanish science, formally around the journal

[7] The existence of this biologically active pro-hormone (PCT) was established chromatographically for the first time in 1975 (Moya *et al.* 1975), unequivocally associated with sepsis and microbial infections, a fast detection of PCT levels directly at the patient's bed would result in significant help in treatment.

[8] In 1932, to bring about the triumph in the academic world of a just and democratic freedom, Nonídez fought with all his might from the pages of the journal *Science* (a periodical publication of the American Association for the Advancement of Science, AAAS (Nonídez 1932b)).

Fig. 1. The clinical researcher José Luis Rodríguez Candela (1908–1985) at CSIC and Professor José Fernández Nonídez (1892–1947) at Cornell University. (Source: historical pictures in the General Archive of the University of Navarre and in the National Natural Science Museum (CSIC) Archive.)

published by the Spanish republicans *Ciencia* (López Sánchez 2014).[9] When it is said that in these days action was for the scientists a part of their everyday professional life, and to run away from it was as inconceivable for them as to close their laboratories (Rodríguez Quiroga 1996), Cajal's conviction on the importance of will, at the basis both of political and scientific affairs (Ramón y Cajal 1951), made the skills of the scientist close to the main characteristic of a politician, never losing faith in the usefulness of their actual tasks. So when in 1947 in Paris, the physiologist Juan Negrín talked about the Marshall Plan aid for Spain,

[9] Spanish medical and scientific culture did fit within a journal published in México D.F., between 1940 (March 1) and 1975 (December 15). *Ciencia. Revista hispano-americana de Ciencias puras y aplicadas* was digitized after a 2009 congress "The Republican Scientific Exile" organized by the López Piñero History of Science and Medicine Institute. Its 29 volumes are available from the Universitat de València and the Residencia de Estudiantes – see: http://cienciaexil.uv.es/volumenes.htm and http://www.edaddeplata.org/tierrafirme_jae/revistaciencia/index.html.

and Nonídez died in the USA, the profoundness of Spanish exile was a central element shaping research directions concerning endocrinology in Spain, which occurred differently in Britain or Austria (Medvei 1982).

I offer below a brief inquiry within this large compass, my purpose being to exhibit a concrete case of a generation facing an original aspiration to work simultaneously in science and politics, and the professional roles of biological researchers in the 1970s, under the historical parameters of molecular endocrinological research. After the Francoist war, to add to the coincidence between the courageous scientific minds and the courageous political minds follows the access of fascist scientists to international politics (Price 1945). And, following the Korean War and the conditional determination of the great powers to build an international organization against war, the Spanish Diabetes Society was founded in 1955 by José Luis Rodríguez-Candela. Rodríguez-Candela was an official in the post of adjunct professor of physiology (at the Madrid University Medicine School), left by the exile S. Ochoa, who had a major interest in the mechanisms of action of hormones, in particular running calcitonin research in the early seventies.

One has to mention the fact that, at that time in Spain, all research into endocrine glands was done in the service of clinical medicine. And as the war is not lost if it is not given up, the development of the research worked in the opposite direction to the firm beliefs of Juan Negrín, who had sharply criticized clinicians' excesses (Medvei 1982). The real problem was not the sharing of information, or the control of scientific work, but the purely political problem of exile. Vain theories and even errors of the over-zealous clinicians slipped under the radar. At the core of these deficiencies was the abolishment of the political unity. That can only exist if the great scientists stand together. Without the generous solidarity and sense of universality (intentions to re-establish democracy in Spain) on the part of the Republican exile, the institutionalization of endocrinology followed a model which had been handed down in a limited quantity of printed materials.

Because the procalcitonin literature in 1974–1975 was concerned with the intellectual unity of an overarching network of co-authorships during the "sixties" period of the Spanish biochemistry, it is argued here that this discovery mobilized a research effort which was not duly recognized. The problems specified by the discovery of the biosynthesis of the precursor of calcitonin are very difficult to consider away from the historical molds which the research faced in this field. The starting point here, as explained by Studer, is that the history becomes a properly sociological tool for disaggregating and interpreting scientific growth (Studer 1977); because in 1975 procalcitonin (PCT) was identified as one of the precursors to calcitonin (CT) in animals, prior to being discovered in humans and was quickly vitally relevant as an integrative factor in endocrinology. Beyond its seemingly modest content, procalcitonin is a very interesting and original marker, and its brief history continues to assume an integrative role for those biomedical researchers interested in calcitonin – itself an excellent marker for medullary thyroid cancer (Bohuon 2000) – who are looking for a historical interpretation for diabetes, and an endocrine-related cancer.

4. Cells producing calcitonin

This contribution analyses the emergence of C cells research in Spain throughout the comparative histories of José Fernández Nonídez (1892–1947) and José Luis Rodríguez Candela (1908–1985). Significantly, J. F. Nonídez formulated the problem very carefully, and put the name of parafollicular cells (also called C cells) to the thyroid gland cells where calcitonin is secreted (Nonídez 1932a). The economic and political powers, which made Nonídez due funding and institutional support possible, paid special attention to the different experimental cultures (Mendelism genetics) which emerged on the basis of his effort while he was in Spain. Rodríguez-Candela was awarded the title of Doctor in Medicine from the University of Madrid (1932) with a distinction "cum laude", given by a jury including Juan Negrín, for his doctoral thesis on experimental diabetes. After the war he was transferred to the CSIC, where he spent most of his career dealing with the director's duties at the Institute of Metabolism and Nutrition; his most cited publication concerned the discovery of the calcitonin precursor in 1975.

Histology, with the silver nitrate method of Cajal (Ferreiro, Ferreiro 1984), brought the parafollicular (C) cells of the thyroid gland into consideration (Fig. 2). Together with Baber's first evidence, Nonídez's professional work endeavored to demonstrate the relevance of C cells, and made him their chief proponent. Baber noticed spaces between these cells; for Nonídez the spaces resulted from the release of a fluid secretion produced through dissolution of the granules in their

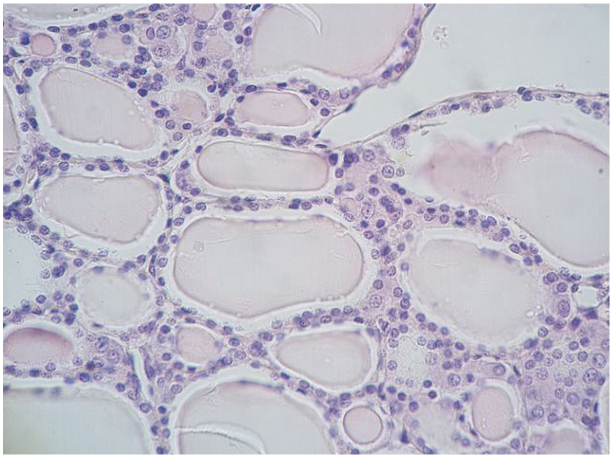

Fig. 2. Parafollicular cells in the wall of a follicle of the thyroid of a dog. Cajal method (Bussolati 2014; source: *Wikipedia*).

cytoplasm. Baber (1876, 1881) first described the cells (termed by Nonídez "parafollicular" cells (1932)) under the name of "parenchymatous" cells, in the thyroid of the dog. The findings of Baber were confirmed by Huerthle (1894), who termed them "grosse" or "protoplasmareichen Zellen". From the observations of Takagi (1922) their nature is undoubtedly secretory. The cells were seen to contain numerous granules in their cytoplasm, which produce the internal secretory activity that maintains the constant content of calcium in the blood. In a sense, Nonídez's approach emphasizes the context to understand the significance of competition.[10] We focused on the ways to discriminate priority

[10] As discussed by Harold Copp, in the first Gordon Conference on Teeth and Bones (1954), the medical problem how to determine with precision the physiological basis of the level of ionic calcium to be maintained was of critical importance. Copp reported on his discovery of calcitonin in 1962 (Copp 1962). In an explicit historical article he introduced his researchers to a small shark ("dogfish", *Squalus suckleyi*), thus providing support on the current view that calcitonin is produced in the thyroid of mammals and the ultimo-branchial glands of low vertebrates (Copp 1967; Copp *et al.* 1967).

However, Anthony G.E. Pearse (1916–2003) was convinced that calcitonin as a second thyroid hormone had to have its origin in a second type of cells. And after having assumed that the parafollicular cell (as a second endocrine-like cell type) might be the source of calcitonin, he proved it by immunocytochemical studies using anticalcitonin antisera. By the discovery of calcitonin, Pearse formulated his APUD in the case of procalcitonin, and the justification became linked to the development of J. Fernández Nonídez's ideas (Allchin 1994). Disclosed by a technique (the reduced silver nitrate method), more specific than the staining methods used by the early investigators, Nonídez's interpretation of the origin and the purpose of the parafollicular cells received a definite support (Nonídez 1932a; 1933). With the loss of time during the Francoist war, an account of the historical anomalies affecting the studies by J. F. Nonídez can be explained in part. The senior members of the scientific society retained his "rights" even in 1938, in particular to those experiments that had led to the location of the cardio-aortic chemo-receptors in the-aortic glomus tissue. There is no better example of this than Corneille Heymans's 1938 Nobel Lecture, where the Belgian Nobel laureate explicitly presents Nonídez's analysis (Heymans 1938). In 1939, Nonídez's writing for the Spanish journal published in Mexico, D.F. *Ciencia: Revista hispano-americana de Ciencias puras y aplicadas*, underlined the important participation of the Spanish histological school in the research on the anatomical base of the cardiovascular reflexes (Fig. 3). This was a study on the receptor areas which initiate the reflex cardiac acceleration that tributes Cajal "to whom we owe an irreproachable neurological technique" (Nonídez 1940). In this spirit, the ingenious experiments formulated in the study of the problem (especially by Heymans and his collaborators) made him admit that they constituted one of the most brilliant pages of the physiological experimentation. In the case of Heymans, Nonídez says that the Nobel was awarded to him "last year" (1938), i.e. as soon as he proved his solidarity with the scientists compelled to leave Spain. He did so by publishing this article related to his specialty, and written at Cornell University, when the first volume of *Ciencia* was published in México D.F. in 1940 (Giral 1994).

The visibility of Cajal's first preoccupation regarding the spread of histology to the entire biology (Marañón 1958) concerned the

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(Amine Precursor Uptake and Decarboxylation) hypothesis on the common origin in the embryonic neural crest for the cells producing peptide hormones. Despite the fact that the hypothesis lost his basis following extensive embryological investigations on the endocrinological cells – showing that pancreatic islet cells were not derived from the neural crest – Pearse's view has inspired today endocrine histology and pathology (by defining the diffuse neuroendocrine system, and multiple endocrine carcinomas) (Bussolati 2014).

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Fig. 3. With "The anatomical basis of reflex blood pressure regulation", Nonídez expanded upon his newly published in 1940s paper for the new republican journal *Ciencia: Revista hispano-americana de Ciencias puras y aplicadas*, a Spanish version of his most recent research on the Bainbridge's reflex. (Source: © 2018, Fundación Residencia de Estudiantes.)

experimental diabetes results achieved by the early Spanish biomedical research (Blázquez 2002). As seen in Fig. 4, the question of the scientific domination of one or another aspect (basic vs. clinical) mirrored the effects of calcium that are manifested in pancreatitis. The date distribution indicates that the academic response to the discovery of C cells, calcitonin and procalcitonin was symmetrical (Fig. 4). In spite of the fact that, in Cornell and Madrid respectively, Nonídez and Rodríguez Candela started their research in 1930 and 1932, but lasted until 1933 and 1975.

The suggestive idea of a conceptual shoehorn is conform with the spirit of a comparative method, in the frame of historical and sociological investigations. Once the political unity has been broken, the mind of a collective body is split in different communities. If supposedly they are rival academies, distortions and complicated factors are to be

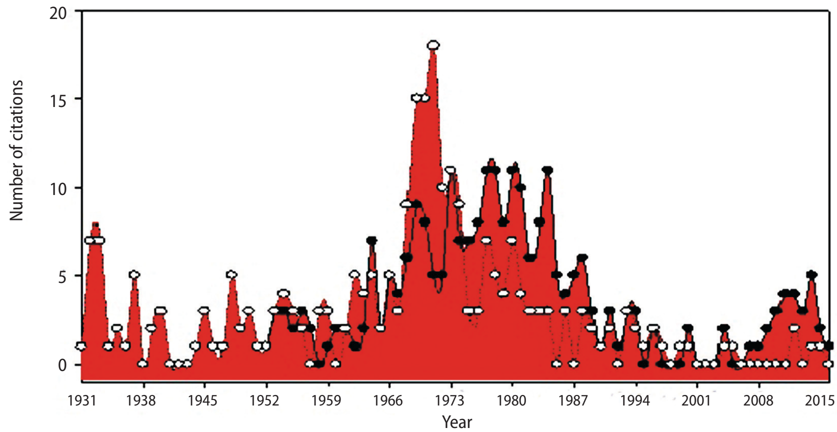

Fig. 4. Citation dates of the research literature using Nonídez's (1930–1933) (white dots) and Rodríguez Candela's (1932–1975) (black dots) ideas on calcium and PCT. (Source: Compilation based on the data mentioned above.)

detected on the base of statistics and at international level. It is easy to see (Fig. 4) that the starting points of the reflections of the two physicians were quite distinct. In 1930, while engaged in research into the anti--rachitic factor in avian rickets at the Medical College of Cornell University (N.Y.), Nonídez had first formulated his ideas (on dogs) on the existence of a parathyroid tissue located in thymus (a hypothesis that he was able to link with the existence of the parafollicular cells (Nonídez 1932a)).[11] From the early 1950s, it was Candela's turn to emerge with his experimental research into diabetes mellitus with tangible results. Going so far as to raise – in terms of citations – the properly sociological question of the captive Spain by Franco with the internal vision of endocrinology based primarily on clinical experiences. Indeed, as problems became redefined with the accumulation of knowledge Nonídez's work stands as a historically important discovery relevant to all areas of

[11] The anatomical connection between this tissue and the follicular cells made Nonídez certain that para-follicular cells had to exist. Again a fair amount of conceptual shoehorning clearly made the distance between Nonídez's discovery and the histological discovery in a clinical case that served to the characterization of calcitonin as a relevant ectopic hormone (the first demonstrated case of truly ectopic calcitonin production by a tumor was in a patient with a small cell carcinoma of the bronchus (Silva *et al.* 1973)).

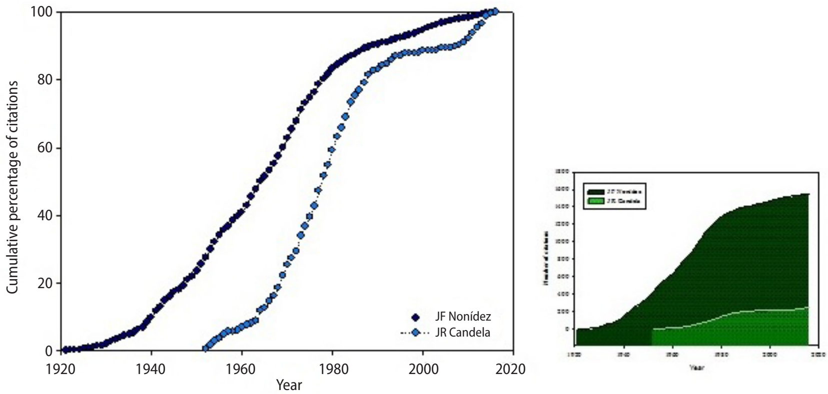

Fig. 5. In a line plot, the symbols (blue and cyan circles) show the cumulative percentage of the total citations received by Nonídez (1552) and Rodríguez Candela (248); for comparison, the citations of both physicians (dark vs. light green) are shown in the area plot on the right. (Source: Compilation based on the data mentioned above.)

biology and medicine and a basic research result. Thoroughly absorbed by a specialty that he had not founded, Rodríguez-Candela (former worker at the Cajal Institute (1934)) obtained outstanding experimental results regarding diabetes achieved as a promoter of biomedical research (Blázquez 2002).

A version of this approach is a point presented in Fig. 5. In accordance with the three fields where Nonídez gave his views from the USA (agricultural breeding research, C cells, anatomy of the cardiac reflex), the ideas raised by his articles are imbued with a "prophetic" quality eventually recognized along a span of 96 years. The realistic descriptions of diabetes mellitus by Rodríguez Candela accounted for the growth of biomedical science with a number of works as was called for by the Francoist authorities (Marañón 1958).

5. Professional science survival in top ranking research posts

Of the two Spanish scholars which reported the "parenchymatous cells" as a separate cellular entity (Nonídez) and the existence of the precursor of calcitonin (Candela), one did it in the first part of his career (1932), but the other did report on a precursor derived from chicken ultimobranchial glands as a measure of last resort (1975).

Consequently, the temporal discordance between the suggested existence of a precursor for calcitonin and of a second epithelial component of the thyroid might be the basis of a survival model of science and politics, when drawing on the endocrinological studies of the Spaniards. The discovery of the second epithelial of the thyroid gland had a strong focus on basic principles (and not on mere descriptive anatomy), but the range of conditions which the Spanish milieu offered in biomedical sciences had a very close relationship with the structure of the medical profession itself. In effect, only to a very limited extent was the laboratory tradition maintained in the biomedical sciences, and that could be explained because – in its organization – it followed the French model of the Centre National de la Recherche Scientifique (CNRS).

It appears that Nonídez pursued his (three) lines of research (1915––1929, 1930–1933, 1934–1947) throughout 32 years, in three temporal sequences that can be recognized from Fig. 6. In the 15 early years (1915–1929), working with agricultural breeding research; between 1930–1933 historiographically privileging a second thyroid epithelial as the site suited for the study of C cells, and over the last 15 years (1934––1947), forging crucial links at the base of the cardiovascular reflex.

By the 1950s, the work of Rodriguez-Candela evidenced that pancreas secretes glucagon. In fact, he investigated if glucagon could be considered an insulin antagonist, which itself made him become a part of

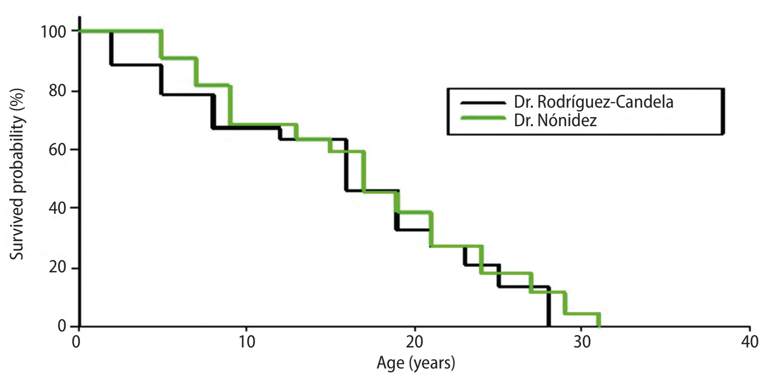

Fig. 6. Kaplan-Meier curves representing the distribution of the age intervals covered by the scholarship of the two authors during their life-time: Nonídez (1915–1929, 1930–1933, 1934–1947) (green line) and Rodríguez Candela (1932–1975) (black line). (Source: Compilation based on the data mentioned above.)

the CIBA colloquia. With this political progression, he took the direction that would lead him to get his work published in the *Perspective in Biology*, where Cori and Ochoa served on the editorial board at the time.

Candela devoted a major part of his research program to proving the physiologic effects of insulin (*in vivo* and *in vitro*). In full accord with observed facts, he subjected duck, rabbit and rat pancreas to a variety of metabolic procedures to determine insulin secretion, reasoning that the biochemical action of the hormone was concomitant with the secretion of ATP. Over a lifetime of work, he accumulated experience that beta-cells contain nucleotides that amplify the insulin secretory response. In connection with his work on insulin secretion by intracardiac glucose Candela's evidence that ATP-induced stimulation of insulin secretion remains a result of interest. In his clinical studies, Candela asserted that in pancreatic islets, when stimulated from exogenic ATP (by intravenous administration), the endocrine pancreas release insulin from beta-cells (Rodríguez-Candela *et al.* 1963). This experiment (extracellular ATP pancreatic beta-cell physiology) led to the launch of a new area of research known today as "Purinergic Signaling".

