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Daling, Dorien

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"On the ruins of seriality": The scientific journal and the nature of the scientific life

Dorien Daling

Department of History, Faculty of Arts, University of Groningen, Oude Kijk in 't Jatstraat 26, 9712 EK Groningen, The Netherlands

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ABSTRACT

Twenty-first-century discourse on science has been marked by narratives of crisis. Science is said to be experiencing crises of public trust, of peer review and publishing, of reproducibility and replicability, and of recognition and reward. The dominant response has been to "repair" the scientific literature and the system of scientific publishing through open science. This paper places the current predicament of scholarly communication in historical perspective by exploring the evolution of the scientific journal in the second half of the twentieth century. I focus on a new genre of scientific journal invented by Dutch commercial publishers shortly after World War II, and on its effects on the nature of the scientific life. I show that profit-oriented publishers and discipline-building scientists worked together to make postwar science more open, while also arguing that formats of scientific publication have their own agency.

Introduction

In 2021 open access advocate Jean-Claude Guédon—one of the sixteen signatories of the Budapest Open Access Initiative (2002)—identified two "errors" in the system of scientific publishing: commerce and competition (Guédon, 2021). He claimed that after World War II commercial publishers "changed the nature of scholarly journals" and "distorted" scientific communication through "the perverse use of rankings." These rankings allowed them to seek market shares while arguing their case in terms of excellence. As a result, the current evaluation system of universities and funding agencies "amounts to little more than chasing citations" in a limited set of journals. Bringing publishing back under the control of research communities will restore the "Great Conversation" of science, Guédon predicted, and unleash "the bootstrapping of humanity to ever-rising levels of understanding reality." He presented open access as "simply a way to express the cross-fertilization" of new technologies with "the very values of science that the great sociologist of science, Robert K. Merton had identified […], the *ethos* that emerged with the Scientific Revolution" (Guédon, 2017).

The problems pointed out by Guédon are very real: commercial publishing *has* strongly affected the nature of scientific journals and the practices attached to them, there *is* a mismatch between the commodification of scientific knowledge and the quantification of scientific excellence on the one hand and vocational dedication to knowledge-making on the other hand, and science's publishing regime and reward structure *are* in need of reform. At the same time, however, Guédon's argument relies on ahistorical conceptions of the scientific life and the scientific journal. It is also connected to specific normative expectations of what science should be.

As such, Guédon's views exemplify the narratives of crisis that mark current discourse on science as well as the dominant response to them. Science is said to be experiencing crises of public trust, of peer review and publishing, of reproducibility and replicability, and of recognition and reward. According to Bart Penders and colleagues, the crisis narratives have brought about a "moral economy of repair" that aims to "identify, expose and expunge as much bias as possible" in order to "uphold objectivity as the hallmark for proper science" (Penders et al., 2020, pp. 107–108). Including the replication movement and certain strands of open science, the moral economy of repair has been primarily concerned with scholarly communication. It invokes heroic images of the scientific literature, which is supposed to be a repository of carefully warranted knowledge claims, the cornerstone of trust within scientific communities, and the bedrock of expert consensus and public legitimacy (Csiszar, 2018). Evidence that the literature does not live up to these expectations is taken as a cue that the published record and the apparatus of scientific publishing need to be "fixed." The fix entails, among other things, radical transparency, increased rigor of research, reporting, and judgment, and the removal of "external" interests and goals (see, e.g., Fidler & Wilcox, 2021). In many respects, attempts to repair science are tantamount to proposing "a nostalgic return to Truth and

E-mail address: W.D.Daling@rug.nl.

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disinterestedness" (Shapin, (2019, December 2).), that is, a return to the "true" nature and goals of the scientific enterprise. Johanna Cohoon and James Howison have argued that "the open science movement is interested in inscribing an ideology into scientific technology" (Cohoon & Howison, 2021, p. 116). Open science advocates pursue "improved science through the use of open technological systems" and "inscription of Mertonian norms" (p. 129).[1]

However, these norms do not represent a present-day consensus on what constitutes good science. Nor do they reflect how scientific knowledge was made and legitimized in the past (as decades of science studies scholarship have shown) or a historical consensus on the purpose of science and the proper virtues of the scientist. Steven Shapin (2008) has described a vast range of different beliefs and realities associated with the scientific life. When Robert K. Merton (1942/1973) outlined his set of norms for the practice of science—communism, universalism, disinterestedness, and organized skepticism—he did not capture a centuries-old ethos but offered a new account of scientific objectivity. His stipulations became a commonplace in response to the institutional integration of science with government and business (the rise of organized Big Science), and to the moral and professional normalization of the scientific life (science as a job as opposed to science as a calling—whether associated with moral superiority or with ascetic dedication). The "very values of science" that Guédon traces back all the way to the Scientific Revolution are thus largely a Cold War legacy.

Furthermore, Guédon's argument implies that throughout the period from the mid-seventeenth century to World War II scholarly communication was "pure" and unproblematic, and the scientific journal remained unchanged. Yet Alex Csiszar (2018) has shown that the scientific journal as we would now recognize it was an invention of the nineteenth century—not the seventeenth (see also, e.g., Fyfe, 2016; Shuttleworth & Charnley, 2016[2]). In Britain and France, the serialization of scientific print began around 1800 as a peripheral challenge to a central institution: the elite learned society. Individuals who were inspired by the French Revolution or recognized science's potential to change everyday life collaborated with commercial publishers to make natural knowledge accessible to broader publics. By 1900 the journal constituted the main source of both scientific identity and the public legitimacy of the scientific enterprise. Its evolution over the course of the century—amidst a media landscape that remained highly diverse—was in large part about balancing scientists' expert cultures with public demands for accountability. Csiszar's study reveals that this process involved a wide array of actors, interests, and values, and many questions of a political nature: who could write about science, who could allocate scientific credit, who should be able to access scientific knowledge. It also entailed the adaptation of genres, technologies, and values borrowed from the commercial press.

The moral economy of repair is therefore trying to restore a past that never was. Moreover, the current predicament of scholarly communication is as much the product of scientific interests and practices as of commercial ones. It is the result of relentless experimentation with formats, genres, publishing models, judgment procedures, indexes, and metrics. All of these efforts were aimed at making knowledge communication and evaluation more efficient, more objective, more fair, or more open. While they were profoundly consequential in shaping the scientific life, they seldom solved the issues they intended to address—disorder, dispersion, inaccessibility, problems of trust and reward. In fact, the opposite was often the case. There is no reason to think that twenty-first-century experiments are fundamentally different. Reproducibility projects, next generation metrics, FAIR data,[3] and open platforms have politics and unintended consequences too. Nevertheless, the repair economy is already changing the future.