Rodriguez-Candela scientific program was an answer to the question of the "insulin secretion metabolism". His most remarkable research interests can be translated into three local institutional initiatives,[12] which can be sequenced in the following way (see Fig. 6): his first years, under Enríquez de Salamanca's (the physician of the royal household)

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[12] On 9 December 1943, the daily newspaper *ABC* announced Mr. Rodríguez-Candela's study tour in the United States. According to the same source, during those years he published communications of true importance and his travel was motivated by a desire for a further study, by dealing with nutritional and alimentary problems.

In February 1944, Candela began with one study comparing alloxanic and pancreoprivic diabetes at the Metabolism Department, headed by Professor L. Bauman, College of Physicians and Surgeons, Columbia University, and with the collaboration of Professors W. Palmer and A. Whipple, heads of the departments of medicine and surgery of the Columbia University. In April and September of that year he presented his results and communicated them at New York's Mount Sinai Hospital and at the College of Physicians and Surgeons of Columbia University.

In the Department, headed by José Luis Rodríguez Candela, where he worked with peptide hormones at the Instituto Nacional de Ciencias Médicas, and from the University where he was Professor of Pathology, the Cultural Relations Attaché at the US Embassy in Madrid was invited to Valladolid in March 1945.

direction at the Instituto Nacional de Ciencias Médicas (1942–1955); between 1956 and 1959 at the head of the Instituto de Metabolismo y Nutrición; and during the last 15 years at the top management of the Instituto Gregorio Marañón (1960–1975).

6. Canonical pathway for calcitonin and its precursor

The circulation of ideas between Nonídez and Rodríguez-Candela was motivated by their desire to communicate. But other social forces resulted in their concepts reaching recipients for whom they were not intended. To explain why, the collective of experiences involved in this circulation is described in terms of citation credit received (always related, in principle, to the creative capability of transformation of original concepts, which is one of the style-determining factors of thought). We derived the publication and the citation timelines for each of their research domains (Fig. 7).

At the center of the circle, the unveiling of C cells as an important source of calcitonin secretion. Anatomico-physiological data on the internal secretory activity of a second epithelial component of the thyroid gland modified the curriculum in endocrinology and the methods of diagnosis and treatment of endocrinological disorders. Also through pharmacological screening of the ATP effect on insulin secretion from rat pancreas, scientific work dealt first of all with problems of the pathogenesis, the clinical picture, and the treatment of diabetes mellitus. Also, the clinical management of medullar thyroid cancer patients is behind the biochemical interests in the secreted product of thyroid C cells, calcitonin, and the identification of its precursor.

Central to Nonídez's activities during 1930–1933 was a passionate description of the "parafollicular" argyrophilic cells. Immersed in that work from 1928, when he first provided advances on his work around the vascular innervation of the thyroid gland in the New York annual meeting of the Association for Research in Nervous and Mental Diseases, Nonídez provided the essential theoretical foundation for the study of parafollicular cells in the thyroid.

These cells have been promptly recognized and appreciated by comparative neurology as a valuable research tool. One year after Nonídez had associated Baber and Huerthle cells with his discovery (1933), researchers focused on experimental designs to examine the histological structure of the human thyroid gland. N. Goormaghtigh, the director of pathology at the University of Ghent, wrote: "We have come to the conclusion that the parafollicular cells (Nonídez) of small mammals are homologous to the small satellite follicles of the human thyroid" (Goormaghtigh, Thomas 1934, p. 727).

While Candela and his students successfully introduced the ATP mechanism of insulin release in the space between medical science and clinical practice in 1963, they also gradually constructed an argument for the phenomenon of heterogeneity of calcitonin (present in multiple heterogeneous forms in medullar thyroid carcinoma patients), which in 1975 became the research program that demonstrated that calcitonin is biosynthesized as part of a larger prohormone, procalcitonin.

These two scenes help to characterize a debate in science in 1975, bearing a striking resemblance to the political world in 1945: a qualitative change in the control of scientific work resulted in both cases in the situation where fascist scientists in Franco's Spain had no political unity. On Christmas 1945, Candela communicated for the first time in the medical press his experiments in which the pancreas is removed from an alloxan diabetic dog (Rodríguez-Candela 1945), and supported the hypothesis that the pancreas affects ketone body production through the elaboration of a hormone other than insulin. It was thus then when his first more prestigious experiment was born, whose methods would result in him being present on the pages of *Endocrinology*, the journal of the Endocrine Society (Washington, DC, USA), discussing subnormal activity of the complement one month before the death of Nonídez, on August 25, 1947 (Rodríguez-Candela *et al.* 1947).

Already in those days, the significance of the "parafollicular cells" in thyroid disease followed a unity of purpose that augured well for the future and that was immersed in the work Nonídez himself encouraged others to join in. In summary it can be said that in the summer of the year 1947, at the crossroads of C cells tissue and diabetes, the development of influence strategies and coverage of scientific competition meets the approach advocated by this analysis. We derived the citation timeline for each of the research domain.

So long as the sum of knowledge was similar between Candela and Nonídez, large percentages of citations were published for each research domain where they were credited. The maximum was 35% of the citations in the category anatomy and morphology; the minimum

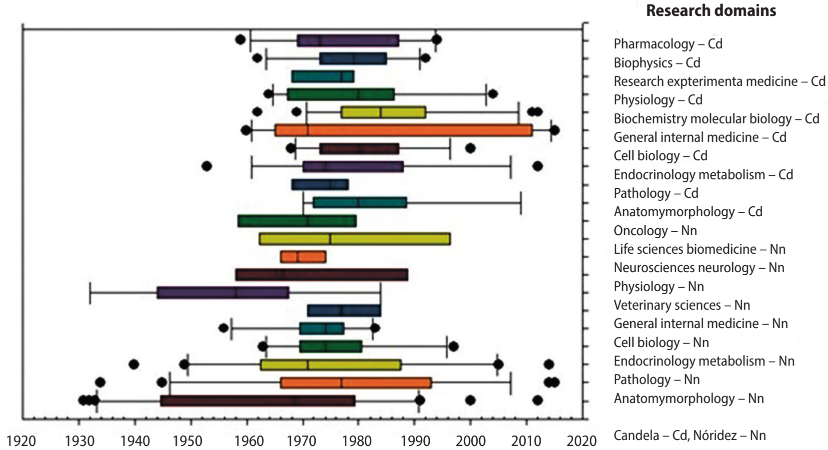

Fig. 7. Citation dates of Candela's and Nonídez's papers in the 15 research domains corresponding to the C cells researches (496 citations). As a source of technical illustrative material for an introduction to the basics of competition at the frontiers of their studies, a detailed analysis was performed on the 496 citations. The data were obtained for 5 articles published by Nonídez between 1931 and 1933, and for 53 articles published by Candela between 1947 and 1975. Box-plot representation: The horizontal line inside the box represents the median; the lower and upper borders of the box represent the 25th and 75th percentiles respectively; the whiskers correspond to the extension of 1.5 times of the box width from both ends of the box, and the circles represent values outside that interval. (Source: Compilation based on the data mentioned above.)

was 7% in the category oncology, both obtained by Nonídez. 24%, and 25% of the citations dealing with endocrinology and metabolism, were credited to Nonídez and Candela respectively. The median citation dates for the different research domains (see Fig. 7) differed significantly ($p<0.001$).

Interwar work on C cells by Nonídez formed an integral part of his consistent basic research career. Clinical work in Franco's Spain systematically advantaged Candela, particularly because he was at the head of medical investigation. On a practical level, their impact was similar concerning endocrinology. And as far as solid cancer with amyloidosis of the stroma arises from C cells, their influence existed on several levels; but in the total picture of contrasting controllability of life and fate, including political fate, it was certainly Nonídez's inheritance that was much more interesting and fruitful. Bearing in mind such a dominion in the medical research on the part of the professor of anatomy at

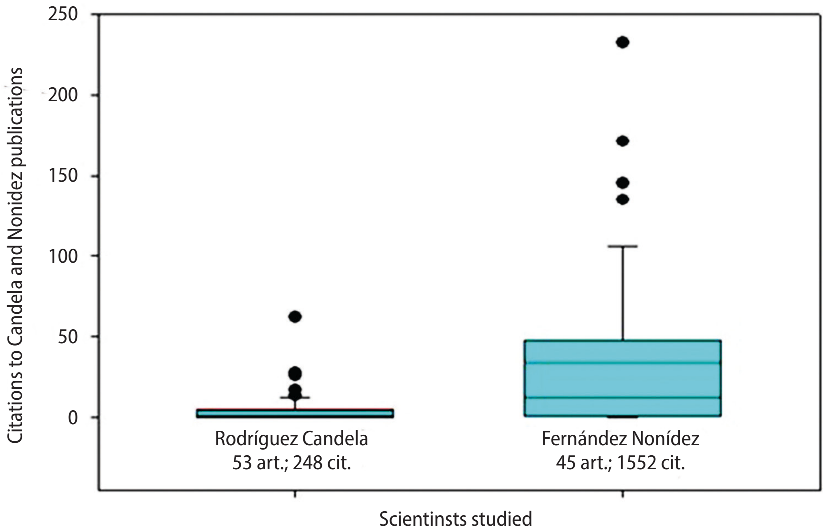

Fig. 8. Population sizes used in the citation studies on Rodríguez Candela and Fernández Nonídez (98 articles). The study population was composed of the 1800 citations received by 98 articles authored by Rodríguez Candela (53 art.; 248 cit.) and Fernández Nonídez (45 art.; 1552 cit.). The largest citations sample was 1552, obtained by Nonídez. The sum of citations is 1800 because the two citation studies analyzed the whole population of articles. Box-plot representation: as in Figure 7. (Source: Compilation based on the data mentioned above.)

Cornell, Fig. 8 reveals the depth of Nonídez's commitment to his chosen research themes, in connection with Candela's professional and intellectual echoes in the international press.

7. Conclusion

Nonídez came to terms with the errors of conceptual shoehorning reflecting on academic freedom by stressing how, as Fleck puts it, "we look with our own eyes but we see with the eyes of the collective". He was dissociated from the principles or interests of any religious faith, political school or political party and asserted the freedom and inviolability of science and the right of every teacher to an independent pursuit of knowledge without interference from any authority. But, on a practical level, the war of ideas in Spain coincided and fused with a common stock of knowledge in which self-interest and moral obligations are important.

In a way that makes sense of the certainty that para-follicular cells had to exist, Nonídez chemical methods and physiological procedures, in their historical singularity, provided the idea from which the finding of the precursor of calcitonin was constructed 40 years later (with the intervention of the clinician Candela). Keeping in mind that the theory that the C cells excrete the hormone actively into the blood stream was initially discarded, it is through its originality, in greater or smaller increments that Nonídez contribution was acknowledged. We have transferred that standard to the state of affairs of procalcitonin, by considering where the 'oxygen' the discoverers breathed was ultimately located. A test for competition in the absence of unity was designed. Dedicated to research in the medical sciences and animal biology, the presented findings have indicated that in the tradition of scientific research in Spain biomedical sciences maintained some degree of continuity.

While endocrine metabolism is primarily the area of competition between both parties, the impact of the discovery of C cells reflected and contributed to a theoretical conflict, the investigation of which was indeed a key element for the implementation of control policies in the hospital and in the general population; it spanned from medullar thyroid carcinoma (MTC) to the 1991 Gulf War burn patients. A large number of such investigations, reflecting the broader need of showing the presence of calcitonin on the basis of its clinical behavior, extended the common cytochemical, structural and functional characteristics that had been codified for the endocrine C cells to pathology.

Besides, the exile as an influential psychological factor made the Republican community appeared as active, competent, successful, its scientists having a journal ready for use at the moment of return, once the victory by the forces of freedom in World War II had been obtained. But these difficulties to translate results of animal experiments (Nonídez) to the human being (Candela) reveal collective moral (political) problems. At the end of the 1950s, the common use of new technology (electron microscopes) enabled building consensus around the specific characteristics of cytoplasmic granules. And in Spain, in the absence of a public debate, it was after the unconditional surrender of the last Axis power, in the year 1947, when Candela became a protagonist of this line of research (he published for the first time in an international journal in the same year, aged 39 in August 1947); the same year, at the age of 55, Nonídez died.

Biochemical and pharmacological phenomena associated with clinical studies were devoted to assessing real-time diagnostic tools and an immense amount of applied research work by Spanish hospitals. We are talking then of an inheritance that is to be estimated in terms of those students who were trained by Candela and learned medicine research by working with him in the hospital. The Higher Council of Scientific Research (CSIC) appeared to be a fruitful place for "bright boys" who had little financial support and who needed to earn some money in order to continue their academic career. Involving the traditions which surround biochemistry research and the structure of the medical profession itself, doctors have always had a very close relation with their patients, the result of this is that Candela had a "positive" image that attracted students to the center of a circle where insulin secretion, pancreatic islets, ATP release and prohormones like PCT combined enough mystery to motivate research with very careful clinical study making the discovery possible.

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Bibliometrics, science policy, scholarly communication

Bibliometria, polityka naukowa, komunikacja naukowa

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Wprowadzenie do tematyki sesji roboczej „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania" (Kraków, 25 czerwca 2019 r.) i konkretne propozycje rozwiązań organizacyjno-redakcyjnych i legislacyjnych

Abstrakt

W artykule przedstawiono wprowadzenie do tematyki sesji roboczej „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania", zorganizowanej przez Komisję Historii Nauki PAU w Krakowie w dniu 25 czerwca 2019 r., wraz z konkretnymi propozycjami

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rozwiązań organizacyjno-redakcyjnych dla czasopism i wydawnictw oraz rozwiązań legislacyjnych dotyczących zasad ewaluacji czasopism.

Słowa kluczowe: *sesja robocza, polskie czasopisma naukowe, polskie wydawnictwa naukowe, historia i filozofia nauki, naukoznawstwo, polski system ewaluacji czasopism naukowych, postulaty rozwiązań organizacyjno-redakcyjnych dla czasopism i wydawnictw, postulaty zmian legislacyjnych, Polska Akademia Umiejętności, Komisja Historii Nauki PAU*

An introduction to the topic of the Working Session "Polish scientific journals from disciplines: «history and philosophy of science» and «science of science» – current challenges" (Kraków, 25 June 2019) and specific proposals for organizational, editorial and legislative solutions

Abstract

The article presents an introduction to the topic of the Working Session "Polish scientific journals from disciplines: «history and philosophy of science» and «science of science» – current challenges" organized by the PAU Commission on the History of Science in Kraków on 25 June 25 2019, along with specific proposals of organizational and editorial solutions for journals and publishing houses, as well as of legislative solutions regarding the principles of journal evaluation.

Keywords: *working session, Polish scientific journals, Polish publishing houses, history and philosophy of science, science of science, Polish system of evaluation of scientific journals, postulates of organizational and editorial solutions, postulates of legislative changes, Polska Akademia Umiejętności (PAU), PAU Commission on the History of Science*

1. Kwestie podstawowe

Powodem zorganizowania sesji jest obowiązujące w Polsce ustawodawstwo dotyczące nauki i szkolnictwa wyższego (tzw. ustawa 2.0), w tym aktualny podział dyscyplin naukowych i aktualny mechanizm ewaluacji publikacji naukowych[1].

1.1. Aktualny podział dyscyplin naukowych

Przywołane w tytule dyscypliny *historia i filozofia nauki* oraz *naukoznawstwo* nie funkcjonują w polskim systemie dyscyplin naukowych, jako samodzielne dyscypliny naukowe. Nie mają też takiego statusu ani *historia nauki* (która jest poddyscypliną *historii*), ani *filozofia nauki* (to poddyscyplina *filozofii*), ani *historie poszczególnych dyscyplin* (to poddyscypliny poszczególnych *dyscyplin*).

Status *naukoznawstwa* – które powstało w Polsce w latach 1910–1930 za sprawą lwowsko-warszawskiej szkoły filozoficznej i Kasy im. Józefa Mianowskiego – jest jeszcze bardziej rozmyty, gdyż dyscyplina ta powstaje na skrzyżowaniu problematyki badawczej historii nauki (w tym historii organizacji instytucji naukowych), filozofii nauki, socjologii wiedzy naukowej, polityki naukowej, ekonomii nauki, komunikacji naukowej, scientometrii, bibliometrii etc.[2]

W systemie dyscyplin OECD, obok takich dyscyplin jak np. *historia*, *filozofia* oraz *etyka,* istnieje samodzielna dyscyplina *historia i filozofia nauki i techniki* (*history and philosophy of science and technology*). Należy ona do nauk humanistycznych, sekcja *filozofia, etyka i religia*, podsekcja *historia i filozofia nauki i techniki*. Ponadto historie poszczególnych dyscyplin zaliczane są do tychże dyscyplin. W systemie dyscyplin OECD nie ma jednak dyscypliny *naukoznawstwo* (*science of science*, *science studies*)[3].

Scopus i WoS klasyfikują czasopisma w zasadzie zgodnie z systemem dyscyplin OECD, dlatego w Scopus i WoS – obok czasopism zaliczanych np. do dyscyplin *historia*, *filozofia*, *etyka* – istnieją czasopisma

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[1] Zob. Kancelaria Sejmu 2018; MNiSW 2018a; 2018b; 2019b; OECD 2007.

[2] Zob. Kokowski 2015a; 2015e.

[3] W pierwotnym projekcie tzw. ustawy 2.0 przyjmowano system dyscyplin OECD (i był to jeden z ważniejszych argumentów przemawiających za przyjęciem ustawy 2.0) – zob. Kancelaria Sejmu 2017, Druk 2446-uzasadnienie, ss. 11–12.

zaliczane do dyscypliny *historia i filozofia nauki*[4], nie ma jednak czasopism zaliczanych do *naukoznawstwa*.

Tego typu sytuacja sprawia, że z dużym stopniem prawdopodobieństwa można założyć, iż problematyka czasopism zajmujących się dyscyplinami: *historia i filozofia nauki i techniki* oraz *naukoznawstwo*, a także problematyka kultury cytowania w tych dyscyplinach zostały zmarginalizowane podczas prac ministerialnych zespołów eksperckich ds. czasopism (zarówno ds. historii, filozofii, jak i wielu innych dyscyplin). Wynikiem tego może być zaniżona ocena punktowa czasopism z tych dyscyplin względem ocen punktowych czasopism zaliczanych do dyscypliny *historia* i dyscypliny *filozofia* oraz innych samodzielnych dyscyplin naukowych (bowiem o ocenie czasopism z dyscyplin *historia i filozofia nauki i techniki* oraz *naukoznawstwo* decydowały z równą wagą różne ministerialne zespoły ekspertów)[5].

Jednakże *z analiz eksperckich opartych na badaniu treści publikacji, a nie liczby cytowań publikacji* wiadomo, że czasopisma (także polskie) zaliczane do dyscyplin: *historia i filozofia nauki i techniki* oraz *naukoznawstwo* pełnią bardzo ważną rolę kulturotwórczą, nie wolno więc deprecjonować tej roli w polskim modelu ewaluacji nauki (poprzez zaniżanie punktacji takich czasopism) ani pomijać dorobku tych dyscyplin w rozwijaniu polskiego modelu ewaluacji nauki (a działo się tak dotąd)[6].

1.2. Podstawowe komponenty czasopisma

W racjonalnym rozwijaniu czasopism naukowych należy zwrócić uwagę na pięć podstawowych komponentów czasopisma:

1) treść czasopisma;
2) przyjęte w czasopiśmie rozwiązania techniczne (używanie DOI, ORCID oraz licencji wydawniczych CC; obecność w bazie SherpaRomeo, informującej o prawach wydawniczych i zasadach tzw. samoarchiwizowania czasopisma; współpraca z Crossref);
3) przyjęte przez czasopismo procedury recenzyjne (zgodność z zasadami COPE[7]);

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[4] W systemie dyscyplin OECD to *historia i filozofia nauki i techniki*.

[5] Zob. MNiSW 2019a.

[6] Kokowski 2015a; 2015d; 2015e.

[7] Zob. stronę internetową Committee on Publication Ethics – https://publicationethics.org.