In this paper, I highlight the evolution of the scientific journal in the period preceding the rise of the moral economy of repair: the second half of the twentieth century. My aim is to provide insight into post-World War II changes in patterns of scientific publication and their effects on the nature of the scientific life, understood as the conditions and meanings of scientific knowledge-making and the (attributed) characteristics, motivations, and satisfaction of knowledge-makers. I concentrate on the invention and rise of a new genre of scientific journal that emerged from collaborations between profit-oriented publishers and discipline-building scientists. This was the type of journal—international, specialized, fast, and free of charge for authors—that came to prevail from the late 1950s onward, and that would eventually become emblematic of an abusive publishing industry. I focus specifically on two journals that were established at publishing companies in the Netherlands and constitute early examples of the postwar genre: *Biochimica et Biophysica Acta*, founded in 1946 at Elsevier, and *Nuclear Physics*, founded in 1955 at North-Holland (merged with Elsevier in 1970). Both companies adopted strategies from what has been called the "German model of science publishing" (Edelman, 2004).

This episode in the history of scientific publishing has received some attention (Andriesse, 2008; Fredriksson, 2001; van Leeuwen, 1980), but these studies were mainly interested in the rise of international publishing, the role of Dutch firms therein, and the perspectives of publishers. Historiography on the media landscape of science using a politics-of-knowledge approach has tended to be Anglocentric and focused on pre-1900 history. In this article, I aim to apply this approach to the period after World War II. I explore the interplay between changing formats of scientific publication and shifting notions of the nature of science and the identity of the scientist, while also analyzing the ways in which this interplay was shaped by different kinds of actors, interests, and values. As Csiszar (2018) has explained, the nineteenth-century evolution of the scientific journal revolved around establishing the virtues and outer boundaries of expert legitimacy. I argue that its twentieth-century development hinged on debates about science's inner contours: the construction of scientific communities and the grounds on which they claimed legitimacy. In the later part of the century, these debates became bound up (again) with contests over the essence of scientific authority within a wider political landscape.

As such, my exploration considers questions of format (the scientific journal) and genre (the postwar type of scientific journal introduced by Dutch commercial publishers). Following Robin de Mourat, Donato Ricci, and Bruno Latour, I understand formats of scientific publication—journal, monograph, handbook, edited volume, and so on—as the results of processes of stabilization in which particular practices became crystallized into a specific name (de Mourat et al., 2020, pp. 103–105). Formats stand for certain material and technological conditions and work in an institutional manner, implying certain modalities of research, writing, and reading, and organizing the whole range of practices and actors that make up a publishing environment. Over the course of time, however, the names by which formats are identified, are reused and related to increasingly heterogeneous materialities, practices, and discourses. In this sense, formats can be seen as the outcomes of local and contingent processes of destabilization, or "as genres associated with a set of cultural techniques and sociotechnological assemblages" (p. 104). In addition to the fluidity of formats of scientific publication—or the "play between difference and repetition" (p. 105)—de Mourat and colleagues also emphasize their agency. There is "a *thingness* at work" (p. 111), they insist, as the role of publishing-related

[1] Other concerns are that open science tends to favor (technological) one-size-fits-all solutions that disregard epistemic pluralism (Penders et al., 2020) and may reinforce global inequality (see, e.g., Albornoz et al., 2018). See also Mirowski (2018) for a critique of open science as a neoliberal artifact, and Eve and Gray (2020) for suggestions for alternative open values.

[2] See Simon (2022) for an extensive survey of the historiography on scientific publishing.

[3] FAIR data are data which are Findable, Accessible, Interoperable, and Reusable.

activities continuously evolves beyond functions of research dissemination to transform the very core of how research is envisioned and conducted. "Formats make publics, set expectations, and orient sense-making practices as much as well-defined organizations" (p. 112).

The paper is organized in three parts. I begin with a discussion of "seriality" and of the interplay between format and vocation in early twentieth-century science. The first part also introduces the German model of scientific publishing and Elsevier's connection to it. Then I examine the invention and character of the postwar genre of scientific journal. This part focuses on *Biochimica et Biophysica Acta* and *Nuclear Physics* and is mainly based on an analysis of mission statements, editorial texts, commemorative contributions, and journal features (including epistemological and geographical scope, language, types of papers and other contents, publication time, and reporting style, among other things). What follows next is an evaluation of how the serial format and specifically the genre introduced by commercial publishers affected the nature of the scientific life. Here I concentrate on the 1960s, while ending with the late 1980s. The latter decade saw the beginning of the "serials crisis," which gave birth to the movement for open access. Studying scientific publishing in the second half of the twentieth century might further our understanding of the problems that we are witnessing today, and inspire new ways of thinking about scholarly communication. As I will argue, the future of knowledge communication is partly dependent on processes and considerations beyond scientists' control, notably format agency and public conceptions of the purpose of the scientific enterprise.

Seriality and the scientific life in the earlier part of the twentieth century

The serialization of scientific print was part of a broader shift to serial arrangements. Seriality occupied a central place as one of the organizing principles of modernity (Anderson, 1998; Hopwood et al., 2010; Lerner, 2015). To nineteenth-century thinkers seriality was not just a logic of ordering information, but rather an expression of, in the words of Michel Foucault (1970), "the profoundly historical mode of being of things and men" (as cited in Hopwood et al., 2010, p. 254). By describing and prescribing a sequential and progressive order in both nature and society, seriality actively informed historical, utopian, evolutionary, and positivist thought (Csiszar, 2010; Hopwood et al., 2010; Lerner, 2015, pp. 128–131). Practices and experiences were serialized too, in governance, labor, and leisure (for instance the use of statistics, factory work, reading the penny press), as well as in science (see also Law & Patten, 2009). Periodical publication brought with it the positivist idea of serial knowledge accumulation. Scientific papers came to be viewed as the building blocks of knowledge, each of them corresponding to a single discovery and author. This view was typically accompanied by the presumption that nature itself was serial in its basic structure. Apart from scientific print, series featured in laboratory experiments, in paper technologies such as medical records, and in the public display of scientific objects in museums.

Seriality was implicated in the disenchantment of nature and natural knowledge. Its increasing relevance to science's systems of representation coincided with systematic attacks against the belief in the religious and moral significance of knowledge about nature (Shapin, 2008, Chapter 2), and with the emergence of the modern notion of objectivity (Daston & Galison, 2007). It was the mid-nineteenth century (again, not the seventeenth) that saw the explicit attempt to take out the human element from knowledge-making and to place trust in the efficacy of "the scientific method." Objectivity emerged as an epistemic virtue attributed to those people who were recognized as knowledge-makers. Drawing on ancient traditions of asceticism and self-cultivation, scientists were supposed to be self-disciplined and self-denying, acting in a machine-like way that left no room for individual subjectivity, emotion, and imagination.

Serial formats and practices of scientific publication did not just reflect these shifts in epistemic values and virtues. They also actively helped to produce the idea of scientific objectivity and impersonality. In the second half of the century, much effort was spent on defining the boundaries of proper scientific publication, on determining what counted as a scientific paper (a self-authored, original research report with a fixed publication date), and on promoting impersonal reporting and judgment (Csiszar, 2010, 2018). While retaining a logic of trust that relied on public transparency, the scientific journal became increasingly exclusive, technical, and specialized. After 1900, when the journal had developed into the central institution of knowledge legitimization, the virtuous man of science was fashioned even more emphatically as a figure who published concise, efficiently written, and standardly formatted papers.[4] Moreover, editorial judgment and criticism between experts moved behind the scenes, and scientific papers were stripped of historical contextualization and detailed methodological reflection, in order to represent the immortalization in print of discoveries made by single researchers in disciplinary spaces.

Yet this drive toward impersonalization and rationalization—in scientific print as well as in presumptions about the nature of science and the character of the scientist—remained in tension with sentiments that underlined the relevance of the personal and the moral. When Max Weber (1917/1919) formulated his prescriptions for the scientific vocation, insisting on scientists' renunciation of claims to moral truth and their ascetic dedication to scientific truth, he did so amidst widely differing conceptions of the scientific life (Shapin, 2008, pp. 11, 15, 25). The virtue of "mechanical objectivity" coexisted with the virtues of "structural objectivity" and "trained judgment" (Daston & Galison, 2007), metaphysical ambitions with anti-metaphysical notions of science (with varying degrees of epistemological arrogance/modesty), commitments to the unity of knowledge with demands for specialization, and beliefs in the moral utility of science with technocratic ideals. From the fin de siècle to the 1940s, according to Shapin, "the identity of the scientist was radically unstable" (Shapin, 2008, p. 46).[5]

This fluidity of scientific identity translates into the "play between difference and repetition" that formats of scientific publication represent. Traces of conflict over epistemic virtues and publication behaviors can also be found *in* the printed pages of scientific periodicals. A volume of *Pflügers Archiv für die gesamte Physiologie des Menschen und der Tiere* (1868) provides a case in point. In 1929 the journal celebrated the 100th birth anniversary of its founder Eduard Pflüger (1829–1910) by publishing four speeches that had been given in his honor at the University of Bonn. Two of the authors engaged in a discussion about criticism in scientific print, for which Pflüger had been known and feared. His former assistant Rudolf Rosemann maintained:

> For the sake of the brevity of scientific publications, the editors of scientific journals do not allow detailed reporting of individual experiments. Polemic discussions are rejected as being undesirable. Our review journals report on all researches with the same "objectivity," and the reviewer is even expressly forbidden to make critical comments (Rosemann et al., 1929, p. 562, translated by the author).

To Rosemann, objectivity was the opposite of criticism and hence a threat to scientific progress. Debate, reflection, and detailed information should not be confined to informal communication or notebooks *behind* the public face of science. "How rarely nowadays does one come across a test of the reliability of a research method?," he asked, lamenting that there were not many scientific questions "to which one cannot find in the literature as many answers in one direction as in the opposite direction" (pp. 561–562). He predicted that scientists would soon choke on the flood of publications of which the value could hardly be assessed,

[4] The format of "Introduction, Methods, Results, Discussion" emerged in the 1920s and became the absolute standard in the 1970s (Fyfe, 2016, p. 394).

[5] See Baneke (2011) and Harrington (1996) for elaborations of this theme in relation to respectively the Netherlands and Germany.

because “the underlayers are no longer reported” (p. 562).

Albrecht Bethe, one of the editors of *Pflügers Archiv*, felt compelled to respond. He recalled that Pflüger “did not tell the contributors to his Archiv to shut up, allowing polemics that were so crude that they seem completely unthinkable to us today” (p. 570 note 2, translated by the author). “Criticism must be!” Bethe asserted, but times had changed:

> We no longer consider every author who is a bit sloppy in sending his findings into the world a vile or malicious person, and we no longer believe from the outset that someone is a fool if he draws different conclusions from the facts than those that seem logical to ourselves. And if we do believe it, then we do not say it, but just try to show in a purely objective manner that the other person is wrong (pp. 570–571).

As this instance shows, confidence in impersonal print as the way to consolidate knowledge was not universal. Nevertheless, the trend toward less elaborate and more “objective” reporting was strong,[6] causing the ever shorter papers to become useless to anyone but the most informed inner circle of experts (and causing the disseminated findings to become less reproducible). Even though (or because) twentieth-century scientists were progressively disciplined through the norms of the printed journal page, the printed pages themselves typically revealed less and less of the controversies surrounding the trend. The journal was gradually stripped of “redundant” materials—(frequent) editorials, varieties of reviews and news—in the same manner as the paper was.

In contrast, it appears that after 1900 commemorative content not only survived but gained in significance, even if reserved for rare occasions such as the passing of an editor or notable anniversaries in the life of the journal or its key figures. This must have had something to do with the fact that by then the scientific journal had become an important locus for building and maintaining a disciplinary community. Commemoration was one of the “sense-making practices” that periodicals relied on in forging and strengthening a disciplinary identity. Commemorative practices in science bolster cohesion, convey continuity and legitimacy, and instill epistemic and cultural values.[7] These agendas can also be observed in the speeches in honor of Pflüger: the authors highlighted the origins of the discipline and the journal, paid tribute to Pflüger as the founding father, praised his “passionate urge for truth” (p. 550), and laid out those values and virtues that were no longer deemed appropriate or part of the disciplinary identity, for example the “personal statements in his works” (p. 570) and his philosophical and teleological approach to understanding nature (p. 565). Another change in the period between the commemorated past and the commemorating present was the pace of specialization. Whereas Pflüger had been deeply distressed by the founding of the *Zeitschrift für physiologische Chemie* (1877),[8] because he saw the unity of the science of physiology as representing the unity of life itself (see also Pflüger, 1877), Bethe accepted specialization as a matter of fact. He believed that it was only logical that “the representatives of every young field that strives for independence want to have their own publication organs” (p. 571).

These aspirations were fueled by commercial publishers. Beginning in the late nineteenth century, publishing companies in Germany recognized the opportunities presented by increasing specialization and rapid scientific growth, in both the university and the industrial research laboratory (Edelman, 2004).[9] Consequently, a publishing model dominated by commercial publishers came to replace the decentralized system of university-based publications and privately-owned journals that had emerged a century earlier.[10]

The new German model of scientific publishing—which contrasted with the Anglo-American one dominated by learned societies—was not only built on an entrepreneurial approach to incipient fields of study. It was also characterized by a “holistic” approach to science in print that took into account the fragmenting effects of seriality and specialization. German publishing companies thus set out to develop a comprehensive system of information for each of the scientific communities that they served (Edelman, 2004, pp. 188–189; Sarkowski, 1996, pp. 164–166). The research journal was at the core of the system, flanked by genres and formats that were supposed to bring unity to the periodical literature and make it more accessible. From 1907 onward, Springer Verlag (1842) in Berlin was the first to deploy this strategy, focusing on six types of publication: research journals, review journals (*Zentralblätter*), progress journals (*Ergebnisse*), handbooks (*Handbücher*), monographs, and professional journals. The handbook was the most authoritative of the synthesizing genres. Handbooks were used as reference works, but on a more fundamental level they were intended as “totalizing bodies of knowledge” (Grote, 2020, p. 199). Their purpose was to cope with the volume, scattering, and outdating of specialized serial knowledge, objectively summarizing the periodical literature of a discipline into an “organic whole” (pp. 191, 199).[11] One such handbook that became canonical for its discipline was *Beilsteins Handbuch der organischen Chemie* (1881)—acquired by Springer in 1916.