4) obecność w bazach indeksacyjnych lub bibliotekach czasopism (Scopus, WoS, DOAJ, ERIH+, Index Copernicus International[8]) i na liście laureatów konkursu MNiSW „Wsparcie dla czasopism naukowych" (i osiągnięty wynik tego konkursu) etc.;
5) cytowalność czasopisma w różnych bazach (Scopus, WoS, Google Scholar, Index Copernicus International etc.).

1.3. Aktualny mechanizm ewaluacji czasopism naukowych

Mechanizm ten oparty jest na niekrytycznej ocenie bibliometrycznej publikacji naukowych i prymacie czasopism indeksowanych w Scopus i WoS, w tym niekrytycznym zliczaniu cytowań czasopism w tych bazach. W mechanizmie tym nie liczy się treść publikacji. W przyjętym przez MNiSW modelu punktowej oceny działalności naukowej pominięto także wycenę punktową prac redakcyjnych (administracyjnych, edytorskich, recenzyjnych, statystycznych, informatycznych), włącznie z rolą redaktora naukowego tomu czasopisma.

Obowiązujące w Polsce ustawodawstwo dotyczące ewaluacji dokonań naukowych ma bardzo poważną wadę, abstrahuje bowiem od wyników badań naukowych zintegrowanego naukoznawstwa, do którego zalicza się konglomerat dyscyplin: historia nauki (w tym historia organizacji instytucji naukowych), filozofia nauki, socjologia wiedzy naukowej, psychologia wiedzy naukowej, polityka naukowa, ekonomia nauki, komunikacja naukowa, naukometria, bibliometria etc. Wspomniana bibliometria jest tylko jednym z wymiarów naukoznawstwa i nie wolno go fetyszyzować – co od lat dzieje się w polskim systemie ewaluacji publikacji naukowych.

1.4. Niedostateczny stopień nasycenia baz indeksacyjnych Scopus i WoS

Badaczom, specjalizującym się w analizach bibliometrycznych, wiadomo m.in., że bazy Scopus i WoS w przypadku dyscyplin humanistycznych i społecznych w innych językach niż język angielski nie mają

[8] Por. wyniki ewaluacji polskich czasopism z historii nauki, historii, filozofii nauki oraz naukoznawstwa z lat 2014–2017 w polskiej bazie indeksacyjnej Index Copernicus International (nie powinno się ignorować tego typu danych w ocenie ministerialnej polskich czasopism) – zob. Kokowski 2019 (w czasie sesji nie były jeszcze znane wyniki ministerialnej ewaluacji czasopism z 3 lipca 2019 r.).

dostatecznego nasycenia publikacjami (czasopismami, a tym bardziej książkami).

Uznanym na świecie warunkiem używania baz indeksacyjnych w ewaluacji badań naukowych jest przekroczenie progu 50% nasycenia. W przypadku nauk humanistycznych i społecznych nasycenie baz Scopus i WoS jest jednak znacznie mniejsze. Dlatego właśnie cytowania w bazach Scopus i WoS nie powinny być używane jako główne narzędzie oceny prac naukowych.

W celu uniknięcia niedomówień w tym punkcie, należy dobrze rozumieć, czym jest nasycenie bazy indeksacyjnej publikacjami.

Zauważmy więc na wstępie, że publikacja indeksowana w danej bazie może cytować publikacje już indeksowane przez tę bazę, a także publikacje, które nie są indeksowane w tej bazie. Wiedząc to, przez nasycenie bazy indeksacyjnej rozumiemy iloraz liczby cytowanych publikacji indeksowanych w bazie indeksacyjnej i ogólnej liczby publikacji wymienianych w bibliografiach, tzn. iloraz liczby publikacji indeksowanych i liczby publikacji nieindeksowanych w danej bazie, przemnożony przez 100%.

W ewaluacji czasopisma w określonej bazie liczą się jedynie te cytowania, które pojawiają się w publikacjach (czasopismach, ewentualnie książkach) już indeksowanych przez tę bazę, a pominięte zostają cytowania publikacji, które nie są indeksowane w tej bazie, choć mogą być one bardzo istotne w sensie merytorycznym; pociąga to za sobą deprecjonowanie czasopism zajmujących się problematyką lokalną (np. polską) i opartą na źródłach archiwalnych (*gdyż nie są one indeksowane w bazach*).

Pomimo wskazanych problemów dotyczących kwestii cytowań w bazach indeksacyjnych, należy jednak sprawić przy pomocy przemyślanych działań organizacyjnych, by jak największa liczba czasopism była obecna w tych bazach, gdyż umożliwia to łatwiejsze odnalezienie publikacji w zasobach internetowych.

1.5. Patologie cytowań

Badaczom specjalizującym się w analizach naukoznawczych (w tym bibliometrycznych) wiadomo, że istnieje wiele bardzo poważnych nieprawidłowości – patologii cytowań. Zalicza się do nich m.in. kradzieże cytowań; brak właściwych cytowań; zjawisko niecytowania publikacji, np. polskich („bo z definicji są rzekomo «gorsze» od publikacji zagranicznych"); niewłaściwe, bardzo ułomne zliczanie cytowań przez bazy indeksacyjne (spowodowane brakiem lub wadliwością metadanych plików)[9].

1.6. Gdzie można i należy publikować – wskazanie redaktorów uznanych na świecie czasopism z historii nauki, techniki i medycyny

W kontekście deprecjonowania znaczenia nauki lokalnej, skupionej na analizie polskiej kultury, należy przypomnieć tezę głoszoną przez siedemdziesięciu czterech redaktorów reprezentujących pięćdziesiąt sześć sztandarowych czasopism na świecie z historii nauki, techniki oraz medycyny (czasopisma te są obecne w bazach Scopus, WoS, ERIH+ etc.). Brzmi ona następująco:

> Great research may be published anywhere and in any language. Truly groundbreaking work may be more likely to appear from marginal, dissident or unexpected sources, rather than from a well-established and entrenched mainstream (Cool et al. 2009, s. 2).
>
> Znakomite prace mogą być publikowane gdziekolwiek i w dowolnym języku. Jest bardziej prawdopodobne, że prawdziwie przełomowa praca pojawi się w ubocznych [peryferycznych], innych niż powszechnie przyjęte źródłach niż w rozwiniętym i ściśle ustalonym głównym nurcie badań [tłum. M.K., cytowane za: Kokowski 2015b, s. 176, tu dodano słowo „peryferycznych"].

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[9] Zob.: Kokowski 2015b, ss. 155–169; 2015c. Odnośnie do kwestii zliczania cytowań czasopisma *Studia Historiae Scientiarum* w czasopismach już indeksowanych w bazie Scopus, efektywność tej bazy wynosi aktualnie 21–27% (tzn. Scopus nie wie o istnieniu większości cytowań!); wiąże się to z bardzo niską jakością zapisu bibliografii w czasopismach cytujących *Studia Historiae Scientiarum* i bardzo niską jakością metadanych tych czasopism.

1.7. Rodzimy paradoks: Polskie specjalności naukowe a ocena polskich czasopism

> W tematyce związanej ściśle z polską kulturą (np. polonistyka, historia Polski, historia nauki polskiej) rodzime czasopisma, choć z reguły nie są indeksowane w bazach WoS i Scopus, są najważniejszymi tego typu na świecie (Kokowski 2015b, s. 177).

Nie jest to wcale dziwne, bowiem nigdzie indziej na świecie nie ma dostatecznie dużej liczby badaczy, którzy zajmują się taką problematyką. Co za tym idzie, tego typu polskie czasopisma powinny być wysoko oceniane w ewaluacji czasopism. Jest jednak inaczej:

> Mamy tu do czynienia z kuriozalną sytuacją. Konstytucja Rzeczypospolitej Polskiej i ustawa o języku polskim uważają kulturę polską za najwyższe dobro państwa – a obowiązujący model ewaluacji traktuje ją bezpardonowo jako coś gorszego (bo z definicji publikacje z tego zakresu zasługują na mniejszą liczbę punktów niż wytwory kultury uniwersalnej) (Kokowski 2015b, s. 177)[10].

1.8. Konkluzja

Ze względu na: a) niedostateczny stopień nasycenia baz indeksacyjnych Scopus i WoS w przypadku dyscyplin humanistycznych i społecznych oraz b) liczne patologie cytowań, w ocenie czasopism naukowych dyscyplin humanistycznych i społecznych należy skupiać uwagę *nie na cytowaniach*, lecz na analizie treści publikacji oraz przyjętych w czasopismach rozwiązaniach technicznych i procedurach recenzyjnych.

2. Kwestie pragmatyczne

Niezależnie od oceny aktualnego systemu ewaluacji polskich publikacji naukowych oraz czy zostanie w Polsce zmieniony system ewaluacji nauki, wszystkie polskie czasopisma naukowe – niezależnie czy zajmują się problematyką uniwersalną, globalną czy lokalną – podlegają rygorom

[10] Zob. Konstytucja Rzeczypospolitej Polskiej z dnia 2 kwietnia 1997 r.: art. 6.1; Ustawa z dnia 7 października 1999 r. o języku polskim, preambuła, art. 1.1 i 3.1.5.

aktualnego systemu ewaluacji, opartego na prymacie cytowań i to najlepiej cytowań widocznych w bazach Scopus i WoS. Rozwijając w Polsce pełnoprawne czasopismo naukowe, nie można abstrahować od tego fundamentalnego faktu legislacyjnego.

2.1. Przeciwdziałanie stosunkowo niskiej cytowalności polskich publikacji z zakresu nauk humanistycznych i społecznych

Mając na względzie fakt wysokiego statusu cytowalności w bazach Scopus i WoS w polskim systemie ewaluacji działalności naukowej, należy przyjąć do wiadomości, że aktualna cytowalność polskich publikacji z zakresu nauk humanistycznych i społecznych (w tym czasopism i książek z dyscyplin *historia i filozofia nauki i techniki* i *naukoznawstwo*) jest w tych bazach relatywnie niska i dla dobra polskiej nauki należy to jak najszybciej zmienić (*metodami zgodnymi z etyką i prawem*).

Chcąc zaradzić niskiej cytowalności polskich czasopism (także w bazach Scopus i WoS), należy jak najszybciej unowocześnić procedury wydawania publikacji naukowych w Polsce. Obowiązek ten spoczywa zarówno na polskich wydawnictwach naukowych i redakcjach czasopism naukowych, jak i na autorach publikacji.

Unowocześnienie to powinno polegać na:

a) wdrożeniu identyfikatorów DOI i ORCID;
b) otwarciu polskich publikacji na świat: chodzi tu o rozszerzenie tematyki czasopism także o problematykę zagraniczną i wydawanie czasopism także w językach kongresowych (nie istnieje bowiem „nauka lokalna" w stu procentach niezależna od „nauki globalnej" czy „nauki uniwersalnej") oraz wdrożenie przez wydawnictwa naukowe i redakcje czasopism naukowych idei otwartej nauki (w oparciu o licencje wydawnicze CC);
c) wdrożeniu przez wydawnictwa naukowe i redakcje czasopism naukowych idei rzetelnego cytowania (należy rzetelnie cytować wszystkie publikacje, jakie autor wykorzystuje w trakcie przygotowywania własnej publikacji; cytowanie musi zawierać komplet informacji bibliograficznych włącznie z identyfikatorem DOI, linkiem dostępu do pliku elektronicznego);
d) wdrożeniu przez wydawnictwa naukowe i redakcje czasopism naukowych zasad etyki wydawniczej COPE (Committee on Publication Ethics);

e) wdrożeniu przez wydawnictwa i redakcje czasopism naukowych procedury rzetelnego przygotowywania metadanych publikacji;
f) sprawieniu, by informacja o przyjętej w czasopiśmie tzw. polityce samoarchiwizowania umieszczona była w międzynarodowej bazie Sherpa-Romeo;
g) włączeniu jak największej liczby polskich czasopism do baz Scopus, WoS, DOAJ, Index Copernicus International, ERIH+;
h) uaktywnieniu autorów publikacji naukowych, by przestrzegali zasady rzetelnego cytowania publikacji i promowali swój dorobek w sieci internetowej (Researchgate, Academia.edu etc.).

2.2. Przeciwdziałanie deprecjonowaniu w Polsce pracy redaktorskiej

Chcąc zaradzić dotkliwemu deprecjonowaniu w Polsce pracy redaktorskiej (niemal z reguły pracy wykonywanej nieodpłatnie), należy ją właściwie dowartościować poprzez dobrze przemyślane akty legislacyjne.

Jeśli w modelu ewaluacji działalności naukowej utrzymana będzie idea oceny punktowej pracy naukowej, istnieje konieczność wprowadzenia punktacji za prace redakcyjne redaktorów czasopism naukowych (sekretarza, redaktorów działów, redaktora statystycznego, z-cy redaktora naczelnego, redaktora naczelnego), recenzentów wewnętrznych czasopism naukowych oraz recenzentów zewnętrznych czasopism naukowych[11]. Hierarchia punktacji uzależniona powinna być od rangi czasopisma mierzonej obecnością w Scopus, WoS, DOAJ, Index Copernicus, ERIH+ oraz ministerialnym programie WCN (Wsparcie Czasopism Naukowych). Ponadto fakt bycia redaktorem naukowym tomu czasopisma należy traktować na takich samych prawach, jak fakt bycia redaktorem naukowym monografii zbiorowej.

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[11] Należy przypomnieć, że zasady COPE zezwalają na wykonywanie zarówno tzw. recenzji wewnętrznych, jak i tzw. recenzji zewnętrznych.

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„Wykazy czasopism MNiSW 2017 i 2019", „ICI Journal Master List 2014–2017" a polskie czasopisma z historii nauki, historii, filozofii nauki oraz naukoznawstwa

Abstrakt

W artykule przedstawiono wyniki ewaluacji czasopism z historii nauki, historii, filozofii nauki oraz naukoznawstwa na podstawie „Wykazu czasopism MNiSW 2017", „Wykazu czasopism MNiSW 2019" oraz „ICI Journal Master List 2014–2017". Dodano także komentarz do tych wyników. Zwrócono uwagę na następujące fakty:

a) fakt istnienia ujemnej korelacji między oceną czasopisma w „Wykazie czasopism MNiSW 2019" a oceną czasopisma na „ICI Journal Master List 2014–2017" dla czasopism z historii i historii nauki;

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b) fakt, że obecność czasopisma w DOAJ nie podniosła oceny ministerialnej czasopisma;
c) fakt, że ocena czasopisma w bazie danych Scopus nie wpłynęła w znaczący sposób na wzrost oceny ministerialnej: ocena ta zależy od dyscypliny i subdyscypliny;
d) fakt, że czasopisma z listy programu ministerialnego „Wsparcie dla czasopism naukowych 2019–2020" (WCN 2019–2020) oraz ERIH+ otrzymały od 20 do 70 punktów; ich ministerialna ocena zależy od dyscypliny i subdyscypliny.

Ponadto wyrażono nadzieję, że dla dobra polskiej nauki w krótkim czasie usunięte zostaną pewne błędy „Wykazu czasopism MNiSW 2019", gdyż niektóre czasopisma otrzymały nieadekwatne oceny (stwierdzenie to wynika z porównania dokonań czasopism, w tym wskaźników bibliometrycznych).

Słowa kluczowe: *Wykaz czasopism MNiSW 2017 i 2019*, *ICI Journal Master List 2014–2017, polskie czasopisma, historia nauki, historia, filozofia nauki, naukoznawstwo, „Wsparcie dla czasopism naukowych 2019–2020" WCN, ERIH+, DOAJ, Scopus, Google Scholar, wskaźniki bibliometryczne*

"Lists of journals of the Ministry of Science and Higher Education in Poland 2017 & 2019", "ICI Journal Master List 2014–2017", and the Polish journals on the history of science, history, philosophy of science and science of science

Abstract

The article presents the results of the evaluation of the Polish journals from the history of science, history, philosophy of science, and science of science, based on the "List of journals of the Ministry of Science and Higher Education in Poland 2017 & 2019" and "ICI Journal Master List 2014–2017". A comment has also been added to these results. The following facts were noted:

a) the fact that there is a negative correlation between the journal's rating in the "List of journals MNiSW 2019" and the

journal's ratings in the "ICI Journal Master List 2014–2017" for journals from the history and history of science;

b) the fact that the presence of the journal in the DOAJ does not raise the ministerial rating of the journal;

c) the fact that the evaluation of the journal in the Scopus database has not significantly affected the increase in the ministerial rating: the rating depends on the discipline and sub-discipline;

d) the fact that journals from the ministerial program "Support for scientific journals 2019–2020" (WCN 2019–2020) and ERIH+ received 20 to 70 points; their ministerial ratings depend on discipline and sub-discipline.

In addition, it was hoped that for the good of Polish science, some errors of the "List of journals of the Ministry of Science and Higher Education in Poland 2019" would be removed in a short time, as some magazines received too low marks (this statement results from a comparison of journals' achievements, including bibliometric indicators).

Keywords: *List of journals of the Ministry of Science and Higher Education in Poland 2017 & 2019, ICI Journal Master List 2014–2017, Polish journals, history of science, history, philosophy of science, science of science, ERIH+, Google Scholar, Scopus, bibliometric indicators*

1. Tabele wyników ewaluacji wybranych czasopism

W zamieszczonych poniższej tabelach przedstawiono wyniki ewaluacji czasopism zajmujących się historią nauki i historią dyscyplin naukowych (w tym filozofii), a także *dyscyplinami pokrewnymi*: historią, filozofią nauki i socjologią wiedzy naukowej oraz informacją naukową, organizacją nauki i szkolnictwa wyższego; zebrano wyniki ewaluacji czasopism MNiSW z 31 lipca 2019 r. i 2017 r. (podając liczbę punktów i listę, na której wymieniane jest czasopismo), informację, czy czasopismo jest indeksowane w DOAJ, Scopus lub Web of Science oraz wyniki ewaluacji „ICI Journal Master List" („ICI") zarówno z 2017 r. (opublikowanej u końca 2018 r.), jak i z lat 2014–2016 (*dla ukazania dynamiki rozwoju czasopism*)[1].

[1] Z uwagi na to, że „ICI Journal Master List" jest przydatnym narzędziem do analizy ewolucji czasopisma, warunkiem uwzględnienia czasopisma w tym artykule była jego obecność na tej liście.

Tabela 1. Historia nauki i historia dyscyplin naukowych

CZASOPISMO	MNiSW 2019	MNiSW 2017	ICI 2017	ICI 2016	ICI 2015	ICI 2014
1. *Studia Historiae Scientiarum* / *Prace Komisji Historii Nauki PAU*[1]	20 WCN / 40 ERIH+	9 B; DOAJ*	100,00	82,72	65,56	64,29
2. *Organon*	20 ERIH+	10 C; SCOP.**	97,38	82,25	–	–
3. *Studia z Historii Filozofii*	20 ERIH+	9 B, DOAJ	93,25	73,28	83,28	66,17
4. *Antiquitates Mathematicae*	–	3 B	73,02	76,21	69,80	45,61
5. *Rocznik Historii Prasy Polskiej*	40 ERIH+	11 B	61,89	59,22	63,40	72,53
6. *Rozprawy z Dziejów Oświaty*	20 ERIH+	10 B	55,61	43,79	55,29	59,26
7. *Medycyna Nowożytna. Studia nad Kulturą Medyczną*	–	10 C	54,35	53,80	42,05	–
8. *Kwartalnik Historii Nauki i Techniki*	20 SCOP.***	10 C; SCOP.***	49,99	52,49	–	50,51
9. *Analecta. Studia i Materiały z Dziejów Nauki*	40 ERIH+	7 B	45,86	40,95	44,04	37,63
10. *Archiwum Historii i Filozofii Medycyny*	–	7 B	29,61	27,33	26,29	–
11. *Opuscula Musealia*	20 ERIH+	13 B	–	82,70	83,56	76,07
12. *Czasopismo Prawno-Historyczne*	40 ERIH+	9 B	–	54,25	43,98	–
13. *Archiwum Historii Filozofii i Myśli Społecznej*	20 ERIH+	10 C	–	–	47,60	–
14. *Rocznik Historii Filozofii Polskiej*	–	4 B	–	34,62	36,78	28,68

* Czasopismo *Studia Historiae Scientiarum* wydawane jest pod tą nazwą od 2016 roku; w latach 2009–2015 wydawano je pod nazwą *Prace Komisji Historii Nauki PAU*, a w latach 1999–2007 pod nazwą *Prace Komisji Historii Nauki. Polska Akademia Umiejętności*. Mimo to stara nazwa pojawiła się w „Wykazie czasopism MNiSW 2019". 27 marca 2019 r. czasopismo *Studia Historiae Scientiarum* zostało laureatem ministerialnego programu „Wsparcie dla czasopism naukowych 2019–2020" (nota 100 punktów na 100 możliwych), czasopismo zostało już zaakceptowane przez Scopus do włączenia do tej bazy; aktualnie (od 14 czerwca 2019 r.) trwają prace nad przygotowaniem umowy o indeksację czasopisma.

** Scopus posiada fragmentaryczne dane z lat: 1970–1971, 1974–1975, 1979–1980, 1997, 2002, 2004–2005, 2007–2008, 2012–2015; podana jest błędna nazwa obszaru tematycznego i kategorii: „medycyna, ogólna medycyna" – zob. Scopus i „medycyna (różne)" – zob. Scimago Journal Rankings.

*** Scopus posiada fragmentaryczne dane z lat 1970–1989, 1991–2001, 2003, 2005–2012, 2014–2015; podana jest błędna nazwa obszaru tematycznego i kategorii: „medycyna, ogólna medycyna" – zob. Scopus i „medycyna (różne)" – zob. Scimago Journal Rankings; w opisie czasopisma w Scimago Journal Rankings podana jest w języku polskim nieprawdziwa informacja, jakoby czasopismo to było jedynym polskim czasopismem publikującym systematycznie obszerne materiały z historii nauki (to skopiowana informacja z serwisu internetowego „Wielcy i więksi", bez zaznaczenia źródła informacji).

Tabela 2. Historia

CZASOPISMO	MNiSW 2019	MNiSW 2017	ICI 2017	ICI 2016	ICI 2015	ICI 2014
1. *Folia Historica Cracoviensia*	20 WCN	9 B, DOAJ	100,00	83,82;	70,64;	63,21
2. *Historia i Polityka*	20 WCN	10 B, DOAJ	100,00	80,57	81,86	55,10
3. *Biuletyn Polskiej Misji Historycznej*	20 WCN	7 B, DOAJ	93,92	88,01	83,45	68,33
4. *Studia Warmińskie*	40 WoS (ESCI)	10 B, WoS (ESCI)	90,77	69,85	71,17	63,01
5. *Zapiski Historyczne Poświęcone Historii Pomorza i Krajów Bałtyckich*	40 WCN	15 B, DOAJ	87,99	83,44	70,95	75,88
6. *Prace Historyczne. Zeszyty Naukowe Uniwersytetu Jagiellońskiego*	40 WCN	15 B	86,78	84,93	95,09	85,06
7. *Przegląd Nauk Historycznych*	–	10 B, DOAJ	86,37	–	58,60	48,73
8. *Res Historica*	70 WCN	12 C	77,91	67,80	77,71	72,69
9. *Przegląd Historyczno-Oświatowy*	20 ERIH+	10 C	73,80	58,14	–	–
10. *Przegląd Zachodni*	40 ERIH+	12 B	69,49	63,02	63,29	63,68
11. *Historyka. Studia Metodologiczne*	70 ERIH+	13 B	62,18	57,54	77,25	88,58
12. *Kwartalnik Historii Żydów*	70 SCOP.	10 B	57,87	46,18	51,26	–
13. *Przegląd Historyczny*	70 WCN	12 C	33,77	41,37	45,49	48,83
14. *Nasza Przeszłość. Studia z dziejów Kościoła…*	–	8 B	24,29	21,10	32,41	31,41
15. *Studia Historyczne*	–	12 B	–	–	70,87	–
16. *Wiadomości Historyczne z Wiedzą o Społeczeństwie*	–	10 B	–	–	23,77	–

Tabela 3. Filozofia nauki, socjologia wiedzy naukowej

CZASOPISMO	MNiSW 2019	MNiSW 2017	ICI 2017	ICI 2016	ICI 2015	ICI 2014
1. *Filozofia Nauki*	40 SCOP.; WoS	15 C; SCOP.; WoS	97,26	83,60	94,24	88,98
2. *Archiwum Filozofii Prawa i Filozofii Społecznej*	40 WCN	6 B	48,85	37,69	42,03	32,46
3. *Filozofia i Nauka. Studia Filozoficzne i Interdyscyplinarne*	20 ERIH+	4 B	42,24	–	58,08	–
4. *Studia Philosophica Wratislaviensia*	20 ERIH+	–	26,87	–	41,81	–
5. *Avant. Pismo Awangardy Filozoficzno-Naukowej*	40 SCOP., WoS (ESCI)	13 B; SCOP., WoS (ESCI)	–	–	–	88,25

Tabela 4. Informacja naukowa, organizacja nauki i szkolnictwa wyższego

CZASOPISMO	MNiSW 2019	MNiSW 2017	ICI 2017	ICI 2016	ICI 2015	ICI 2014
1. *Folia Toruniensia*	20 ERIH+	6 B	87,90	86,01	48,80	43,16
2. *Zagadnienia Informacji Naukowej. Studia Informacyjne*	20 WCN	8 B	67,35	–	82,86	74,50
3. *Nauka*	20 ERIH+	10 B	–	–	49,59	–
4. *Nauka Polska. Jej Potrzeby, Organizacja i Rozwój*	–	5 B	–	–	33,84	–

2. Komentarz

1) W „Wykazie czasopism MNiSW 2019" spośród wymienionych powyżej czasopism najwyżej oceniane są czasopisma z zakresu „historii" (średnia 43,33 pkt), następnie „filozofii nauki, socjologii wiedzy naukowej" (32,00 pkt), „historii nauki i historii dyscyplin naukowych" (26,00 pkt) oraz „informacja naukowej, organizacji nauki i szkolnictwa wyższego" (20,00 pkt).
2) Dla wymienionych powyżej czasopism fakt wysokiej oceny czasopisma w „ICI Journal Master List" nie wpłynął na zwiększenie punktacji czasopisma w „Wykazie czasopism MNiSW". W przypadku czasopism z „historii" i „historii nauki i historii dyscyplin naukowych" istnieje nawet ujemna korelacja, tzn. im wyższa ocena w „ICI", tym niższa ocena w „Wykazie czasopism MNiSW 2019" (patrz tabele 1 i szczególnie 2).
3) Dla wymienionych powyżej czasopism obecność czasopisma w bazie DOAJ (bardzo cenionej w „Planie S" Europe Science) nie wpłynęła na podwyższenie punktacji w „Wykazie czasopism MNiSW 2019" (nie jest zrozumiałe, dlaczego DOAJ nie jest uwzględniane przez MNiSW).
4) Dla wymienionych powyżej czasopism ewaluacja czasopisma w bazie Scopus i wyznaczone wskaźniki bibliometryczne w tej bazie nie wpłynęły w zasadniczy sposób na podwyższenie punktacji: punktacja ta zależy od dyscypliny i subdyscypliny.
5) W „Wykazie czasopism MNiSW 2019" czasopisma z listy WCN lub ERIH+ otrzymywały od 20 do 70 punktów; w przypadku rozważanych tu czasopism nie miało to zawsze związku z lepszymi wskaźnikami cytowalności w Google Scholar, notami uzyskanymi w konkursie WCN, obecnością tych czasopism w innych bazach etc.: ich ministerialna punktacja zależy od dyscypliny i subdyscypliny.

3. Konkluzja

Przedstawione fakty mogą zniechęcić do pracy redaktorów, którzy w *racjonalny sposób* zajmowali się dotąd w Polsce rozwijaniem czasopism naukowych, poświęcali temu zadaniu ogrom czasu oraz czynili to dodatkowo bez wynagrodzenia finansowego w imię wyższych zasad. Okazuje się bowiem, że niezależnie od obiektywnych osiągnięć czasopism, ich wyniki w ocenie ministerialnej mogą się okazać kwestią natury loteryjnej.

Miejmy jednak nadzieję, że dla dobra polskiej nauki tego typu błędy „Wykazu czasopism MNiSW 2019” zostaną w krótkim czasie usunięte.

Jeśli tak się nie stanie, doprowadzi to do bardzo poważnego osłabienia rangi polskich czasopism naukowych o zaniżonej punktacji. W szczególności nastąpi osłabienie rangi polskich czasopism z historii nauki względem polskich czasopism historycznych, niezajmujących się dotąd zawodowo badaniami z historii nauki, co wywoła odpływ publikacji od mniej punktowanych polskich czasopism z historii nauki ku wyżej punktowanym polskim czasopismom historycznym. Innymi słowy, zostanie wygenerowane patologiczne zjawisko „punktozy” w myśl nieetycznej zasady: „nieważne, gdzie się publikuje, ważne, za ile punktów”.

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Discussions, polemics, letters to the Editor

Dyskusje, polemiki, listy do Redakcji

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Komentarz do artykułu Mariusza W. Majewskiego opublikowanego w *Studia Historiae Scientiarum* 17 (2018), ss. 89–117

Abstrakt

Przedstawiono kilka uzupełniających uwag do tekstu Mariusza W. Majewskiego o Instytucie Metalurgii i Metaloznawstwa Politechniki Warszawskiej oraz o roli prof. Jana Czochralskiego, by pokazać, że temat nie został wyczerpany, a pewne sformułowania wymagają korekty.

Słowa kluczowe: *Jan Czochralski, Instytut Metalurgii i Metaloznawstwa, Politechnika Warszawska, Chemiczny Instytut Badawczy*

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Comments on the paper by Mariusz W. Majewski published in *Studia Historiae Scientiarum* 17 (2018), pp. 89–117

Abstract

Several remarks on the text by Mariusz W. Majewski devoted to the history of the Institute of Metallurgy and Metal Science at the Technical University of Warsaw, and on the role of Prof. Jan Czochralski, are presented. The aim was to show that the topic has not been exhausted, and some wordings need correction.

Keywords: *Jan Czochralski, Institute of Metallurgy and Metal Science, Warsaw University of Technology, Chemical Research Institute*

1. Wstęp

Bardzo ciekawa i cenna praca Mariusza W. Majewskiego pt. „Prace Instytutu Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej i Jan Czochralski”[1] jest chyba dopiero drugą pracą analizującą działalność Instytutu i część dorobku metaloznawczego prof. Jana Czochralskiego. Dotychczas jedyne poważne opracowanie napisał Zbigniew Tucholski na temat metalu B i jego dziejów[2]. Nie wspominam tu o pracach dotyczących krystalografii, a zwłaszcza metody Czochralskiego otrzymywania monokryształów, bo jest to dość dobrze opracowany temat. Wystarczy wspomnieć pracę pt. „Od wazeliny do krzemowej rewolucji”[3]. Zresztą cały dorobek dotyczący otrzymywania monokryształów

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[1] Majewski 2018.

[2] Warto zachęcić do lektury jedynego naukowego opracowania na ten temat pt. „Stop kolejowy [B]ahnmetall prof. Jana Czochralskiego i jego zastosowanie w kolejnictwie” (Tucholski 2014), a także „Stop kolejowy Bahnmetall (Metal B) profesora Jana Czochralskiego i jego militarne zastosowanie” (Tucholski 2015a). Skrócona wersja ukazała się na łamach *Biuletynu Roku Czochralskiego* (Tucholski 2015b; 2015c). Oczywiście warto przypomnieć sobie oryginalne prace prof. Jana Czochralskiego – „Nowoczesne kolejowe metale łożyskowe jako klasyczny przykład rozwiązania namiastkowania stopów cynowych” z *Przeglądu Mechanicznego* (Czochralski 1936) oraz „Kolejowy metal B – klasyczny przykład namiastki szlachetnych stopów cynowych” z *Hutnika* (Czochralski 1936a). Warto też zajrzeć do artykułu Czochralskiego pt. „Czy stosowanie metalu «B» jest gospodarczo wskazane” z *Przeglądu Mechanicznego* (Czochralski 1937).

[3] Tomaszewski 2016b; 2017.

metodą Czochralskiego jest dobrze znany specjalistom. Niestety, dorobek metaloznawczy Czochralskiego nie został praktycznie zbadany, a współczesne metaloznawstwo właściwie nie odnosi się do prac naszego bohatera. Nawet tzw. diagramy Czochralskiego były tak nazywane tylko przez krótki czas po wojnie. Jedynie A. Królikowski i J.R. Przygodzki dokonali krótkiego przeglądu metod badawczych opracowanych przez Czochralskiego[4]. To jednak za mało. Trzeba też pamiętać, że dotychczas nikt nie zajął się poważnie analizą dorobku Czochralskiego z okresu niemieckiego. Warto też zauważyć, że grozi nam ponowne zapomnienie dorobku Czochralskiego w związku z wymieraniem kadr znających zarówno ówczesny stan wiedzy metalograficznej, jak i język niemiecki. Nadal więc brakuje fachowego opracowania naukowego dorobku prof. Czochralskiego i jego Instytutu.

Omawiana praca tylko częściowo wypełnia lukę w naszej wiedzy o Janie Czochralskim. Wprawdzie udało się M.W. Majewskiemu odnaleźć sporo ciekawych i dotychczas nieznanych dokumentów na temat współpracy Jana Czochralskiego i jego Instytutu Metalurgii i Metaloznawstwa Politechniki Warszawskiej z wojskiem, ale opisana historia Instytutu oglądana jest jednak przez pryzmat sprawozdań, zeznań czy artykułów prasowych niechętnych Czochralskiemu (m.in. z procesów Czochralski–Broniewski).

Dokładniejsza lektura omawianej pracy przynosi jednak pewne rozczarowanie. Przede wszystkim, chociaż w artykule (w Bibliografii i w przypisie nr 4) cytowane jest jedyne dotąd tak obszerne opracowanie poświęcone tematyce prof. Czochralskiego, jakim jest książka *Powrót. Rzecz o Janie Czochralskim*[5], ze szkodą dla tego artykułu nie skonfrontowano jego treści z treścią tej książki.

Dlatego obecny komentarz stara się uzupełnić najważniejsze braki artykułu M.W. Majewskiego. Z konieczności uwagi należało podzielić na kilka kategorii: niedociągnięcia dotyczące zakresu tematycznego, źródeł historycznych oraz faktografii, informacje poza tematem pracy, niewłaściwie konstruowane przypisy, nieścisłości w słownictwie oraz błędy zapisu bibliograficznego.

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[4] Królikowski i Przygodzki 2013; 2014; 2014a.

[5] Tomaszewski 2012.

2. Niedociągnięcia dotyczące zakresu tematycznego, źródeł historycznych oraz faktografii

Praca M.W. Majewskiego analizuje działalność Instytutu Metalurgii i Metaloznawstwa (IMM), zorganizowanego i prowadzonego przez prof. Jana Czochralskiego, z punktu widzenia organizacyjnego i administracyjnego. Nie zajmuje się jednak konkretnymi osiągnięciami od strony naukowej, choć o nich wspomina. Trudno o to mieć pretensje do Autora, który nie jest metaloznawcą.

Zarówno tytuł, jak i pierwszy akapit publikacji sugerują jednak, że tematem jest działalność Instytutu i związek tych prac z rozwojem sił zbrojnych II RP. Tymczasem pewna część artykułu dotyczy także prac Chemicznego Instytutu Badawczego (ChIB) bez wyraźnego wydzielenia prac samego Czochralskiego i bez uwzględnienia ChIB w abstrakcie. Niestety, nie jest łatwo zorientować się, które osiągnięcia czy prace dotyczą IMM albo ChIB. O ile prof. Jan Czochralski, trzeci bohater publikacji, odpowiadał za całość Instytutu politechnicznego, to nie odpowiadał za całość prac w ChIB (tu miał tylko skromny Dział Metalurgiczny). Nie jest też jasne, jaka była odpowiedzialność Czochralskiego za prace prowadzone w niektórych częściach Instytutu Metalurgii i Metaloznawstwa; sam zawiadywał tylko jednym zakładem – Metalurgii i Metaloznawstwa[6].

Autor nie podjął próby rozdzielenia, o ile to w ogóle możliwe, prac wykonywanych w IMM i ChIB, i tych firmowanych przez Czochralskiego. Oczywiście szef odpowiada (jakoś) za całość, ale nie wiemy, ile z prac było bezpośrednio związanych z osobą Czochralskiego. Nie wszystkie publikacje miały współautorstwo Czochralskiego, np. praca Śmiałowskiego o mikrokalorymetrze[7].

Poza tym tytuł opracowania sugeruje, że Autor opisał prace naukowe i naukowo-techniczne Instytutu. Tymczasem o tych pracach dowiadujemy się zaledwie z krytycznych raportów różnych instytucji oraz ogólnikowych sprawozdań. Wprawdzie Autor broni się stwierdzeniem, że zabrakłoby miejsca, ale nawet nie wspomina o zawartości czasopisma *Wiadomości Instytutu Metalurgii i Metaloznawstwa oraz Zakładu Metalurgii i Metaloznawstwa Politechniki Warszawskiej* pokazującego dorobek naukowy (jawny) Instytutu i Zakładu. W publikacji brakuje np. analizy tych

[6] Tomaszewski 2012, s. 96.
[7] Śmiałowski 1936.

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prac. Przecież tylko część prac związana była z badaniami dla wojska. Jest to ważne, bo tytuł omawianego artykułu sugeruje, że dotyczy on wszystkich prac Instytutu, choć ogranicza się de facto do prac dla wojska. Warto pamiętać, że prace dla wojska były objęte tajemnicą wojskową, o czym wspominał m.in. prof. Walther Gerlach wizytujący Instytut niedługo przed wybuchem wojny[8].

Trudno więc mieć pretensje do Autora o brak analizy kompletu (!) ponad osiemdziesięciu zamówień na sprzęt, aparaturę naukową i odczynniki, jakie za pośrednictwem wojska (Instytut Techniczny Uzbrojenia) były składane przez Czochralskiego[9].

Kolejna uwaga pokazująca, jak potrzebna jest dogłębna analiza materiałów źródłowych, dotyczy procesów sądowych z lat 1934–1938 znanych jako „Czochralski contra Broniewski". Autor korzystał jedynie z wybranych siedmiu tekstów prasowych i to wyłącznie z *Gońca Warszawskiego* – gazety, która rozpętała nagonkę na Czochralskiego. Tymczasem proces był relacjonowany w co najmniej trzydziestu pięciu gazetach w całej Polsce. Niepełny, zapewne, zbiór obejmuje ponad sto dziesięć tekstów[10], w tym dziewiętnaście w samym tylko *Gońcu Warszawskim*. Jestem przekonany, że prawnik potrafiłby odtworzyć przebieg procesów, co stanowiłoby cenny materiał do analizy ówczesnej sytuacji (krótkie omówienie w książce *Powrót. Rzecz o Janie Czochralskim* jest tu niewystarczające[11]). Zagadnienie jest poważne, wobec *nieuprawnionej* sugestii Autora o naciskach władz na sąd (gdyż nie są dotąd znane żadne źródła historyczne, które to potwierdzają), i czeka na wnikliwe opracowanie.

Tu wystarczy w skrócie wspomnieć, że prof. Witold Broniewski w marcu 1933 r. wystosował pismo do Senatu i Rektora Politechniki Warszawskiej, a następnie zgłosił dziewięć zarzutów (nazwanych pytaniami) do Komisji Dyscyplinarnej dla Profesorów UW. Wobec negatywnej decyzji Komisji prof. Broniewski skierował sprawę do Sądu Grodzkiego (20 września 1934 r.). Po przegranym procesie w maju 1936 r. „uruchomił" prasę, czyli *Gońca Warszawskiego*. W odpowiedzi prof. Czochralski wytoczył sprawę przed Sądem Okręgowym. W październiku

[8] Tomaszewski 2012, ss. 142–143.
[9] WBH-CAW, Biuro Przemysłu Wojennego.
[10] Tomaszewski 2016a.
[11] Tomaszewski 2012, ss. 110–119.