By the 1920s, after many mergers, scientific publishing in Germany was dominated by two large firms: Springer and Akademische Verlagsgesellschaft (1906) in Leipzig (Edelman, 2004; Sarkowski, 1996). Both companies earned more than half of their revenues abroad. International demand for German scientific literature had been substantial since the mid-nineteenth century (except for the years around World War I). The coming to power of the National Socialists changed all this, not least because the leading firms—and several smaller ones—were owned by Jews. The Nazi regime had devastating consequences for German scientific publishing as well as for German science and German as a language of science. In the late 1930s and early 1940s, Jewish émigrés brought the German publishing model to the United States, founding companies such as Interscience Publishers (1940) and Academic Press (1942).

Jewish émigrés and contacts with German publishers were also crucial to the rise of Elsevier’s Scientific Publishing Company (1936; Daling, 2006). In 1937 the Dutch firm acquired English translation rights to a number of classic German books.[12] In the same year Elsevier employed two Jewish former editors of *Beilsteins Handbuch*, Edith Josephy and Fritz Radt, who began to compile an English-language, modernized version of the German handbook. The result of their

[6] Presumably, Germany was relatively late in joining this trend (Csiszar, 2018, pp. 16–17).

[7] See Abir-Am and Elliott (1999) for a systematic analysis of collective memory in science. The volume does not specifically address commemorative practices of or regarding scientific journals, though. See Tollebeek (2015) for a study of commemoration in the humanities.

[8] The *Zeitschrift für physiologische Chemie* was founded by Felix Hoppe-Seyler in Strasbourg (nine years after Pflüger had launched the *Archiv*). It still exists today, under the title *Biological Chemistry*.

[9] See Meinel (1997) for contributions on a variety of topics related to German scientific literature in the nineteenth and twentieth centuries.

[10] In the German lands, the serialization of scientific print had been pioneered by individual scientists in their capacity as university professors. Renowned examples of journals that were founded and edited by individual scientists include the *Annalen der Physik* (1799), the *Annalen der Pharmacie* (1832), and the above-mentioned *Archiv für die gesamte Physiologie des Menschen und der Tiere* (1868). The former two were established respectively by Ludwig Wilhelm Gilbert in Halle and Justus Liebig in Giessen. While their titles and scopes have changed several times, all three journals still exist and are now published in English: *Annalen der Physik*, *European Journal of Organic Chemistry*, *Pflügers Archiv: European Journal of Physiology*.

[11] Grote argues that this idea reflected “a vitalist leitmotiv” of German scientific discourse prior to World War II (p. 199; see also Harrington, 1996).

[12] One highly successful example is the four-volume work *The chemistry of carbon compounds* (1939–1947), compiled by Viktor von Richter.

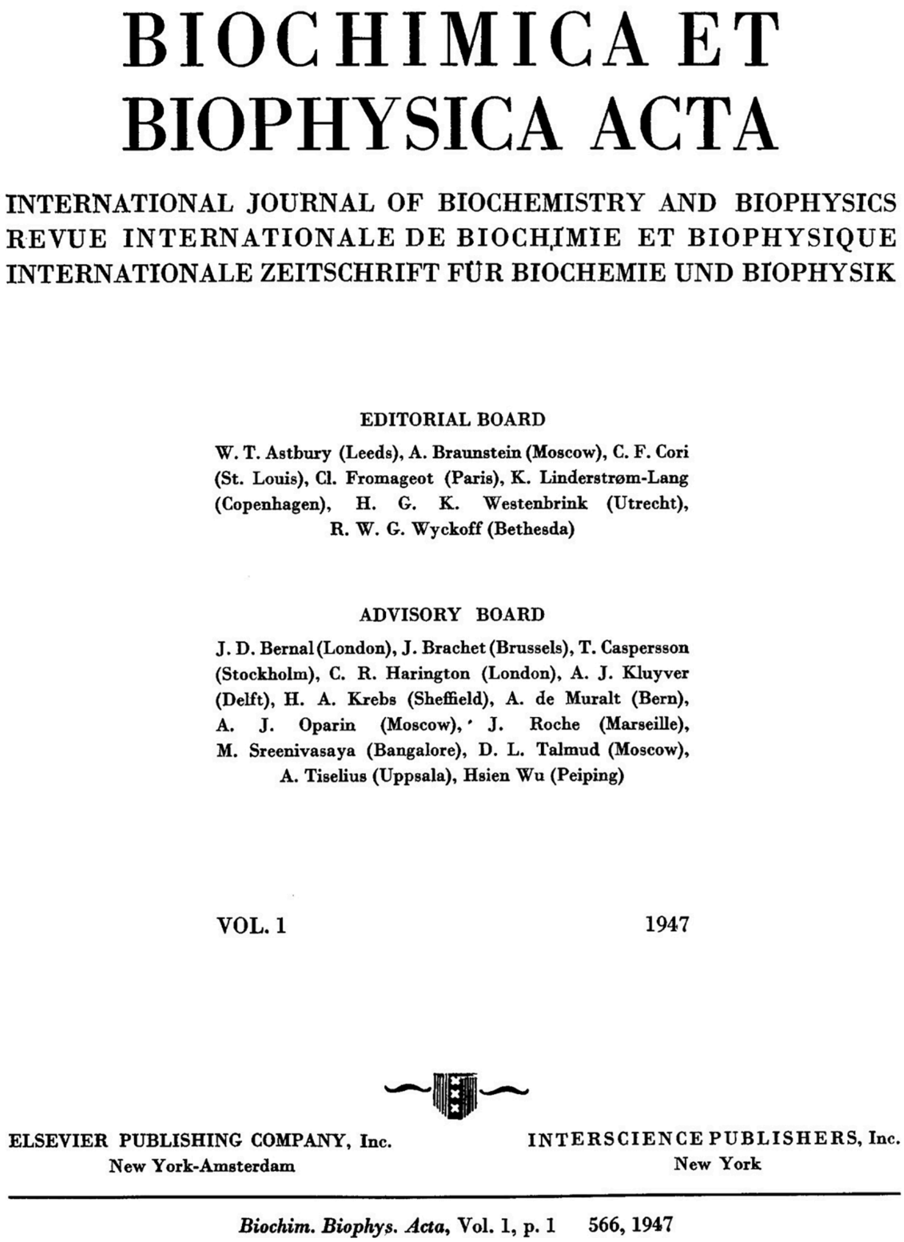
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Fig. 1. Title page of the first volume of *Biochimica et Biophysica Acta* (1947). Courtesy of Elsevier, https://www.sciencedirect.com/.

initiative, *Elsevier's Encyclopaedia of Organic Chemistry* (1940–1969), was pivotal to Elsevier's entry as a journal publisher.