1936 r. skazano Broniewskiego i jego wspólników z *Gońca Warszawskiego*. W styczniu 1937 r. Sąd Apelacyjny zatwierdził poprzedni wyrok. Z kolei Sąd Najwyższy uchylił wyrok. W grudniu 1938 r. zapadły ostateczne wyroki skazujące profesora Broniewskiego i redaktorów gazety[12].

Na s. 91 Autor pisze, że przyjazd do Warszawy (na stałe) nastąpił w kwietniu 1929 r., a na s. 93 mowa o uczestnictwie w spotkaniu w październiku 1928 r. W rzeczywistości Czochralski wraz z rodziną przyjechał do Polski w pierwszych dniach października 1928 r., natomiast w kwietniu 1929 r. objął posadę profesora na Wydziale Chemicznym Politechniki Warszawskiej[13]. Warto zauważyć, że już w końcu marca 1929 r. wygłosił referat na III Zjeździe Inżynierów Mechaników Polskich[14].

Są też w opracowaniu konstatacje bez poparcia w dokumentach. Taki charakter mają stwierdzenia na s. 91: a) „wiadomo także, że dalszy pobyt [Czochralskiego] w Niemczech, pomimo rezerw gotówki [...] nastąpił z pobudek osobistych", b) [Czochralski] zajmował się „organizowaniem *dorywczej* sprzedaży praw patentowych", c) „zapewne na podjęcie decyzji o emigracji wpłynęły nie tylko niedostatki materialne, ale także ograniczony dostęp do laboratoriów Metallurgische Gesellschaft AG"[15].

Problem adresu Instytutu – Autor pisze o dwóch siedzibach: przy ul. Topolowej 18 i al. Niepodległości 222 (s. 96, przypis nr 9). Ale w rzeczywistości jest to ten sam budynek i ten sam adres! Nowo budowana al. Niepodległości „wchłonęła" dawną ul. Topolową. Niestety, nie udało się ustalić, kiedy to nastąpiło[16].

Poniżej podaję dalsze zauważone niedociągnięcia i luki w wyjaśnieniu odpowiednich faktów.

- S. 93 – brak wyjaśnienia, co to były Zakłady Hohenlohego w Wełnowcu i gdzie dziś leży ta miejscowość (obecnie jest to dzielnica Katowic).

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[12] Patrz przyp. 11.

[13] Tomaszewski 2012, ss. 85, 95.

[14] *Ibidem*, s. 86.

[15] Wprawdzie w pracy przywołano moje prace (Tomaszewski 2012; 2013; 2014; 2015; 2016), ale w nich takich informacji nie ma.

[16] W Biurze Geodezji i Katastru Urzędu Miejskiego Warszawy nie ma żadnego dokumentu, który podawałby datę przemianowania ul. Topolowej. Inf. telefoniczna z 20 lutego 2019 r. (dop. – P.T.).

- S. 93 – przy opisie spotkania założycielskiego Komitetu Budowy Instytutu Metalurgicznego brak dopasowania nazwisk uczestników do firm, które reprezentowali. Wymienienie osobno firm i nazwisk niczego nie wyjaśnia. Brak też wyjaśnienia, czy i jaki był związek z Komitetem Budowy Gmachów Technologicznych[17].
- S. 94 – Autor pisze, że Dział Metalurgiczny ChIB wyodrębniono w 1935 r. Tymczasem dwie strony wcześniej w przypisie 6 podaje datę sprzed 1933 r. Nie wiadomo, która jest właściwa. Wiadomo natomiast, że Jan Czochralski rozpoczął organizację Działu Metalurgicznego (jako Działu V) od 1 maja 1929 r.[18]
- S. 97 – Autor pisze: „wraz z powołaniem Instytutu", ale nie podaje żadnej daty. Na poprzedniej stronie mowa jest tylko o budowie budynków dla Instytutu. A przecież praca M.W. Majewskiego dotyczy właśnie Instytutu. Niestety, nie wiem, kiedy formalnie powołano Instytut. Jego tworzeniem zajmował się Komitet Budowy Gmachów Technologicznych utworzony przez Politechnikę po 1924 r. i przekształcony w 1928 r. w Towarzystwo „Studium Technologiczne"[19]. Być może Instytut Metalurgii i Metaloznawstwa utworzono w 1933 r. po wykończeniu pierwszej części nowego budynku Technologii Chemicznej.
- S. 98 – Autor pisze, że „wyłoniono komitet", nie podając ani daty powstania, ani jego nazwy. Skład osobowy wskazuje na Komitet ds. Badań Metaloznawczych, ale data powołania Komitetu nie jest mi znana[20].
- S. 100 – Autor nie podaje, jakie to „niedokończone" ekspertyzy zostały odłożone (w IMM?) *ad acta*. Ważne jest też, kiedy to się stało. W dalszej części tego akapitu jest tylko enigmatyczne stwierdzenie „w tym czasie". Zresztą o procesie, a raczej serii procesów, Autor nic więcej nie mówi, choć obficie cytuje teksty prasowe na ten temat.
- S. 101 – przypis nr 18 nic nie mówi na temat odrzucenia wniosku Czochralskiego o dymisję – powinien być wcześniej.

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[17] Tomaszewski 2012, s. 96

[18] *Ibidem*, s. 106.

[19] Ibidem, s. 96.

[20] Ibidem, s. 131.

Z omawianego artykułu nie dowiemy się, w jakich dokumentach jest mowa o odrzuceniu dymisji.

- S. 103 – zapis w pierwszym akapicie sugeruje, że badania nad przebijalnością blach pancernych zakończono w 1936 r. Wiadomo jednak, że prace trwały nadal – np. praca doktorska o tej tematyce Jerzego Kaczyńskiego została zniszczona (jako tajna) we wrześniu 1939 r.[21]
- Ss. 110–111 – Autor pisze, że „środowisko naukowe wyraziło swoją dezaprobatę *żądając usunięcia* Czochralskiego z grona profesorskiego", nie podając jednak, kiedy i gdzie sformułowano takie *żądania* – przypis nr 26 nic o tym nie mówi.
- S. 111 – w przypisie nr 26 Autor wspomina o „krytycznych wspomnieniach w memuarystyce epoki", ale nie podaje żadnych konkretnych przykładów.
- S. 112 – brak pełnej nazwy i wyjaśnienia, co to była „Wspólnota Interesów" (właściwie: Wspólnota Interesów Górniczo-Hutniczych, czyli koncern górniczo-hutniczy istniejący w latach 1929––1939, z siedzibą w Katowicach). Z uwagi na to, że działalność w tej instytucji miałaby obciążać Czochralskiego, wątek ten powinien być bardziej rozwinięty przez Autora.

3. Informacje poza tematem

W omawianej pracy M.W. Majewskiego dostrzegam pewną nierównowagę w kompozycji tekstu. Z jednej strony mamy bardzo obszerne części i przypisy niezwiązane z głównym tematem artykułu, a z drugiej braki wyjaśnień istotnych problemów. Podaję poniżej trzy wybrane przykłady.

- S. 94 – obszerne akapity o dziejach Chemicznego Instytutu Badawczego powinny raczej znaleźć się w przypisach (np. w przypisie nr 6), a nie w głównym tekście.
- S. 101 – przypis nr 18 – dzieje Stacji Doświadczalnej Politechniki Lwowskiej, tak obszernie tu omówione, są poza zasadniczym tematem opracowania M.W. Majewskiego.
- S. 104 – przypis 20 wydaje się zbędny – dotyczy prac Warsztatów Samochodowych i Huty „Baildon" bez widocznego związku

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[21] Tomaszewski 2012, s. 105.

z pracami Czochralskiego. Zresztą akapit pierwszy na kolejnej stronie powinien znaleźć się (jeśli jest rzeczywiście potrzebny) w przypisach.

4. Niewłaściwie konstruowane przypisy

Sposób stosowania przypisów w artykule jest niezrozumiały – w wielu przypadkach odsyłacz pojawia się na końcu długiego akapitu i nie sposób zorientować się, czego dotyczy.

Autor podaje niektóre informacje bez poparcia w dokumentach. Pisze np. na s. 92 o „aktywnych działaniach prezydenta Mościckiego" oraz o tym, że to „promotorzy podjęli również starania mające na celu otrzymanie przez Czochralskiego samodzielnego stanowiska w [...] ChIB". Przywołany tu odsyłacz nic o tym nie mówi.

- S. 95 – przypis podaje informacje o Zakładach „Ursus", ale wstawiony jest na końcu akapitu o badaniach prowadzonych w ChIB. Powinien być wcześniej – na s. 94.
- S. 95 – wydaje się, że odsyłacz nr 8 jest nieporozumieniem – nie dotyczy ani prac Czochralskiego, ani działalności Instytutu. Zresztą, ostatnie dwa akapity zasadniczego tekstu na tej stronie (o budowie huty i pracach prof. Dominika) powinny być w przypisach, a nie w głównym tekście.
- S. 96 – przypis nr 9 – można mieć wątpliwości, czego dotyczy dokument przytoczony w tym przypisie – budowy Instytutu (tam się znajduje odsyłacz) czy struktury Instytutu opisanej w samym przypisie.
- S. 97 – przypis nr 10 powinien znajdować się dużo wcześniej – gdy w poprzednim akapicie pojawia się nazwa WIBInż, a nie po spisie członków komitetu.
- S. 100 – przypis nr 16 powinien być wcześniej – po nazwie Instytutu, a nie na końcu zdania (dotyczy przecież powstania Instytutu, a nie niewykonania polecenia ministra).
- S. 100 – akapit dotyczący zmiany nazwy Zakładów Skody S.A. powinien być w przypisie, a nie w tekście głównym, bo nie dotyczy Instytutu.
- S. 102 – przypis nr 19 powinien być wcześniej – przed zdaniem dotyczącym faktu sporadycznego zaproszenia przedstawicieli Instytutu IMM.

5. Nieścisłości w słownictwie

Wydaje się, że angielskie tłumaczenie tytułu, a właściwie nazwy Instytutu jest niepoprawne. Metaloznawstwo to po angielsku *Metal Science*, a nie *Metallurgical Science*. Poza tym liczba mnoga, *Sciences*, użyta w tytule odnosi się do obu elementów nazwy, co nie jest właściwe. Słowo *Science* dotyczy tylko metaloznawstwa, a nie metalurgii.

W wersji polskiej tytułu i tekstu Autor przyjął ciekawy zapis nazwy Instytutu: „Instytut *przy* Politechnice", co wydaje się rozwiązywać problem zagadkowego charakteru powiązań Instytutu z Politechniką Warszawską. Autor wspomina o urzędowych papierach z nazwą „Instytut Politechniki". Wydaje się jednak, że Politechnika nie miała żadnego wpływu na to, co się dzieje na terenie Instytutu, chociaż wybudowanego na terenie Politechniki. Instytutem rządziła Komisja, w skład której wchodziło dziewięciu oficerów i tylko jeden cywil – prof. Czochralski [22]. Vide – sprawa dymisji Czochralskiego (s. 101), o ile prawdziwa, załatwiana była przez organy spoza Politechniki!

Na Politechnice Warszawskiej działał Wydział *Chemiczny*, a nie Wydział *Chemii*. Ta ostatnia nazwa dotyczy wyłącznie wydziałów uniwersyteckich, politechniki stosują tę pierwszą nazwę.

Trudno się zgodzić na stosowanie terminu „*emigracja* z Niemiec" na określenie powrotu Czochralskiego do Polski (s. 91). Niemcy nie były ojczyzną Czochralskiego, co może sugerować takie sformułowanie. Podobnie nie mówimy o Polakach z Kresów, że po wojnie *emigrowali* z ZSRR.

Na s. 98 mamy niepotrzebny skrót myślowy, gdy mowa o „*takiej* przynależności", skoro chodzi tylko o Politechnikę Warszawską, a wymieniono także ChIB.

Na s. 104 mowa jest o „sformułowaniu *tezy o udarności* płyt". Poprawne zdanie powinno zawierać informację, o jakiej udarności mowa – wysokiej czy niskiej. Inaczej zdanie jest bez sensu.

Z kolei na s. 105 mamy niezręczne sformułowanie: „przełom lipca i września".

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[22] Tomaszewski 2012, s. 131.

6. Błędy zapisu bibliograficznego

- S. 98 – błędny przypis nr 11. Dokumenty o podanej sygnaturze są inaczej opisane w Bibliografii.
- Ss. 113–114 – w Bibliografii i w przypisie nr 12 pojawia się nazwa placówki w Londynie w formie błędnej: w przypadku Bibliografii – *Instytut Polski i Muzeum Sikorski,* a w przypadku przypisu nr 12 – *Instytut Polski i Muzeum Sikorskiego*; poprawna nazwa to *Instytut Polski i Muzeum im. gen. Sikorskiego.*
- S. 117 – błędny zakres stron (46–50) pracy Zbigniewa Tucholskiego z 2014 r. zamieszczonej w Bibliografii – powinno być: ss. 41–61.

* * *

Mimo wskazanych niedociągnięć i braków (na tym skoncentrowane były powyższe rozważania) praca M.W. Majewskiego jest jednak cennym przyczynkiem do dziejów Jana Czochralskiego, otwierającym nowe perspektywy badawcze.

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Odpowiedź na pracę dr. Pawła E. Tomaszewskiego pt. „Komentarz do artykułu Mariusza W. Majewskiego opublikowanego w *Studia Historiae Scientiarum* 17 (2018), ss. 89–117"

Abstrakt

Niniejszy artykuł jest odpowiedzią na polemiczny komentarz dr. Pawła E. Tomaszewskiego, opublikowany w bieżącym tomie czasopisma *Studia Historiae Scientiarum* 18 (2019), dotyczący wcześniejszego artykułu autora (M.W. Majewskiego) na temat prac Instytutu Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej oraz uzupełnień do biografii Jana Czochralskiego, opublikowanego w poprzednim tomie czasopisma.

Jest dobrze wiadomo, że każda próba opracowania historii jakiegokolwiek zagadnienia wymaga podjęcia krytyki źródeł

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historycznych i że pozyskanie informacji zgromadzonych w źródłach archiwalnych, prasie oraz memuarystyce wymaga od badacza podjęcia rzetelnej pracy porównawczej. Jednakże problem powstaje wówczas, gdy autor przyszłej publikacji dysponuje tylko nielicznymi źródłami, a nawet tylko jednym – tak było w przypadku badań autora (M.W. Majewskiego).

Autor udzielił odpowiedzi na nurtujące dr. Pawła E. Tomaszewskiego wątpliwości dotyczące finansowania budowy i wyposażenia Instytutu Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej oraz pochodzenia i prac Jana Czochralskiego. Przybliżono również problematykę Zakładów Hohenlohe SA, „Wspólnoty Interesów" i Wspólnoty Interesów Górniczo-Hutniczych SA.

Autor postuluje przeprowadzenie systematycznej zespołowej kwerendy zarówno prasoznawczej, jak i archiwalnej dotyczącej Jana Czochralskiego, a także udostępnienie badaczom archiwów prywatnych dotyczących tej postaci. Pozwoli to tworzyć bardziej ugruntowane źródłowo syntezy historyczne Jana Czochralskiego.

Słowa kluczowe: *źródła prasowe*, *źródła archiwalne*, *memuarystyka*, *krytyka źródeł*, *Korpus Kontrolerów MSWojsk.*, *przemysł na Górnym Śląsku*, *Jan Czochralski*, *patenty*, *Instytut Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej*, *Zakłady Hohenlohe SA*, *Wspólnota Interesów*, *Wspólnota Interesów Górniczo-Hutniczych SA*

Response to the work of Paweł E. Tomaszewski, PhD, entitled "Comments on the paper by Mariusz W. Majewski published in *Studia Historiae Scientiarum* 17 (2018), pp. 89–117"

Abstract

The article is a response to the polemical commentary by Paweł E. Tomaszewski, PhD, published in the current volume of the journal *Studia Historiae Scientiarum* 18 (2019), regarding the author's earlier article on the work of the Institute of Metallurgy and Metal Science at the Warsaw University of Technology with the addenda to the biography of Jan Czochralski, published in the previous volume of the journal.

It is well known that any attempt to compile the history of any issue requires critical approach to historical sources and that acquiring information collected in archival sources, the press and memoirs requires from the researcher a diligent and thorough comparative work. However, the problem arises when an author of a future publication has only few sources at his disposal, or even only one, which was the case here.

The article answers the doubts bothering Paweł E. Tomaszewski regarding the financing of the construction and equipment of the Institute of Metallurgy and Metal Science at the Warsaw University of Technology and the origin and work of Jan Czochralski. The issues of the enterprises Zakłady Hohenlohe SA, Wspólnota Interesów and Wspólnota Interesów Górniczo-Hutniczych SA were also discussed.

The author proposes that a systematic team inquiry into both press and archival resources regarding Jan Czochralski should be carried out, and researchers should be allowed access to respective private archives. This will allow for a more research-grounded historical syntheses of Jan Czochralski.

Keywords: *press sources*, *archival sources*, *memoirs*, *criticism of sources*, *Corps of Controllers of the Ministry of Military Affairs in Poland*, *industry in Upper Silesia*, *Jan Czochralski*, *patents*, *Institute of Metallurgy and Metal Science*, *Warsaw University of Technology, Zakłady Hohenlohe SA, Wspólnota Interesów, Wspólnota Interesów Górniczo-Hutniczych SA*

1. Źródła archiwalne a publicystyka dr. Pawła E. Tomaszewskiego

Wykorzystywane w pracach historycznych źródła to wynik mniejszych lub większych odkryć dokonywanych przez historyków, wywołanych splotem różnorodnych przyczyn i przypadków, zależnych od posiadanej wiedzy, pracowitości, wytrwałości, a także szczęścia w trakcie kwerend. Historyk dążyć powinien do maksymalnego wykorzystania źródeł, co w przypadku historii najnowszej napotyka szereg barier z powodu rozproszenia albo obfitości takich źródeł. Poszukiwania źródeł najczęściej rozpoczynamy już w domowym zaciszu, próbując odnaleźć wskazówki pomocne przy dalszych pracach w bibliotece, a następnie archiwum.

W przypadku analizowanego artykułu (Majewski 2018a) drogi wiodły nieco inaczej – najpierw w trakcie żmudnych kwerend odnalazłem materiały, które zakwalifikowałem do zupełnie innej publikacji. Przypadek zadecydował, że pod artykułem pióra Pana doktora, zamieszczonym na którymś z portali internetowych, zostawiłem komentarz, a następnie adres e-mail. Wkrótce otrzymałem *Biuletyn Roku Czochralskiego* (prowadzony przez dr. Tomaszewskiego) i dalszą korespondencję. Z upływem czasu przekazałem także swoje materiały archiwalne oraz wskazałem miejsca przechowywania innych. W zamian otrzymałem kopie artykułów *Gońca Warszawskiego* oraz maszynopis książki *Powrót. Rzecz o Janie Czochralskim*. Konsekwencją tych działań była propozycja napisania artykułu do *Biuletynu Roku Czochralskiego*. Zaproponowałem ze swojej strony wspólne prace nad artykułem, ale nie na łamach periodyku redagowanego przez dr. Tomaszewskiego, a w czasopiśmie naukowym. Z oferty tej ostatecznie niewiele wyszło, tym bardziej zaskoczony byłem brakiem dalszych wspólnych działań, zwłaszcza że w tym czasie przesłałem dr. Tomaszewskiemu fragmenty raportu Korpusu Kontrolerów MSWojsk., które następnie były kanwą mojego artykułu o Instytucie Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej.

Trudno już dziś domniemywać, dlaczego krytyczny dla Jana Czochralskiego raport spowodował regres w dalszej współpracy. Później zwyczajny przypadek sprawił, że w trakcie prowadzonych kwerend archiwalnych i bibliotecznych, poświęconych przemysłowi zbrojeniowemu w Drugiej Rzeczypospolitej, napotkałem kolejne źródła odnoszące się do początków działalności Instytutu Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej. Uzupełnieniem archiwaliów Centralnego Archiwum Wojskowego i Archiwum Państwowego w Katowicach były źródła wyszukane w Instytucie Polskim i Muzeum im. gen. Władysława Sikorskiego.

2. Źródła archiwalne w Instytucie Polskim i Muzeum im. gen. Władysława Sikorskiego

Dr Tadeusz Cyprian oraz świadkowie wydarzeń składający zeznania przed Komisją dla Rejestracji Faktów i Zbierania Dokumentów (następnie Komisją Powołaną w Związku z Wynikiem Kampanii Wojennej

1939), m.in. płk KK Witold Tyszkiewicz, mjr Tadeusz Biernacki, płk obs. Tytus Karpiński, mjr Stanisław Hyciak, odtwarzali przebieg wydarzeń najczęściej tylko z pamięci, bez dostępu do źródeł archiwalnych. Luki w pamięci, a także oskarżenia wysuwane pod adresem elit pokonanego przez Wehrmacht państwa nie mogą zaskakiwać. Pamiętajmy, że żołnierze armii gen. Władysława Sikorskiego we Francji, następnie Anglii byli częścią społeczeństwa skonfliktowanego znacznie wcześniej niźli przed wrześniem 1939 r. Dodatkowo doświadczyli traumy, w trakcie której świat, jaki znali i rozumieli, uległ całkowitej dezintegracji i bezprecedensowemu zniszczeniu.

Negatywny wydźwięk wypowiedzi wspomnianych powyżej osób dr Tomaszewski tłumaczy m.in. ich niechętnym stosunkiem do *establishmentu* Drugiej Rzeczypospolitej, w tym Jana Czochralskiego. Przede wszystkim zaś kwestionuje on wartość tego zasobu archiwalnego (Tomaszewski 2019, s. 519). Jednakże, z uwagi na to, że ów zasób ma niezależne potwierdzenie w dokumentach przechowywanych w Centralnym Archiwum Wojskowym i Archiwum Akt Nowych, nie jest to uzasadnione.

Należy ponadto zauważyć, że pełną nazwę Instytutu Polskiego i Muzeum im. gen. Władysława Sikorskiego uprościłem do Instytutu Polskiego i Muzeum Sikorskiego (IPMS) zgodnie z pisownią stosowaną przez wydawców i Naczelną Dyrekcję Archiwów Państwowych.

3. Zadania Korpusu Kontrolerów Ministerstwa Spraw Wojskowych

Podobnie dr Tomaszewski (2019, s. 520) bagatelizuje wnioski raportu Korpusu Kontrolerów MSWojsk, odnoszące się do błędów w zarządzaniu Instytutem Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej. Dlatego należy nadmienić, że skupiał on nieliczny zespół oficerów starszych, począwszy od majorów, na generałach dywizji skończywszy, posiadających wykształcenie prawnicze, ekonomiczne lub techniczne. Oficerowie Korpusu Kontrolerów ponosili odpowiedzialność służbową bezpośrednio przed ministrem spraw wojskowych i jego zastępcami. Spoczywała na nich drobiazgowa wręcz odpowiedzialność za grosz publiczny, a ponadto przestrzeganie regulaminów, rozporządzeń oraz ustaw władz zwierzchnich. Nie ma potrzeby uzasadniać, że prowadzone przez ten zespół kontrole w instytucjach państwowych oraz prywatnych, współpracujących z organami wojska, piętnowały wszelkie nieprawidłowości finansowe i organizacyjne. Kolejną kwestią wartą podkreślenia pozostaje ocena wiarygodności raportów Korpusu Kontrolerów MSWojsk. W tym bowiem przypadku wnioski, które były przedstawiane zwierzchnikom sił zbrojnych, opracowywał personel merytoryczny korpusu. Notabene podobne zadania realizowała Prokuratoria Generalna Rzeczypospolitej Polskiej oraz Urzędy Delegata Prokuratorii Generalnej w Katowicach i Wolnym Mieście Gdańsku, reprezentując interesy Skarbu Państwa i współpracując również z Najwyższą Izbą Kontroli (Państwa)[1].

4. Procesy Czochralski – Broniewski na łamach prasy

Kolejną kwestią wymagającą komentarza pozostaje wykorzystanie artykułów prasowych dotyczących procesów o zniesławienie pomiędzy Broniewskim i Czochralskim (Tomaszewski 2019, ss. 521–522). Kwestie tych procesów analizowałem na podstawie artykułów zamieszczonych na łamach *Gońca Warszawskiego*.

Nieuprawniona jest teza dr. Tomaszewskiego o rozpoczęciu przez redakcję tej gazety „nagonki na Czochralskiego". Wręcz odwrotnie, zespół dziennikarzy postawił sobie inne zadanie: rzetelne informowanie opinii publicznej o przebiegu procesu. Przegrana w pierwszym procesie Broniewskiego spowodowała, że zespół redakcyjny wystąpił w odmiennej roli. Nie tylko będzie w dalszym ciągu skrzętnie relacjonował przebieg procesu, ale także przedstawiał nowe dowody, powołując świadków oraz zadając na tej podstawie niewygodne dla Czochralskiego, jak i ówczesnych elit, pytania. Przyjęte funkcje miały jeden podstawowy cel – obronę za wszelką cenę prof. Broniewskiego. Należy jednak zauważyć, że poza usunięciem dwóch artykułów zawierających relację z procesu Czochralski – Broniewski oraz „listu" tego ostatniego, po których pozostały tylko puste miejsca w *Robotniku* i *Gazecie Warszawskiej*, cenzura prewencyjna, z której słynęła Druga Rzeczpospolita, na ogół rezygnowała ze swych ponurych powinności[2].

Zgadzam się z dr. Tomaszewskim, że w przypadku omówienia procesów o zniesławienie pomiędzy Broniewskim i Czochralskim można było w artykule wykorzystać lepsze i bardziej zorientowane źródło

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[1] Styk 1978, ss. 237–246; Marszałek 2011, ss. 147, 210–214.
[2] Zawadzki 1936, s. 5.

prasowe. Te, które jednak zostały wybrane, są wartościowymi źródłami prasowymi, gdyż opisują dostatecznie szerokie spektrum poruszanych problemów, a zeznania świadków przytaczano dosłownie.

5. Uwagi o wymiarze sprawiedliwości w Drugiej Rzeczypospolitej

Baczną uwagę dr. Tomaszewskiego (2019, s. 521) wzbudziła sugestia autora (M.W. Majewskiego) dotycząca nacisków na wymiar sprawiedliwości. Nie zagłębiając się zbytnio w tę problematykę, odpowiedź będzie ze wszech miar pozytywna. Przedstawiciele wymiaru sprawiedliwości (sędziowie, woźni, komornicy, prokuratorzy, asesorzy i urzędnicy kancelaryjni) należeli do słabo opłacanych sektorów sfery budżetowej w Drugiej Rzeczypospolitej. Możliwości awansu, wyrażenia zgody na podjęcie dodatkowej pracy, przyznania dodatków i remuneracji powodowały, że system ten można określić mianem korupcjogennego. W naszej historiografii scharakteryzowano liczne przypadki ferowania wyroków sądowych nie tylko przed obliczem Temidy, ale również w zaciszu gabinetów polityków oraz Sejmu. Wady tego systemu najłatwiej można przeanalizować na podstawie działań obozu pomajowego. Na łamach prasy prawniczej, jak również codziennej, tok postępowania władz sądowych, nie tylko w najgłośniejszych procesach sądowych, ale również pomniejszych, może zaskakiwać. W trakcie owych analiz łatwo dostrzec paradoksalnie niskie wyroki za przestępstwa zagrożone karą długoletniego pobytu w więzieniu lub karą śmierci, i odwrotnie – niewspółmiernie wysokie kary za podobne przestępstwa[3].

6. Dalsze badania nad Instytutem Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej oraz Działem Metalurgicznym Chemicznego Instytutu Badawczego

Z dużą dozą prawdopodobieństwa można stwierdzić, że zasób archiwalny, pozostający zarówno w gestii władz państwa, instytucji kościelnych, jak i szczególnie prywatnych, na który zwrócił uwagę dr Tomaszewski (2019) umożliwia podjęcie dalszych badań dotyczących Instytutu

[3] Materniak-Pawłowska 2011, ss. 63–110; Krzyżanowski 2011, ss. 261–289, 368–396.

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Dla historyków prowadzących prace nad jakimkolwiek tematem niezmiernie ważne jest poznanie odpowiedniej bazy źródłowej, a następnie ujawnienie jej innym badaczom. Bez udostępnienia tej bazy źródłowej wysiłki kolejnego badacza będą znacznie utrudnione, a interpretacje czasami odmienne. Ze zrozumiałych względów szczególnie trudna jest sytuacja historyków rozpoczynających dopiero prace.

W trakcie moich prac parokrotnie spotykałem się z Autorami, którzy umieszczali w przypisach zdawkowe informacje na temat wykorzystanych dokumentów (np. Tomaszewski). Czasami była to tylko nazwa archiwum, zespół lub sygnatura. W innych wypadkach powoływano się na archiwum domowe Autora. Podobnie było w trakcie prac nad monografią przemysłu motoryzacyjnego w Polsce (1918–1939), gdy odczuwałem znaczny niedosyt źródeł archiwalnych, dotyczący dziejów Zakładów Mechanicznych „Ursus"[4]. Ponieważ spuścizna archiwalna ma charakter znikomy, zwróciłem się do Autorów poruszających w swojej publicystyce tę arcyciekawą tematykę[5] z prośbą o wskazanie miejsca przechowywania notatek i korespondencji dyrektora Fabryki Metalurgicznej „Ursus" prof. dr. Kazimierza Gierdziejewskiego (1926–1938). Po uzyskaniu odpowiednich informacji udałem się do Archiwum Instytutu Odlewnictwa w Krakowie, w którym prof. dr Kazimierz Gierdziejewski pełnił funkcję dyrektora (1947–1952). Otrzymałem tam informację, że materiały te zostały przekazane do Archiwum Narodowego w Krakowie, Oddziału IV z siedzibą w Spytkowicach. Niestety, skromna spuścizna dotycząca w większości okresu okupacji oraz przede wszystkim PRL pozwoliła pozyskać jedynie znikomą ilość informacji odnoszących się do prac prof. dr. Kazimierza Gierdziejewskiego w „Ursusie".

Ujawnienie archiwum Gierdziejewskiego, które prawdopodobnie zawiera korespondencję na temat Czochralskiego, mogłoby mieć kapitalne znaczenie dla historyków zajmujących się dziejami lotnictwa i motoryzacji w Polsce, z kilku zasadniczych powodów. Fabryka Metalurgiczna „Ursus" współpracowała z Czochralskim (w 1931 r.), a następnie

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[4] Zakłady Mechaniczne „Ursus" włączono do holdingu Państwowych Zakładów Inżynierii od 1 października 1931 r. na podstawie decyzji Komitetu Ekonomicznego Ministrów. Zob. Majewski 2016, ss. 101–104.

[5] Tomaszewski 2016, s. 399.

z IMM (w 1933 r.), początkowo przy wdrożeniu produkcji stopu B, a następnie stopów aluminiowo-magnezowo-tytanowych i miedziano-niklowo-magnezowo-tytanowo-krzemowo-żelaznych[6]. Wprowadzenie tych stopów odlewniczych (hidumin i termodyn) umożliwiało odlewanie, a następnie kucie m.in. korbowodów, głowic i tłoków. Patent J. Czochralskiego i A. Pacza zakupił m.in. Rolls-Royce, i po ustaleniu właściwych proporcji pierwiastków odstąpił High Duty Alloys. Za pośrednictwem tej ostatniej firmy Zakłady Metalurgiczne „Ursus" nabyły opracowania patentowe na stopy R56, R59, wykorzystywane do odlewania części silników licencyjnych Bristol, produkowanych dla Polskich Zakładów Skody SA, a następnie Państwowych Zakładów Lotniczych Wytwórnia Silników. Paradoks polega na tym, że w 1931 r. Czochralski wraz ze stopem B odstąpił równocześnie prawa patentowe termodynu[7]. Należy mieć nadzieję, że odnalezienie archiwum prof. dr. Kazimierza Gierdziejewskiego umożliwi odpowiedź na nurtujące pytania.

7. Literatura wspomnieniowa i Jan Czochralski

Zarówno w przypadku dokumentów archiwalnych, jak i pozostałych publikacji, w tym memuarystyki, przeprowadzam krytykę źródeł, albowiem pamiętam, że jest to podstawowa powinność historyka. Odpowiadając na uwagi dotyczące „braku konkretnych przykładów" (Tomaszewski 2019, s. 524), należy zauważyć, że obydwa pamiętniki wskazałem w przypisach (a źródła te są dostępne w znakomitej większości bibliotek).

Wyszukane opinie napisane zostały przez piłsudczyków, trudno zatem podejrzewać Autorów tych wspomnień o bezpodstawną krytykę Jana Czochralskiego. Obydwa osądy należą jednak do źródeł wywołanych (*in situ*). W przypadku gen. bryg. Kordiana Józefa Zamorskiego autorem pierwszej części spostrzeżeń jest kapitan Tadeusz Biernacki, a drugiej – odnoszącej się do forsowanych przez Czochralskiego materiałów wykorzystanych przy produkcji schronów – gen. Zamorski, który był świadkiem owych wydarzeń. W nieco innej konfiguracji pozostawił nam swoją opinię wiceminister wyznań religijnych i oświecenia publicznego

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6 Szczawiński 1931, ss. 333–339.

7 Instytut Polski i Muzeum im. gen. Władysława Sikorskiego (dalej IPMS), Dowództwo Lotnictwa, sygn. Lot A1/24/i/e, k. 1. Pismo Nikodema Dudzińskiego do gen. bryg. pil. Stanisława Ujejskiego z 4 VI 1941 r.

ks. prof. dr Bronisław Żongołłowicz. Powoływał się na zapis rozmowy z prof. dr. hab. Władysławem Taklińskim. Druga natomiast z opinii, dotycząca prof. dr. hab. Witolda Broniewskiego, stanowi wynik bezpośrednich i wieloletnich obserwacji.

Obydwie opinie gen. Zamorskiego i ks. prof. Żongołłowicza wywołują obfitość spostrzeżeń i skrajnych refleksji. Ponieważ źródeł tych nie wykorzystał dr Paweł E. Tomaszewski w swoich opracowaniach, wskazałem na obydwa. Ponieważ zarzucił mi także brak przykładów, przytaczam obydwie *in extenso*.

2/1[19]35

> [b]ył kpt. Biernacki z 35. P[ułku] P[iechoty], znakomity znawca stali i jej produkcji, specjalnie płyt stalowych. Jest to czerwona płachta na [Tadeusza] Kossakowskiego, którego on uważa za ostatniego drania i wyraża się o nim z pogardą. Prezydent Warszawy [Stefan] Starzyński chce go dostać z wojska na dyrektora gazowni miejskiej. [Stanisław] Surzycki z Huty Baildon chciał mu dać 250 000 zł za wstąpienie do ich zakładów. Chłopak siedzi w wojsku, z którego za ostatni wynalazek produkcji płyt stalowych lepszych niż je robi Vickers, nic nie dostał. Nie chce się jednak dać eksploatować, zwłaszcza że nie ma żadnych wobec wojska zobowiązań z tytułu studiów, na które nie miał żadnej pomocy, nawet w postaci urlopu. Pełnił służbę i chodził na wykłady do politechniki. Twierdzi, że zgłoszony przez Kossakowskiego i [Ottona] Czuruka za znakomitość w dziedzinie stali profesor [Jan] Czochralski jest dureń, a może i drań, przy czym w dziedzinie stopów aluminiowych jest znakomitym fachowcem. Był fakt, kiedy tenże Czochralski prowadził kwestię produkcji w Ostrowcu [Świętokrzyskim] kopuł pancernych ze stali węglistych mimo gwałtownych sprzeciwów Biernackiego. Dopiero ten musiał działać przez gen. [Józefa] Zająca i [Leona] Berbeckiego, by przeforsować produkcję tych kopuł ze stali chromowej. Późniejsze próby z działaniem trotylu wykazały całkowitą słuszność twierdzeń Biernackiego [...][8]

[8] Zamorski 2011, s. 305.

Ks. prof. dr Bronisław Żongołłowicz, poza przytoczeniem opinii prof. dr. hab. Władysława Taklińskiego dotyczącej Czochralskiego, sporo miejsca poświęcił także prof. dr. hab. Witoldowi Broniewskiemu. Miał ku temu również więcej okoliczności. Ich kanwą były, jak się później okazało, bezpodstawne zarzuty formułowane przez profesorów Michała Broszko oraz Witolda Broniewskiego pod adresem dziekana Politechniki Warszawskiej prof. zw. Stanisława Zwierzchowskiego. Poczynione obserwacje umożliwiły Żongołłowiczowi następującą charakterystykę osoby profesora Broniewskiego:

> 21. I. poniedziałek. [1935] Rektor Tokliński bardzo ujemnie dziś charakteryzował prof. Czochralskiego (Politechnika Warszawska), jako Niemca nieuka, który zarzucał Broniewskiemu „zdradę stanu" za co dostał w twarz od Broniewskiego [...] Sprawa się jeszcze toczy [...] Takliński nie chce iść do Wacka, powiada „nie chcę, ja go nie znam, po co on mnie?" [...][9]
>
> 15. X. wtorek. [1935]. Prof. Broniewski chce koniecznie mieć dyscyplinarkę w sprawie zarzutów na Zwierzchowskiego. Sprawę dotychczas trzymałem. Ale on mi dziś powiedział: czemu ja go oszczędzam więcej niż on sam siebie. Prawda. Szkoda go, rozumny, prawy, uczony, energiczny, ale jest w nim wielki niepokój, w którym się kołacze jakiś chochlik pieniacki [...][10].

8. Komitet Budowy Instytutu Metalurgicznego i źródła finansowania

Odpowiadając na kolejną wątpliwość dr. Tomaszewskiego (2019, s. 523), należy wskazać, że postacie wymienione w protokole Komitetu Budowy Instytutu Metalurgicznego to: George Sage Brooks (Gische SA),

[9] Władysław Takliński – rektor Akademii Górniczej [AGH od 1947 r.] w latach 1933–1939. Prowadził badania nad mechaniką teoretyczną i wytrzymałością materiałów. Wacław Jędrzejewicz [Wacek] – minister wyznań religijnych i oświecenia publicznego w rządach Janusza Jędrzejewicza, Leona Kozłowskiego oraz Walerego Sławka. Por. Żongołłowicz 2004, ss. 593, 630–631, 717.

[10] Żongołłowicz 2004, s. 684.

Wojciech Świętosławski i Zenon Martynowicz (Chemiczny Instytut Badawczy), Jan Czochralski i Henryk Mierzejewski (Politechnika Warszawska), Szymon Rudawski (naczelnik Wydziału Przemysłowo-Handlowego w Urzędzie Wojewódzkim Śląskim), Claus Kallenborn (Huta „Bismarcka" SA/Katowicka Spółka Akcyjna dla Górnictwa i Hutnictwa), Brunon Absalon (Friedenshütte Aktiengesellschaft/Huta „Pokój" SA), Aleksander Ciszewski (Zakłady Hohenlohe SA), Oskar Vogt (Oberschlesische Kraftwerk SA), Feliks Noakowski (The Henckel von Donnersmarck – Beuthen Estates Ltd), Georg Behagel (Górnośląski Związek Przemysłowców Górniczo-Hutniczych)[11].

W artykule poświęconym pracom Instytutu Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej i Janowi Czochralskiemu (Majewski 2018, ss. 93–94) powątpiewałem, że menadżerowie zrzeszeni wokół Górnośląskiego Związku Przemysłowców Górniczo-Hutniczych podołają znaczącym wydatkom przewidywanym na budowę instytutu. Przypadek sprawił, że wyszukałem informację potwierdzającą jednak takie działania. Przemysłowcy śląscy udostępnili na rzecz budowy Instytutu Metalurgicznego łącznie milion złotych. Środki przekazane prezydentowi Ignacemu Mościckiemu w trakcie spotkania na Zamku 16 stycznia 1929 r. rozdzielono w równej wartości pomiędzy dwa podmioty. Donatorzy dofinansowali istniejące już Studium Hutnicze przy Akademii Górniczej w Krakowie oraz budowę Instytutu Metalurgicznego w Warszawie[12].