The postwar reinvention of the serial format

Beginning in the interwar period, scientific publishing became a focal point of disputes over scientific organization, first in relation to left-wing calls for the central planning of scientific progress, and after World War II in relation to the rise of Big Science. In Britain in the 1930s, the so-called "red science movement" (see Hobsbawm, 1999; Roberts, 2005) campaigned for a comprehensive reform of the apparatus of scientific communication. The distinguished crystallographer John Desmond Bernal in particular expressed discontent with the publishing system (East, 1998; Muddiman, 2003). In his view, publication controlled through periodicals of scientific societies—the principal journal publishers in the United Kingdom—was an unacceptable element of "high science" that delayed and distorted research dissemination. Matters came to a head in 1948, when Bernal submitted a proposal for the national distribution of scientific papers to the Royal Society Scientific Information Conference. Implying that the system of scientific journals would be replaced by a central agency of scientific information, the proposal not only outraged the scientific societies but also proponents of "scientific freedom".[13] It even sparked a hostile reaction from the British press, which accused Bernal—who was well known as a Marxist—of "totalitarianism," destroying "free scientific enquiry," and putting "truth in danger" (as cited in East, 1998, pp. 295–296). Due to the vested interests of the societies and in the context of the unfolding Cold War, Bernal's proposal did not stand a chance.

The publishing apparatus was also crucial to postwar discourses of scientific objectivity. As Shapin has pointed out, science's entanglement with government and industry, and scientists' employment in institutions long considered external to science, required a new account of

[13] See Reinisch (2000) for a study of the Society for Freedom in Science (1940–1963).

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NUCLEAR PHYSICS

MONTHLY JOURNAL DEVOTED TO THE EXPERIMENTAL AND THEORETICAL STUDY OF ATOMIC NUCLEI, NUCLEAR FIELDS AND THE FUNDAMENTAL ASPECTS OF COSMIC RADIATION

EDITOR: L. ROSENFELD - MANCHESTER

VOLUME I

N·H P·C

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Fig. 2. Title page of the first volume of *Nuclear Physics* (1956). Courtesy of Elsevier, https://www.sciencedirect.com/.

scientific integrity (Shapin, 2008, pp. 15, 21–23). Following Merton's (1942/1973) stipulations, the emphasis was shifted from the virtues and vocational dedication of the individual researcher to structural virtue and communal ethos. Scientific spokespeople came to describe scientists as members of an autonomous, self-regulating community endowed with a special set of virtues—disinterestedness, universalism, amorality, and so on. The scientific journal was essential to this argument, as it represented the scientific community's primary means of self-policing and public accountability (Csiszar, 2018, pp. 286–287). Accordingly, the periodical literature became even more synonymous with impersonal judgment and expert consensus. Only then did peer review emerge as a common practice—applied by scholarly journals of all types—and as a procedure easily recognizable to twenty-first-century eyes (Fyfe, 2016, pp. 394–395).

These issues were, however, of less immediate concern to researchers in continental Europe. Their first priority after World War II was to reestablish scientific communication. Reaching an international audience was especially difficult, given the collapse of the German publishing industry and the scarcity in other European countries of journals with an international readership. British and American journals, furthermore, were published by nationally oriented scientific societies, only accepted papers in English, and did not really need contributions from European authors. The Dutch plant scientist Frans Verdoorn, who had moved to the United States in 1940, tried to raise awareness of these conditions in a 1948 issue of *Science*. He made a case for the potential of scientific publications to advance international cooperation, the aims of which he defined as "the exchange of information […] in such a way that it will be available to anyone who can profit by it," "the attainment of objectives which individuals or scientists of a single institution or nation cannot accomplish," and "the formation of an *esprit de corps* which may […] counteract the evils of human international politics" (Verdoorn, 1948, p. 492).

Additionally, Verdoorn insisted that "the modern scientific journal offers a number of problems to its readers or, rather, to its authors and editors which were unthought of one or two generations ago" (p. 493). In the same year as the Royal Society Scientific Information Conference, and in a similar but more international vein, he identified the following issues: high production costs and prices, the changing mode of use and the decline of reading, differing views on the desired degree of editing and uniformity of papers, the need for a register of all scientific journals (with suitable subject and geographical indexes), and "factors in academic life which lead to the habit of publishing a great many small papers" (p. 494). The Dutch scientist then made the following suggestion: "I believe one of the great possibilities to be the establishment of large, international, scientific journals." There seems to be "quite a *raison d'être*," he explicated, for journals with a "truly international character" (not just in readership), a wide scope, world-wide circulation, and the capacity to "publish short, somewhat important material quickly" (pp. 494–495).

Shortly before Verdoorn's proposal, Elsevier had begun to publish a periodical with exactly those characteristics: *Biochimica et Biophysica Acta* (*BBA*). The biochemical journal was founded in 1946 at the initiative of Hendrik Westenbrink (1901–1964), professor at the University of Utrecht, and with the help of Kaj Linderstrøm-Lang from Denmark and Claude Fromageot from France. North-Holland (1931) followed in 1955 with the establishment of *Nuclear Physics* (*NP*). Its editor in chief was Léon Rosenfeld (1904–1974), a Belgian theoretical physicist working in Manchester and from 1958 in Copenhagen. Both periodicals represent the postwar reinvention of the serial format by Dutch and American exponents of the German model of scientific publishing (Daling, 2011). This postwar genre of scientific journal, introduced by commercial publishers, seemed to offer solutions to some of the above-mentioned problems. It aimed to counterbalance the hegemony of American and British journals—and certain norms and practices attached to them—and provided opportunities to form and reform scientific communities.

Westenbrink's main ambitions with *BBA* were to revive biochemistry in formerly Nazi-occupied Europe, and to emancipate biochemistry as an autonomous discipline—independent from medicine and physiology, oriented toward chemistry and physics (Daling, 2011, Chapters 1, 3–4). Regarding the latter ambition, *BBA* distinguished itself from older biochemical periodicals by explicitly addressing biophysicists in its title and mission statement, and by including them on the editorial board (Fig. 1). Biophysics was rapidly expanding in the 1940s and early 1950s as a result of the molecularization of the life sciences. A content analysis of *BBA*'s first ten years reveals that the journal served the full spectrum of the life sciences: from "old-style" and "new-style" biochemists (with, respectively, physiological-chemical and physical–chemical orientations) to bioscientists who studied cutting-edge topics—for instance protein synthesis and the structure of nucleic acids (DNA and RNA)—with techniques such as electron microscopy and X-ray diffraction.