Istotny wkład założycielski pod budowę Instytutu Metalurgicznego wniesiony został również przez podoficerów Wojska Polskiego. W początkach 1926 r. powstała Fundacja Lotnicza im. Marszałka Piłsudskiego. Inicjatorami tej organizacji byli mjr dypl. Ludwik Lepiarz, sierż. Edward Walerjańczyk, sierż. mech. Stanisław Kaczmarek, wach. Henryk Kwiatkowski, st. wach. Z. Papiernik oraz sierż. Franciszek Legaja. W obrębie tej organizacji zainicjowano zbiórkę pieniężną przeznaczoną na zakup samolotu. Termin udostępnienia środków pieniężnych zaplanowano na 19 marca 1928 r., w dniu imienin marszałka. Duży odzew

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[11] Archiwum Państwowe Katowice. Urząd Wojewódzki Śląski, Wydział Przemysłowo-Handlowy, sygn. 1775, k. 1. Pismo Górnośląskiego Związku Przemysłowców Górniczo-Hutniczych do naczelnika Wydziału Przemysłowo-Handlowego Śląskiego Urzędu Wojewódzkiego Szymona Rudawskiego z 6 XI 1928 r.; kk. 2–3. Protokół z posiedzenia Komitetu Budowy Instytutu Metalurgicznego, (bd.).

[12] *Polska Zbrojna* 1929a.

wśród podoficerów oraz sprawnie zrealizowane wpłaty w dość krótkim czasie przekroczyły koszty zakupu płatowca. Pojawiły się wówczas kolejne propozycje, aby zrezygnować z samolotu na rzecz okrętu podwodnego, później także planowano budowę lotniska. Ostatecznie brak porozumienia zarządu fundacji przyczynił się do przekazania w początkach lipca 1929 r. kwoty 57 902 zł na rzecz budowy Instytutu Metalurgicznego[13]. Warto podkreślić, że decyzja formalnie nastąpiła znacznie wcześniej, albowiem już w początkach listopada 1928 r. Komitet Budowy Instytutu Metalurgicznego, zawiązany, przypomnijmy, w Zakładach Hohenlohe SA, dysponował częścią tej darowizny[14].

9. Wątpliwości do życiorysu Jana Czochralskiego

Odniosę się poniżej do krytycznych komentarzy dr. Tomaszewskiego (2019, ss. 522, 526) dotyczących sytuacji materialnej i zawodowej Czochralskiego.

W sensie formalnym Jan Czochralski był emigrantem – obywatelem obcego państwa (urodzonym na ziemiach byłej Pierwszej Rzeczypospolitej, które zaborca pruski włączył do swego państwa). Będąc obywatelem niemieckim polskiego pochodzenia, wyjechał do Berlina, a później wrócił do wolnej Polski, zachowując jednak posiadane przywileje związane z przynależnością do Republiki Weimarskiej, a następnie III Rzeszy. W przypadku Czochralskiego kwestie obywatelstwa wzbudzają wiele wątpliwości, bowiem posiadał w okresie okupacji niemieckiej przywileje tożsame z Reichsdeutsche.

Na podstawie znanych dotąd źródeł historycznych nie wiemy, dlaczego stateczny mąż, odpowiedzialny ojciec trojga potomstwa, opływający we wszystkie dostatki nagle podejmuje decyzję o rezygnacji z kontynuowania kariery naukowej w świetnie wyposażonych pracowniach i laboratoriach, po czym przeprowadza się na terytoriom państwa, w którym sfery niedostatków, pomimo upływu dekady od odzyskania

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[13] Walerjańczyk 1927; Lepiarz 1928; *Polska Zbrojna* 1929.

[14] Archiwum Państwowe Katowice, Urząd Wojewódzki Śląski, Wydział Przemysłowo-Handlowy, sygn. 1775, k. 1. Pismo Górnośląskiego Związku Górniczo-Hutniczego do naczelnika Wydziału Przemysłowo-Handlowego Śląskiego Urzędu Wojewódzkiego Szymona Rudawskiego z 6 XI 1928 r.; kk. 2–3. Protokół z posiedzenia Komitetu Budowy Instytutu Metalurgicznego, (bd.).

niepodległości, wciąż są duże, a możliwości kontynuowania dotychczasowych zainteresowań naukowych nader ograniczone. W dotychczasowych wystąpieniach dr Tomaszewski uzasadniał tę problematykę zaangażowaniem na rzecz wywiadu wojskowego, nie przedstawiając jednak żadnych dowodów źródłowych. Dlatego wyrażone przez prof. dr. hab. Michała Kokowskiego wątpliwości, dotyczące braku dowodów źródłowych przemawiających za tego typu zaangażowaniem, należy uznać za uzasadnione[15]. Warto także zauważyć, że świadkowie (mjr/ppłk dypl. Wojciech Fyda i Stefan Bratkowski) działalności wywiadowczej Czochralskiego, na których powołuje się dr Tomaszewski[16], nie mieli nic wspólnego z pracami Oddziału II Sztabu Głównego. Pierwszy z nich pełnił (sierpień 1926 – luty 1931) służbę zastępcy szefa Gabinetu Wojskowego Prezydenta RP (adiutant), następnie był dowódcą Pułku Artylerii Ciężkiej we Lwowie i dopiero 26 kwietnia 1936 r. przyjął funkcję *attaché* wojskowego we Francji. Stefan Bratkowski urodził się w 1934 r., a sprawę Czochralskiego pamiętał tylko z przekazów rodzinnych[17].

Rozpoczęcie prac logistycznych przy budowie Instytutu, być może także otrzymanie tytułu doktora *honoris causa*, było asumptem dla Czochralskiego do przedstawienia w 1929 r. perspektyw gospodarczych państwa polskiego[18]. Wyraził on wówczas pogląd, że dalszy rozwój będzie zależny od powstania instytutów naukowych zajmujących się metalurgią żelaza, stali stopowych i metali kolorowych. Dostrzegał: a) problem dywersyfikacji bazy ekonomicznej z rejonu Górnego Śląska na Podkarpacie, b) korzyści płynące z wydobycia gazu ziemnego i ropy naftowej oraz c) korzyści wynikające z dużego zasobu wody oraz taniej siły roboczej na tym terenie. Jednocześnie rozpoczęcie eksploatacji tych zasobów uzależniał od rozbudowy infrastruktury komunikacyjnej[19].

Poglądy Czochralskiego mogą zaskakiwać, albowiem były zbieżne z opiniami najwyższych władz państwowych. W wystąpieniu prasowym

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[15] Kokowski 2014, ss. 139–140; 2015, ss. 283–288; 2016, ss. 405–408.

[16] Tomaszewski 2014, ss. 57–72.

[17] Archiwum Akt Nowych, Attaché wojskowi przy rządach państw kapitalistycznych, sygn. AII/133, k. 119–121. Płk dypl. Wojciech Fyda, Raport z moich pierwszych wrażeń po przybyciu do Paryża (26 IV – 7 V 1936 r.) z 8 V 1936; Ciałowicz 1970, ss. 224–225; Łowczowski 1980, ss. 38–39; Koreś 2018, ss. 187–241.

[18] Zob. Czochralski 1929a; 1929b; 1929c.

[19] *Ibidem*.

powoływał się na terminologię wojskową („rejonu bezpieczeństwa"), którą można przypisać grupie oficerów skupionych wokół gen. dyw. Kazimierza Sosnkowskiego[20]. Może to dowodzić, że plany inwestycyjne rządu, które wdrożono dopiero w drugiej połowie lat trzydziestych XX w., były już znane Czochralskiemu w 1929 r.[21] Miał więc on związki z wielką polityką, mimo iż zachował obywatelstwo państwa wrogiego Polsce. Oznacza to, że fakt tego obywatelstwa nie miał negatywnego wpływu na ocenę jego działalności w opinii władz.

10. Zakłady Hohenlohe SA

Odpowiadając na pytanie dr. Tomaszewskiego (2019, s. 522) dotyczące Zakładów Hohenlohe w Wełnowcu, należy zauważyć, że były one dużymi i rozpoznawalnymi zakładami w Drugiej Rzeczypospolitej. Hohenlohe-Werke AG zostały założone w 1905 r. przez Christiana Hohenlohe. Po podziale Górnego Śląska wyodrębniono Zakłady Hohenlohe SA w gminie Wełnowiec oraz Öhringen Bergbau AG z siedzibą w Gliwicach, a później w Berlinie. Przedsiębiorstwa te trudniły się wydobyciem oraz przerobem węgla kamiennego, glinek ogniotrwałych oraz blendy cynkowej. Poza tym produkowano m.in. blachę cynkową, kwas siarkowy, ołów, energię elektryczną oraz cegły. Kapitał zakładowy wynosił 24 250 000 zł. W skład Rady Nadzorczej wchodzili: Karol Petschek, Karol Strassburger, Ryszard Chrambah, Roman Brzeski, Jakub Goldschmidt, Ernest Henryk Heimann, ks. Hans zu Hohenlohe-Oehringen, Alfred ks. zu Hohenlohe-Schillingsfürst, Paweł Lemay, Franciszek Petschek, Henryk de Peyerimhoff, Szymon Rudawski, Stanisław Widomski. Funkcję dyrektora zarządzającego przedsiębiorstwa pełnił Aleksander Ciszewski. Warto także podkreślić, że Zakłady Hohenlohe SA posiadały znaczące pakiety papierów wartościowych w dwudziestu pięciu przedsiębiorstwach[22].

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[20] Gołębiowski 2000, ss. 12–20; Stachiewicz 1998, ss. 296–305.

[21] Czochralski zapewne zapoznał się wcześniej z rozporządzeniem Prezydenta Rzeczypospolitej Ignacego Mościckiego z marca 1928 r., „W sprawie ulg dla przedsiębiorstw przemysłowych i komunikacyjnych", które określiło też kształt przestrzenny rejonu bezpieczeństwa. Por. Dz.U.RP 1928, nr 36, poz. 329.

[22] Landau, Tomaszewski 1964, ss. 261–265; Jaros 1969, ss. 187–189; Gołębiowski 2004, ss. 59, 68, 85, 205–207; Archiwum Państwowe Katowice, Śląski Urząd Wojewódzki, Wydział Przemysłu i Handlu, sygn. 1871, kk. 68–69. Bilans operacyjny Zakładów Hohenlohe SA za lata 1927–1930.

11. Prace Jana Czochralskiego we „Wspólnocie Interesów" a koncern Wspólnoty Interesów Górniczo-Hutniczych SA

Odpowiadając na kolejną wątpliwość dr. Tomaszewskiego (2019, ss. 524), dotyczącą tego, czym była „Wspólnota Interesów", należy wyjaśnić, że owa „Wspólnota" została powołana przez Friedricha Flicka w oparciu o Katowicką Spółkę Akcyjną dla Górnictwa i Hutnictwa oraz Górnośląskie Zjednoczone Huty „Królewska" i „Laura" SA. Zadania postawione przed tą spółką nomenklaturową przewidywały obniżenie kosztów produkcji, zwiększenie asortymentu, penetrację nowych rynków zbytu oraz osiągnięcie jak największej rentowności przez wzajemną pomoc i współpracę obydwu przedsiębiorstw górniczo-hutniczych. W wyniku uchwał Walnych Zgromadzeń Górnośląskich Zjednoczonych Hut „Królewska" i „Laura" SA (8 czerwca 1929 r.) oraz Katowickiej Spółki Akcyjnej dla Górnictwa i Hutnictwa (20 września 1929 r.), połączonych we „Wspólnotę Interesów", opublikowano rezolucję głoszącą związek gospodarczy z Consolidated Silesian Steel Corporation. Portfel gestyjny spółki holdingowej zarejestrowanej w Nowym Jorku należał do Friedricha Flicka, który dążył do sprzedaży części aktywów Williamowi Averellowi Harrimanowi. W skład Rady Nadzorczej (Prezydium) „Wspólnoty Interesów" wchodził potentat finansowy Friedrich Flick. Obok niego wyróżniamy trzy grupy podmiotów posiadających bezpośredni wpływ na funkcjonowanie przedsiębiorstwa. Grupę Flicka najliczniej reprezentowali: Robert Scherff, Fryderyk Bernhardt, Claus Kallenborn, Walther Thomalla, Alfred Rhode, Herbert Monden, Robert Sznapka. Gwarantem więzi Harrimana, a także wnikliwym obserwatorem poczynań Flicka, był Eugéne Lubovitz, następnie także Irving Rossi. Interesy Skarbu Państwa oraz Skarbu Śląskiego absorbowały uwagę Alfreda Potockiego, Hipolita Gliwitza, Jana Haase, Mariana Przybylskiego. Do tej ostatniej grupy dołączył najpóźniej także Jan Czochralski. Nie wiadomo, jakie funkcje pełnił w obrębie Prezydium: czy była to tylko synekura, czy też może reprezentował interesy sektora militarno-przemysłowego. Po ustanowieniu Nadzoru Sądowego nad Górnośląskimi Zjednoczonymi Hutami „Królewska" i „Laura" SA oraz Katowicką Spółką Akcyjną dla Górnictwa i Hutnictwa (29 III 1934 – 30 I 1937), przeprowadzeniu fuzji, a następnie przejęciu kapitałów gestyjnych przez Bank Gospodarstwa Krajowego powstał dopiero 16 kwietnia 1937 r. koncern

Wspólnoty Interesów Górniczo-Hutniczych SA. Osoba Jana Czochralskiego w złożonych władzom skarbowym sprawozdaniach spółki (za lata 1937–1939) nie występuje[23].

12. Zastrzeżenia do przypisów i odsyłaczy

Odnosząc się do uwagi dr. Tomaszewskiego (2019, s. 525) dotyczącej kompozycji przypisów i odsyłaczy, chciałbym podkreślić, że decyzja o umieszczeniu tej lub innej informacji w przypisach lub tekście głównym jest decyzją własną Autora. Uzależniona jest oczywiście od poruszanej problematyki. Trudno jest pisać o eksperymentach Chemicznego Instytutu Badawczego, nie poruszając zwłaszcza tych osiągnięć, które posiadały bezpośredni związek z przemysłem zbrojeniowym. Podobne zależności odnoszą się do Huty Baildon SA, Polskich Zakładów Skody SA, Wojskowego Instytutu Badań Inżynierii (Biura Badań Technicznych Broni Pancernych).

Dziękuję za zwrócenie uwagi na działalność Stacji Doświadczalnej Politechniki Lwowskiej (Tomaszewski 2019, s. 524). Odpowiadając, warto podkreślić, że ówcześni producenci, jak i odbiorcy stali stopowych i metali kolorowych ściśle współpracowali z placówką lwowską, zlecając wykonanie ekspertyz, a następnie prób odbiorczych. Znaczenie tej placówki dla przemysłu zbrojeniowego było nie do przecenienia. Warto przy tym podkreślić, że przejęto w ten sposób ważną część zadań, które realizować miał Instytut Metalurgii i Metaloznawstwa.

Kuriozum wśród dociekań dr. Tomaszewskiego (2019, s. 524) stanowi zarzut bezcelowego opisania w artykule (M.W. Majewskiego) wykorzystania blach pancernych ... a następnie Państwowych Zakładów Inżynierii. Na wstępie odpowiedzi należy zwrócić uwagę na nieuzasadnione skracanie przez dr. Tomaszewskiego nazwy do Warsztatów Samochodowych. W przeciwieństwie do innych tego rodzaju zakładów – pułkowych, garnizonowych i okręgowych – warsztaty w Warszawie nosiły miano Centralnych. Przechodząc do wątpliwości dr. Tomaszewskiego,

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[23] Archiwum Akt Nowych, Prezydium Rady Ministrów, Komitet Ekonomiczny Rady Ministrów, sygn. 82, kk. 45–47. Uzasadnienie wniosku Antoniego Romana i Eugeniusza Kwiatkowskiego w sprawie przejęcia przez Skarb Państwa Wspólnoty Interesów z 12 VII 1936; Zawadzki 2010, ss. 155–156; Majewski 2018b, ss. 43–69.

chciałbym tylko nadmienić, że wspólne osiągnięcie IMM, a także Huty Baildon SA uniezależniało nasze państwo od kosztownego importu oraz powstrzymywało odpływ walut, pozyskiwanych z eksportu surowców, m.in. węgla i cukru. Ponadto blachy, płyty i kopuły pancerne wykorzystywane w zabudowie sprzętu pancernego, artyleryjskiego i fortyfikacyjnego znakomicie podnosiły możliwości obrony. Osiągnięcie produkcyjne IMM i Huty Baildon SA było dużym wydarzeniem w historii techniki, albowiem byliśmy drugim państwem na świecie po Anglii (Vickers-Armstrong), które samodzielnie rozwiązało te problemy uzbrojenia.

13. Miernik sukcesów naukowych Czochralskiego z okresu zatrudnienia w Instytucie Metalurgii i Metaloznawstwa

Odpowiadając na kolejny zarzut dr. Tomaszewskiego (2019, s. 520–521), dotyczący niewykorzystania artykułów zamieszczonych na łamach *Wiadomości Instytutu Metalurgii i Metaloznawstwa oraz Zakładu Metalurgii i Metaloznawstwa Politechniki Warszawskiej*, należy zgodzić się z Panem dr. Tomaszewskim, iż autor artykułu o Instytucie Metalurgii i Metaloznawstwa i postaci Czochralskiego nie jest ani chemikiem, ani metaloznawcą. Dlatego właśnie, w moim artykule, po pierwsze pominięta została analiza treści chemicznej i metaloznawczej tych artykułów, po drugie, odnotowane zostały tylko eksperymenty i doświadczenia przekładające się na rozwiązanie problemów rodzimego przemysłu zbrojeniowego.

Podzielam zdanie dr. Tomaszewskiego, że tematyka chemiczna i metaloznawcza artykułów zamieszczonych na łamach wspomnianego powyżej czasopisma może być interesującym tematem badawczym dla specjalistów tych dyscyplin. Z zainteresowaniem przeczytam artykuł poświęcony tym zagadnieniom.

Należy jednak podkreślić, iż trudno będzie w pełni ocenić dorobek własny Czochralskiego na podstawie prac opublikowanych na łamach *Wiadomości*…, albowiem w znacznej ich części występuje jako współautor. Przy ocenie tego dorobku skłaniałbym się raczej do analizy zastrzeżeń patentowych. W tej dziedzinie jego dorobek jest skromny. W 1932 r. otrzymał patent na *stopy łożyskowe odporne na zżeranie zwłaszcza stosowane* *w kolejnictwie*, a po upływie pięciu lat kolejny, dotyczący *sposobu otrzymywania prętów metalowych bez wtrąceń i likwidacji*[24].

Pozostałe prace na rzecz armii dr Tomaszewski zwykł tłumaczyć „gryfem tajemnicy wojskowej"[25], ale prawda jest taka, że poszukiwania patentów zgłoszonych przez Instytut Metalurgii i Metaloznawstwa przy Politechnice Warszawskiej oraz Dział Metalurgiczny Chemicznego Instytutu Badawczego nie przyniosły jak dotąd pozytywnych rezultatów.

14. Podsumowanie

Ufam, że udało mi się jaśniej opisać poruszoną uprzednio tematykę i wyjaśnić niektóre wątpliwości podniesione przez Pana dr. Pawła E. Tomaszewskiego. Chciałbym przede wszystkim jednak przeprosić za błędy, które wkradły się w trakcie formatowania tamtego tekstu. Pracując równocześnie nad kilkoma artykułami, przygotowując dokumentację przewodu habilitacyjnego, a także prowadząc kwerendy, bardzo łatwo o przeoczenia. Ufam, że będzie to nauczką dla mnie, aby koncentrować się na jednej tylko publikacji.

Zaledwie w jednym artykule przedstawiłem nieznane zagadnienia odnoszące się do biografii i działalności naukowej Jana Czochralskiego. Materiały archiwalne odnalazłem przypadkowo, nie koncentrując moich poszukiwań tylko i wyłącznie na tych wątkach. Dlatego postuluję przeprowadzenie rzetelnej, kompleksowej kwerendy zarówno prasoznawczej, jak i archiwalnej, w kraju oraz poza granicami, dotyczącej Jana Czochralskiego. Ponieważ jest to zadanie przekraczające możliwości pojedynczej osoby, winien powstać interdyscyplinarny zespół badawczy. Apeluję także do badaczy tej problematyki, w tym Pana dr. Tomaszewskiego, i świadków tamtych wydarzeń lub ich spadkobierców, by udostępnili swoje cenne prywatne archiwa, ażeby można było efektywnie kontynuować analizowanie tej problematyki i budować bogatsze historyczne syntezy, oparte na zweryfikowanym materiale, dzięki

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24 Urząd Patentowy Rzeczypospolitej Polskiej 1932; Urząd Patentowy Rzeczypospolitej Polskiej 1938.

25 Tomaszewski 2014, ss. 57–72; Kokowski 2014, s. 139–140; 2015, ss. 283–288; 2016, ss. 405–408.

zastosowaniu krytyki licznych źródeł historycznych. Życzę jednocześnie Panu dr. Pawłowi E. Tomaszewskiemu wielu kolejnych spektakularnych sukcesów publicystycznych, w tym również naukowych.

15. Podziękowania

Uprzejmie dziękuję Panu prof. dr. hab. Michałowi Kokowskiemu oraz zespołowi redakcyjnemu *Studia Historiae Scientiarum* za konstruktywne, rzeczowe uwagi i wskazówki.

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Scientific chronicle

News and conference reports, report on the activity of the PAU Commission on the History of Science

Kronika naukowa

Informacje i sprawozdania konferencyjne, sprawozdanie z działalności Komisji Historii Nauki PAU

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Sesja robocza „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania" (Kraków, 25 czerwca 2019 r.)

Abstrakt

Artykuł opisuje przebieg sesji roboczej „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania" (Kraków, 25 czerwca 2019 r.), zorganizowanej przez Komisję Historii Nauki PAU.

Słowa kluczowe: *polskie czasopisma naukowe, ewaluacja czasopism naukowych, polski system ewaluacji czasopism naukowych, historia i filozofia nauki, naukoznawstwo, Komisja Historii Nauki PAU, Polska Akademia Umiejętności*

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The Working Session „Polish scientific journals from the disciplines: «history and philosophy of science» and «science of science» – current challenges” (Kraków, 25 June 2019)

Abstract

The article describes the course of the Working Session „Polish scientific journals from the disciplines: «history and philosophy of science» and «science of science» – current challenges” (Kraków, 25 June 2019), organized by the Commission of the History of Science of the Polish Academy of Arts and Sciences.

Keywords: *Polish scientific journals, scientific journals evaluation, Polish scientific journals evaluation system, history and philosophy of science, science of science, Commission of the History of Science, Polish Academy of Arts and Sciences*

1. Wprowadzenie

W dniu 25 czerwca 2019 r. odbyła się w Krakowie w Polskiej Akademii Umiejętności sesja robocza „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» i «naukoznawstwo» – aktualne wyzwania” organizowana przez Komisję Historii Nauki PAU. Było to pierwsze tego typu wydarzenie w historii nauki polskiej.

2. Uczestnicy sesji

W sesji udział wzięli:

– Prof. dr hab. Andrzej Mączyński, Wiceprezes PAU.
– Przedstawiciele komitetów naukowych PAN i komisji naukowych PAU:
  1) Komitet Historii Nauki i Techniki PAN (dr hab. Jaromir Jeszke, prof. UAM, wiceprzew.; prof. dr hab. Michał Kokowski, czł. Zarządu);
  2) Komitet Naukoznawstwa PAN (prof. dr hab. Ewa Okoń-Horodyńska, przew.; prof. dr hab. Przemysław Kisiel, czł. Zarządu);

3) Komisja Filozofii Nauk PAU (ks. dr hab. Wojciech Grygiel, FSSP, sekretarz);
4) Komisja Historii Nauki PAU (prof. dr hab. Michał Kokowski, przew.; dr Rita Majkowska, wiceprzew.).

– Przedstawiciele czasopism:
1) *Historyka. Studia Metodologiczne* (red. nacz. prof. dr hab. Krzysztof Zamorski; sekretarz redakcji dr Jakub Muchowski); http://journals.pan.pl/hsm;
2) *Kwartalnik Historii Nauki i Techniki* (z-ca red. nacz. dr Danuta Ciesielska); http://www.ejournals.eu/KHNT/;
3) *Medycyna Nowożytna. Studia nad Kulturą Medyczną* (z-ca red. nacz. dr hab. Magdalena Paciorek, prof. PAN); http://www.ejournals.eu/MedycynaNowozytna/;
4) *Wiadomości Matematyczne* (red. nacz. dr Krzysztof Ciesielski); https://wydawnictwa.ptm.org.pl/index.php/wiadomosci-matematyczne;
5) *Filozofia Nauki* (red. nacz. dr Joanna Gęgotek); https://www.fn.uw.edu.pl/index.php/fn;
6) *Zagadnienia Filozoficzne w Nauce* (red. ks. dr hab. Wojciech Grygiel, FSSP); http://zfn.edu.pl/index.php/zfn/about/;
7) *Filozofia i Nauka. Studia Filozoficzne i Interdyscyplinarne* (z-ca red. nacz. dr hab. Andrzej Łukasik, prof. UMCS); http://filozofiainauka.ifispan.waw.pl;
8) *Organon* (z-ca red. mgr Paulina Pludra-Żuk) http://www.ejournals.eu/Organon/;
9) *Studia z Filozofii Polskiej* (red. dr hab. Marek Rembierz); http://weinoe.us.edu.pl/nauka/nauka-weinoe/serie-wydawnicze/studia-z-filozofii-polskiej;
10) *Nauka Polska. Jej Potrzeby, Organizacja i Rozwój* (red. nacz. dr hab. Jaromir Jeszke, prof. UAM); https://naukapolska.locloud.pl;
11) *Studia Historiae Scientiarum* (red. nacz. prof. dr hab. Michał Kokowski); http://www.ejournals.eu/sj/index.php/SHS/.

3. Przebieg sesji

Przebieg sesji określa zamieszczony poniżej plan sesji:
1) Prof. dr hab. Andrzej Mączyński, Wiceprezes PAU: Przywitanie uczestników sesji.

2) Michał Kokowski (KHN PAU), „Ogólne wprowadzenie do tematyki sesji i konkretne propozycje rozwiązań redakcyjnych i legislacyjnych”.
3) Wystąpienia reprezentantów poszczególnych czasopism: „Krótka charakterystyka czasopisma wraz z komentarzem do tematyki sesji”.
4) Wystąpienia reprezentantów Komitetu Naukoznawstwa PAN, Komitetu Historii Nauki i Techniki PAN, Komisji Filozofii Nauk PAU oraz Komisji Historii Nauki PAU.
5) Wypracowanie dokumentu skierowanego do Przewodniczącego Komisji Ewaluacji Nauki w sprawie zintegrowanej ewaluacji czasopism z dyscyplin „historia i filozofia nauki” i „naukoznawstwo”.
6) Prof. dr hab. Andrzej Mączyński, Wiceprezes PAU: Zamknięcie sesji.

4. Zakres tematyczno-problemowy sesji i dalsze perspektwy rozwoju czasopism

Tematyka sesji dotyczyła aktualnej sytuacji polskich czasopism naukowych z zakresu dyscyplin „historia i filozofia nauki” i „naukoznawstwo”, które nie funkcjonują w polskim systemie dyscyplin naukowych jako samodzielne dyscypliny badawcze.

Powodem zorganizowania tej sesji była trwająca ówcześnie ewaluacja czasopism naukowych, wynikające stąd perspektywy rozwoju czasopism oraz konieczność podjęcia zorganizowanych działań mających na celu podwyższenie jakości czasopism i zwiększenie ich cytowalności (przy pomocy działań zgodnych z zasadami etyki i prawa) w bazach indeksacyjnych Scopus, WoS, Google Scholar etc.[1]

Uczestnicy spotkania wyrazili nadzieję, że tego rodzaju sesje będą organizowane cyklicznie.

Owocem spotkania było pismo skierowane do Komisji Ewaluacji Nauki z uprzejmą prośbą o przeprowadzenie *zintegrowanej* oceny czasopism z dyscyplin: „historia i filozofia nauki” i „naukoznawstwo”, w którym wyrażono nadzieję, że część czasopism z tych dyscyplin uzyska w aktualnej ewaluacji czasopism więcej niż 20 punktów.

[1] Szersze omówienie tej problematyki, zob. Kokowski 2019.

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The report on the activities of the PAU Commission on the History of Science in 2018/2019

Abstract

The report discusses the activities of the Commission on the History of Science of the Polish Academy of Arts and Sciences in 2018/2019. It presents the lists of: scientific meetings, conferences, symposia, new members of the Commission, and new publications.

Keywords: *Commission on the History of Science, Polish Academy of Arts and Sciences (PAU), 2018/2019*

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Sprawozdanie Komisji Historii Nauki PAU w roku 2018/2019

Abstrakt

Omówiona została działalność Komisji Historii Nauki PAU w roku 2018/2019. Przedstawiono spisy: posiedzeń naukowych, konferencji i sesji naukowych, nowych Członków Komisji oraz nowych publikacji.

Słowa kluczowe: *Komisja Historii Nauki PAU*, *2018/2019*

1. Meetings of the Commission

In the period from October 2018 to June 2019 eight scientific meetings of the Commission were held, during which the following eight papers were delivered:

- dr hab. Andrzej J. Wójcik, prof. of the Polish Academy of Sciences (Institute for the History of Science, Polish Academy of Sciences), "Emil Habdank Dunikowski – a geologist and a traveler" (delivered in Polish, 24 October 2018);
- prof. dr hab. Michał Kokowski (Institute for the History of Science, Polish Academy of Sciences), "Higher education and research activities in Poland and in exile during World War II (a review of main issues)" (delivered in Polish, 28 November 2018);
- dr Jan Surman (Poletayev Institute for Theoretical and Historical Studies in the Humanities, National Research University Higher School of Economics, Moscow, Russia), "A political epistemology of Polish positivism" (delivered in Polish, 19 December 2018);
- dr hab. Tomasz Pudłocki (Department of the History of Culture and Historical Education, Institute of History, Jagiellonian University), "Unknown Canadian Polonica – William John Rose and his archival legacy" (delivered in Polish, 23 January 2019);
- dr hab. Stanisław Domoradzki, prof. URz (University of Rzeszów), prof. dr M. Zarichnyi (University of Rzeszów, Poland and Ivan Franko National University of Lviv, Ukraine), co-operation: prof. dr M. Stawiska-Friedland (AMS, USA), "Mathematics in Lviv during WWII" (delivered in Polish, 27 March 2019);

- dr Paweł Brzegowy (Institute of Geography, Pedagogical University of Cracow), "Lviv's geographic school of Antoni Rehman" (delivered in Polish, 24 April 2019);
- prof. Andre Goddu, PhD (Stonehill University, USA), "The Solitary Networker, the Circles Around Copernicus" (the meeting jointly organized in Warsaw by the PAU Commission on the History of Science and the Institute of the History of Science PAN, paper delivered in English, 22 May 2019);
- dr hab. Wiesław Wójcik, prof. UJD (Institute of Philosophy, Jan Długosz University in Częstochowa), "Was there a second Warsaw mathematics school?" (delivered in Polish, 12 June 2019).

On 27 February 2019, an organizational and scientific meeting of the Commission was held, concerning, among others, the evaluation of journals on the history of science, the activities of the Commission in the defense of journals on the history of science, and the scientific conferences co-organized by the Commission.

On 12 June 2019, supplementary elections were held for the members of the PAU Commission on the History of Science. As a result of the voting, three new members of the Commission were elected:

1) dr Sławomir Dorocki (Department of Business and Spatial Management, Institute of Geography, Pedagogical University of Cracow);
2) dr hab. Marek Rembierz (Faculty of Ethnology and Educational Science in Cieszyn, University of Silesia in Katowice);
3) prof. dr hab. Zenon Roskal (Chair of Philosophy of Nature, Faculty of Philosophy, The John Paul II Catholic University of Lublin).

The results of the Commission votes were approved by the PAU Council on 10 September 2019.

2. Publications

In December 2018 the following work was published:

- *Studia Historiae Scientiarum*, vol. 17. Edited by Michał Kokowski. Kraków: Polska Akademia Umiejętności, 2018, pp. 617.

In 2018/2019 work was ongoing on the release of the following publication:

- *Studia Historiae Scientiarum*, vol. 18. Edited by Michał Kokowski. Kraków: Polska Akademia Umiejętności, 2019, 570 pp.

3. Conferences and scientific sessions organized by the Commission

In 2018/2019, the Commission organized two scientific conferences and one working session:

- Conference "Leonardo da Vinci – on the 500th anniversary of death" (5 May 2019; together with the National Museum in Kraków and the PAU Commission on the History of Art).[1]
- Working session "Polish scientific journals from disciplines: *history and philosophy of science* and *science of science* – current challenges" (25 June 2019).[2]
- "Dickstein Forum" (19–21 September 2019; together with the International Academy of the History of Science).

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Kokowski, Michał (ed.) 2019a: *Studia Historiae Scientiarum*, volume 18. Kraków: Polska Akademia Umiejętności, pp. 570.

Kokowski, Michał 2019b: The Working Session "Polish scientific journals from the disciplines: «history and philosophy of science» and «science of science» – current challenges" (Kraków, 25 June 2019) [in Polish]. *Studia Historiae Scientiarum* 18, ss. 557–561. DOI: 10.4467/2543702XSHS.19.018.11024.

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1 See "Invitation" and "poster" (with the conference program, in Polish).

2 See Kokowski 2019b (in Polish).

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Sprawozdanie Komisji Historii Nauki PAU w roku 2018/2019

Abstrakt

Omówiona została działalność Komisji Historii Nauki PAU w roku 2018/2019. Przedstawiono spisy: posiedzeń naukowych, konferencji i sesji naukowych, nowych członków Komisji oraz nowych publikacji.

Słowa kluczowe: *Komisja Historii Nauki PAU, 2018/2019*

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The report on the activities of the PAU Commission on the History of Science in 2018/2019

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Keywords: *Polish Academy of Arts and Sciences (PAU), Commission on the History of Science, 2018/2019*

1. Posiedzenia Komisji

W okresie od października 2018 do czerwca 2019 r. odbyło się osiem posiedzeń naukowych Komisji, na których wygłoszono osiem referatów:

- Dr hab. Andrzej J. Wójcik, prof. PAN (Instytut Historii Nauki PAN), „Emil Habdank Dunikowski – geolog i podróżnik" (24 października 2018 r.)
- Prof. dr hab. Michał Kokowski (Instytut Historii Nauki PAN), „Szkolnictwo wyższe i nauka w Polsce i na uchodźstwie w czasie II wojny światowej (przegląd głównych problemów)" (28 listopada 2018 r.)
- Dr Jan Surman (Poletayev Institute for Theoretical and Historical Studies in the Humanities, National Research University Higher School of Economics, Moskwa, Rosja), „Polityczna epistemologia polskiego pozytywizmu" (19 grudnia 2018 r.)
- Dr hab. Tomasz Pudłocki (Zakład Historii Kultury i Edukacji Historycznej, Instytut Historii Uniwersytetu Jagiellońskiego), „Nieznane polonika kanadyjskie – William John Rose i jego archiwalna spuścizna" (23 stycznia 2019 r.)
- Dr hab. Stanisław Domoradzki, prof. URz (Uniwersytet Rzeszowski), prof. dr hab. Mykhaylo Zarichnyy (Uniwersytet Rzeszowski i Narodowy Uniwersytet Iwana Franki we Lwowie, Ukraina), współpraca: prof. dr M. Stawiska-Friedland (AMS, USA), „Matematyka we Lwowie w czasie II wojny światowej" (27 marca 2019 r.)

- Dr Paweł Brzegowy (Instytut Geografii Uniwersytetu Pedagogicznego im. Komisji Edukacji Narodowej w Krakowie), „Lwowska szkoła geograficzna Antoniego Rehmana" (24 kwietnia 2019 r.)
- Prof. dr André Goddu (Stonehill College, Easton, Massachusetts, Stany Zjednoczone), „The Solitary Networker, the Circles Around Copernicus", posiedzenie organizowane wspólnie przez Komisję Historii Nauki PAU i Instytut Historii Nauki PAN (Warszawa, 22 maja 2019 r.)
- Dr hab. Wiesław Wójcik, prof. AJD (Instytut Filozofii, Akademia im. Jana Długosza w Częstochowie), „Czy istniała druga warszawska szkoła matematyczna?" (12 czerwca 2019 r.).

W dniu 27 lutego 2019 r. odbyło się posiedzenie organizacyjno-naukowe Komisji, dotyczące m.in. ewaluacji czasopism z historii nauki, działań Komisji w obronie czasopism z historii nauki oraz konferencji naukowych współorganizowanych przez Komisję.

W dniu 12 czerwca 2019 r. odbyły się wybory uzupełniające członków Komisji Historii Nauki PAU. W wyniku przeprowadzonych głosowań wybrano trzech nowych członków Komisji:

1) Dr Sławomir Dorocki (Zakład Przedsiębiorczości i Gospodarki Przestrzennej, Instytut Geografii, Uniwersytet Pedagogiczny im. Komisji Edukacji Narodowej w Krakowie);
2) Dr hab. Marek Rembierz (Wydział Etnologii i Nauk o Edukacji w Cieszynie, Uniwersytet Śląski w Katowicach);
3) Prof. dr hab. Zenon Roskal (Katedra Filozofii Przyrody, Wydział Filozofii, Katolicki Uniwersytet Lubelski Jana Pawła II).

Wyniki głosowań Komisji zostały zatwierdzone przez Radę PAU w dniu 10 września 2019 r.

2. Sprawy wydawnicze

W grudniu 2018 r. wydano:

- *Studia Historiae Scientiarum* t. 17. Pod redakcją Michała Kokowskiego. Kraków: Polska Akademia Umiejętności, 2018, ss. 617.

W roku 2018/2019 trwały prace redakcyjne nad wydaniem:

- *Studia Historiae Scientiarum* t. 18. Pod redakcją Michała Kokowskiego. Kraków: Polska Akademia Umiejętności, 2019, ss. 570.

3. Konferencje i sesje naukowe organizowane przez Komisję

W 2018/2019 r. Komisja była organizatorem dwóch konferencji naukowych i jednej sesji roboczej:

- Konferencja „Leonardo da Vinci – w pięćsetlecie śmierci" (5 maja 2019 r.; wspólnie z Muzeum Narodowym w Krakowie i Komisją Historii Sztuki PAU)[1].
- Sesja robocza „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania" (25 czerwca 2019 r.)[2].
- „Dickstein Forum" (19–21 września 2019 r.; wspólnie z International Academy of the History of Science).

Bibliografia

Dokumenty bieżące Komisji Historii Nauki PAU 2018/2019.

Kokowski, Michał (red.) 2018: *Studia Historiae Scientiarum*, tom 17. Kraków: Polska Akademia Umiejętności, ss. 617. Dostęp online: http://www.ejournals.eu/sj/index.php/SHS/issue/view/668/showToc.

Kokowski, Michał (red.) 2019a: *Studia Historiae Scientiarum*, tom 18. Kraków: Polska Akademia Umiejętności, ss. 570.

Kokowski, Michał 2019b: Sesja robocza „Polskie czasopisma naukowe z dyscyplin: «historia i filozofia nauki» oraz «naukoznawstwo» – aktualne wyzwania" (25 czerwca 2019 r.). *Studia Historiae Scientiarum* 18, ss. 557–561. DOI: 10.4467/2543702XSHS.19.018.11024.

Muzeum Narodowe w Krakowie, Polska Akademia Umiejętności (Komisja Historii Nauki PAU, Komisja Historii Sztuki PAU) 2019: Konferencja „Leonardo da Vinci – w pięćsetlecie śmierci" (5 maja 2019 r.). Zaproszenie, plakat. Dostęp online: http://pau.krakow.pl/zaproszenia/2019/Leonardo_da_Vinci_zaproszenie_9052019.pdf; http://pau.krakow.pl/zaproszenia/2019/Leonardo_da_Vinci_plakat_9052019.pdf.

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[1] Zob. Zaproszenie na konferencję i plakat (z programem konferencji).

[2] Zob. Kokowski 2019b.