With respect to the former ambition, the mission statement presented *BBA* as "a symbol of the re-establishment of international cooperation in the post-war period" (as cited in Slater, 1986, p. 20). The journal was indeed fully international, with editorial and advisory board members coming from the Netherlands, Denmark, France, Britain (including Bernal!), the United States, the Soviet Union, Belgium, Sweden, Switzerland, India, and China. *BBA* deliberately chose a title in a politically neutral language (Latin)—particularly crucial to getting the Russians to participate—and accepted papers in English, French, and German.[14] Until 1956 a summary in each of the three languages was provided for every article. Although the first volumes contained contributions from more than ten different countries, almost ninety percent of the papers originated from Western Europe and the most loyal authors from France.[15] *BBA* became more global in the 1950s.

NP was a fairly global journal from the start (Daling, 2011, Chapters 7–8). Twenty-eight countries were represented in the editorial board (Fig. 2) and contributions came from all corners of the world. *NP*'s language policy was similar to that of *BBA*, but abstracts were always in English and already during the first five years more than ninety percent of the papers were written in English. The community behind *NP* was not only nationally diverse, but also linked to certain geopolitical visions and politics-of-knowledge agendas. The journal can be seen as a site of resistance to dominant (Western) Cold War discourse. Nicknamed "the square root of Bohr times Trotsky" (as cited in Pais, 1997, p. 161), editor in chief Rosenfeld was a non-conformist thinker who adopted a dissident position within various contexts (Skaar Jacobsen, 2012). He was a proponent of a "third way" (ideologically leaning toward the Soviet Union), a materialist among adherents of the so-called "Copenhagen interpretation" of quantum mechanics (developed by Niels Bohr in close collaboration with Rosenfeld; see, e.g., Faye, 2019), and a "Copenhagener" and non-Leninist among Marxists. The editorial board included several other Western physicists who were known for their Marxist sympathies—for example Frédéric Joliot-Curie—and seven members from communist countries. A significant number of the board members were active in organizations such as the World Federation of Scientific Workers and Pugwash.[16] Furthermore, there was a substantial overlap between the social contexts of *NP* and the slightly older European Organization for Nuclear Research (1954), known as CERN.

Considering these leanings and connections, the following objective of *NP* will come as no surprise. In the first volume, somewhat tucked away in a book review, Rosenfeld wrote: "Neglect of foreign work is an attitude […] widespread among American physicists. […] Is it too much to hope that *Nuclear Physics* might help to restore some balance in the appreciation and utilisation of valuable contributions to the common endeavour?" (Rosenfeld, 1956d, p. 671). *NP*'s epistemological objectives were to a certain extent related to its (geo)political ones. The editorial preface in the first volume stated that the journal "will be devoted to the experimental and theoretical study of atomic nuclei, not excluding […] the investigation of the 'elementary' particles" (Rosenfeld, 1956a). By the time of *NP*'s launch, low-energy nuclear physics and high-energy particle physics were already growing apart. The latter field was dominated by American experimental physicists, working in teams and with "big machines," whereas *NP* was led by members of the prewar elite: European, theoretically oriented, and with a nostalgia for intimate conditions of research. In contrast with *BBA*, therefore, *NP*'s ambition was not of an emancipatory but a defensive nature. Attached to a discourse of epistemic and cultural superiority, the journal was set on keeping particle physics under the control of nuclear physics, both discursively and institutionally.

Yet with regard to postwar problems of scientific print not related to disciplinary identity, *BBA* and *NP* offered similar solutions (Daling, 2011; Fredriksson, 2001). These features made them very attractive to authors. First of all, *BBA* and *NP*—and other new journals like them—operated on a subscription-based business model that charged the user but not the author. As such, they provided an alternative to prominent American journals, which were inaccessible to many authors from Europe and the Global South due to their high page charges.

Second, commercial periodicals issued as many volumes as were needed to satisfy the demand for space. These so-called "open-end journals" usually had a shorter publication time than society journals. Brief reports were published even faster than regular-length papers. In 1951 *BBA* was the first biochemical journal to start a section for "short communications" (concise but complete descriptions of small investigations) and "preliminary notes" (brief reports of important findings of ongoing research, typically claiming certain results). The maximum length allowed was respectively three and two printed pages. This novelty had a major role in *BBA* becoming more global—including authors from the Anglo-American world—and one of the largest biochemical periodicals around the mid-1950s. *NP* had a section for short notes too, which developed into a separate journal in 1962: *Physics Letters*.

Third, both Westenbrink and Rosenfeld invested a lot of energy in supporting authors through the publication process. Shortly after the former's death in 1964, *BBA* published a collection of commemorative essays in which the new managing editor Edward (Bill) Slater venerated Westenbrink for his "ideals of devoted and disinterested service" (Gruber et al., 1965, p. xviii). He highlighted:

> [Westenbrink] had sympathy for those whose knowledge of English was poor and for those without the facilities for the drawing of figures ready for publication. He and the sub-editing staff of Elsevier gave much assistance to authors in this respect […]. He was distressed that the very rapid growth of the journal made it inevitable

[14] See Gordin (2015) for a history of scientific languages, including Cold War approaches to "Scientific Babel."

[15] Because of this strong French connection, *BBA* prolonged its trilingual policy until 1982. This was much later than when most European-based journals switched to publishing in English only.

[16] Out of the twenty-two participants in the first Pugwash Conference on Science and World Affairs (1957), five were members of *NP*'s editorial board: Mark Oliphant, Hans Thirring, Sin-Itiro Tomonaga, Victor Weisskopf, and Dmitri Skobeltzyn.

that the close personal relations between author and editor became more tenuous (p. xvii).

Other than that, Westenbrink was committed to a limited conception of editorship. According to Slater, he only required a high standard of research, feeling that "the author should be given as much freedom as possible in the manner of presentation" (p. xvii). In the same collection of essays, editorial board member Ralph Wyckoff emphasized that the postwar information explosion gave "much support to those who favored the dehumanizing of scientific publications," and that *BBA* "was founded on an editorial policy the antithesis of this" (p. xi). Westenbrink disapproved of forcing "submitted manuscripts into a common mold" and of demanding revisions until "the individuality of the men who had done the research was lost in this common pattern of reporting" (p. xi). Wyckoff added that *BBA* relied on the principle that authors were accountable to their colleagues: "They were his judges and not editors or anonymous referees deciding whether or not a reasonable-appearing piece of work should be published" (p. xi). This restricted notion of the editorial role was reflected in the content and appearance of *BBA*. There was hardly any communication beyond the dissemination of research findings: the journal was basically a bundle of articles (printed on cheap paper). And although Westenbrink was *BBA*'s founder and did the majority of the work, he refused the title of managing editor. His name was to be found on the cover page only as a member of the editorial board—one of seven names in alphabetical order. Contrary to the papers it published, the journal was in fact an example of "dehumanization."

In these respects, *BBA* and *NP* were each other's opposites. Rosenfeld acted very much as the editor *in chief*—leading a board of fifty members—and his personality and preoccupations shine through in every issue of *NP*. His key concern was the readability of papers instead of prioritizing brevity and uniformity (the general trend, particularly in the Anglo-American world) or the freedom of authors (*BBA*). Rosenfeld spoke out against further decontextualization and underlined the relevance of clarity and even elegance. He despised "the uncouth *Physical Review* dialect" (Rosenfeld, 1956c)—the reporting style of the leading physics journal founded in 1893 by the American Physical Society. Additionally, *NP* consistently included book reviews and news items (on conferences, awards, research projects), whereas most journals now devoted their pages almost entirely to original research papers. *NP*'s book review section tended to focus on synthetic literature and works concerning subfields of physics other than nuclear physics. This focus indicates that as a specialized periodical—one of the few in physics in the 1950s—*NP* took it upon itself to keep the community informed about what was going on in the wider discipline.

Rosenfeld frequently used his book reviews as an outlet for venting personal opinions and as a tool for instilling the "proper" values and virtues. His main preoccupations were nomenclature, irresponsible publication behaviors (bad writing, sloppy analysis, incomplete results, duplicate publication), and the Copenhagen interpretation of quantum mechanics. Remarkably, *NP*'s editor in chief did not shy away from harsh criticism in a tone which the editors of *Pflügers Archiv* had condemned as outdated already thirty years earlier. Many people took offense at it (Skaar Jacobsen, 2012, pp. 57–58). After a devastating review of his book *Foundations of quantum mechanics* (Rosenfeld, 1956b), for instance, Alfred Landé called Rosenfeld out on his "self-assurance," and on behaving as if "*the real* wisdom is to be found only in Copenhagen and surrounding feodalities" (Landé, 1957, p. 133). Intentionally or not, Rosenfeld actually confirmed the point when he declared that there had been no hesitation in deciding to publish Landé's comments, but "whether they will help his cause is another question, which may safely be left to the judgement of the readers" (Rosenfeld, 1957).

In sum, the postwar genre of scientific journal invented by Dutch commercial publishers was shaped by a curious mix of innovation and tradition (in both form and content), epistemic and non-epistemic values, and impersonal and personal dimensions (the growing trust in impersonal norms and the continuing—or even accentuated—importance of personal virtues and relations[17]—among editors, between editors and authors, and between editors and publishers). As a genre, open-end subscription journals were international, specialized (but still with a wide scope), fast, and free of charge for authors. The individual representations of the genre were associated with specific practices, discourses, and conceptions (of editorship, authorship, readability) depending on disciplinary identities, political agendas, epistemological commitments, and editors' idiosyncratic preferences. This curious mix was essential to the postwar rebuilding of continental European research communities, and enabled the new commercial publishers in the Netherlands (and the United States) to take the leading positions in the rapidly growing market for international scientific literature.[18] The British and American societies retained their national orientation. From the 1960s onward, however, commercial journals and those sponsored by scientific societies increasingly converged on forms and norms (including standardized refereeing procedures), while the interests of editors and commercial publishers began to diverge—at least that is how editors of first-generation postwar journals perceived it.

Accelerating seriality and the serials crisis

"New journals are born every day by Caesarean section performed by skilful publishers," wrote the biochemist Erwin Chargaff (1905–2002) in 1963. He decried the alliance between profit-oriented publishers and discipline-building scientists, accusing these "pioneer[s] at no extra cost" of exacerbating "the fragmentation of the sciences" and of transforming the published record into "a Tower of Babel made of paper" (Chargaff, 1963a, p. 109, 1963b, p. 177). Chargaff was quite perceptive—and early—in drawing links between shifting patterns of scientific publication and changes in the nature of the scientific life. He also exposed the relation between the inflated volume of published "scientific news" and careerist motivations. In Chargaff's view, the transition from artisanal little science to organized Big Science as well as the increasingly public character of mass science had produced a new kind of scientist who was, among other things, prone to adopting publicity strategies from mass media (see also Abir-Am, 1980, pp. 31–32; Shapin, 2008, pp. 84–85, 173). These "noise boys" were "passionately ambitious" instead of "passionately passionate," spending their lives "dancing a minuet before assembled science reporters" and "send[ing] out mimeographed copies of their papers long before publication" (Chargaff, 1963b, pp. 175–176, 190). He further remarked: "What can be done to stem the ever-growing avalanche of rubbish being published? I can think of only one way: to publish all papers anonymously, without authors' names" (p. 196).

Chargaff was a Jewish American of Austro-Hungarian descent who had emigrated to New York in 1935. There he had focused his research on elucidating the chemical composition of DNA. His findings (together with Rosalind Franklin's X-ray diffraction images) had laid the groundwork for the discovery of the double-helix structure of DNA (1953). By the late 1950s Chargaff was presenting himself as an outspoken critic of postwar (American) science in general and of molecular biology in particular, famous for his statement that this new, transdisciplinary field was "essentially the practice of biochemistry without a license" (p. 176).[19] The standard explanation as to why he

[17] This observation is in line with Shapin's (2008) argument about the relevance of personal virtue, familiarity, and charisma in everyday institutional practices, particularly in late modern contexts of uncertainty.

[18] Yet, despite its immediate scientific success, the first four years of *BBA* were extremely difficult from a commercial point of view. The journal was a financial burden to Elsevier. It took until 1951 for *BBA* to realize a modest profit and another three years until the accumulated losses were balanced (Slater, 1986, pp. 27–28).

[19] See also Chargaff (1978), his autobiography.

became a polemicist against molecular biology is bitterness at his exclusion from the 1962 Nobel Prize in Physiology or Medicine (for the discovery of the structure of DNA) and at the lack of recognition from the actual awardees: molecular biologists James Watson, Francis Crick, and Maurice Wilkins. Yet he had other reasons too, such as being absorbed in "the cult of a vanishing European spirit" (Abir-Am, 1980, p. 52) and having lost his mother and sister to the Holocaust. Due to the latter, Chargaff saw biotechnology and genetic engineering as one of the greatest threats to humanity: "What I see coming is a gigantic slaughterhouse, a molecular Auschwitz, in which valuable enzymes, hormones, and so on will be extracted instead of gold teeth" (Chargaff, 1987, p. 199).

For present purposes it is especially relevant to note that Chargaff's views were emblematic of a larger conflict between biochemists and molecular biologists. Pnina G. Abir-Am has characterized this clash as "a debate between two modes of scientific authority, that is, between a traditional, disciplinary, empiricist, method-oriented, slow, small-scale, vocational mode and a progressive, transdisciplinary, conventionalist, model-oriented, fast, large-scale, entrepreneurial mode" (Abir-Am, 1992, p. 167; see also, e.g., de Chadarevian, 2002; Rheinberger, 1997). Even more relevant is that Chargaff served on the editorial board of *BBA* from 1955 to 1972. Shortly after his entry the journal had begun to lose its innovative momentum, as self-proclaimed molecular biologists were seeking to establish an identity of their own (Daling, 2011, Chapter 4). Some of them had previously published in *BBA*, including Watson—the epitome of a "noise boy" in Chargaff's eyes. In 1959 John Kendrew, Wilkins, Watson, and a few others launched the *Journal of Molecular Biology*, published by Academic Press in London. The journal became influential quickly, much to the dismay of *BBA*'s inner circle. Not surprisingly, therefore, the 1965 collection of essays in commemoration of Westenbrink presented him as "one of the pioneers of the modern and now fashionable Molecular Biology" (Gruber et al., 1965, p. xiv). In their dismay, Chargaff and other *BBA* figures conveniently forgot that twenty years earlier biochemists had accomplished the emancipation of their own discipline with the help of a "skilful publisher."

The proliferation of new journals—representing novel interdisciplinary fields, covering ever smaller subjects—with its implication of having to share disciplinary power was not the only thing that bothered editors of early examples of the postwar genre. They were also confronted with intensifying commercial demands. Elsevier's way of dealing with competition and unbridled growth of the literature was "twigging": the division of a journal into more specialized sub-journals, each with its own subscription (besides subscription to the full set). From 1962 onward, *BBA* was published in five sections (ten in 2023): *General Subjects*, *Enzymological Subjects*, *Lipids and Related Subjects*, *Nucleic Acids and Related Subjects*, and *Biophysical Subjects*. In 1967 North-Holland split *NP* into two sections (which still exist today): *A* for nuclear physics and *B* for high-energy physics. According to Rosenfeld, "this arrangement has a purely practical function and is in no way intended to introduce the kind of artificial splitting to which the editorial policy […] remains firmly opposed" (Rosenfeld, 1966, p. vi). *Nuclear Physics B* should not be seen as a separate journal, he asserted, "which would be a direct encouragement to a 'compartmentalization' (a word as ugly as the thing itself) detrimental to the healthy development of science" (p. vi). A decade earlier he had justified *NP*'s founding, itself an act of "compartmentalization," as a step in the direction of "a more rational organisation of the publication of original papers in various branches of physics" (Rosenfeld, 1956a).

But here again, the wound was partially a self-inflicted one. Twigging was only possible because scientists had been preoccupied with classifying the branches of science, both for the purpose of coping with the expanding volume of serial scientific information and for the purpose of dictating the direction of scientific development (defending the boundaries of disciplines or carving out new specialisms). This is why the fin de siècle had seen a transition from author-based catalogues and indexes to subject-based indexes, although the latter did not replace the former (Csiszar, 2010). Just as author-based projects such as the Royal Society's *Catalogue of Scientific Papers* (1867–1925) had made it possible to quantify scientific worth, instigating the emergence of a publish-or-perish culture (see also Csiszar, 2017),[20] so were subject-based indexes followed by unanticipated consequences such as twigging.

Indexing practices also had the effect of transforming readers into users and reading into browsing and scanning. Verdoorn had commented already in 1948 that "many scientists have now lost the reading habit to such an extent that […] we approach our journals, either via the abstracting journal or by glancing through the table of contents, and quickly copy on an index card whatever seems essential" (Verdoorn, 1948, p. 494). Soon thereafter journals commenced the indexing of the content of papers. First it became common to place an abstract at the beginning of the paper instead of a summary at the end, then journals began to add a classification code at the top of the page,[21] next came keywords under the abstract. Rosenfeld put *NP* in the forefront of these innovations. The journal's subject index was altered several times during the 1950s and 1960s.[22] In 1964 the editor announced that *NP* was going to try out the keyword system that was being devised by a committee of the American National Academy of Sciences. In addition to "showing at a glance the scope of a given paper and the new results it contains," he explained, the system "should allow the setting up of cumulative indexes entirely by means of machines" (Rosenfeld, 1964).

It is in processes like this that "a thingness is at work," independent of intentions of either publishers or editors. Rosenfeld's insistence on the readability of papers seems futile now, in view of his commitment to indexing. Authorship too was affected by seriality's agency. The *raison d'être* of *BBA*'s preliminary notes was fast circulation of important findings—besides serving Elsevier's business model—but the means to this end soon became an end in itself. What was the point of writing a 6,000-word paper when priority was established as firmly with a 600-word note, especially when these notes were associated with excellence, counted as full publications, and could be followed up by another publication in the form of a regular-length paper? New formats and genres, and innovations within them, do not simply arise as solutions to problems of communication or satisfy preexisting needs; they also create needs. These, in turn, may give rise to new problems. And to new formats and genres.

As such, the short notes introduced in commercial periodicals—together with the older tradition of writing "letters to the editor" (Adair, 2008)—gave birth to the genre of the letter journal. The first journal exclusively for brief reports was *Physical Review Letters* (1958), which has been one of the most prestigious journals in physics ever since. The above-mentioned "sister journal" *Physics Letters* was founded with the goal of serving physicists outside the United States (Brown & ter Haar, 1962). Both letter journals used new techniques—typewriter composition and photo-offset printing—to speed up production. The editor of *Physical Review Letters*, the Dutch-American physicist Sam Goudsmit, claimed that journals for research papers could no longer be considered vehicles for scientific advance. They were now just sites for the registration of knowledge. Letter journals, on the other hand, "keep physicists informed about the most significant advances in all highly active areas of basic research" (Goudsmit, 1962). To do this effectively they "must maintain their readability," Goudsmit stressed, otherwise letter journals "will experience the discouraging fate of so-called 'archive' journals which, as the name seems to imply, are more often just stored than read by the subscribers." The arrival of the letter journal marked a new phase in the "morselization" of scientific knowledge,

[20] The Science Citation Index and the Journal Impact Factor made their debut respectively in 1964 and 1972.

[21] An example from *NP* is 1.D.1, which stood for Nuclear structure, Nuclear models, Shell model.

[22] All issues contained a table of contents. The last issue of every volume came with an author index and a subject index.

Fig. 3. The volumes of *BBA* from the years 1947–1963 in the depot of the University of Groningen Library, November 2010. Soon after 1963 the growth of the journal began to spiral out of control. Soon after 2010 the digitization of *BBA* led to the destruction of the printed volumes in Groningen. Photograph by the author.

which had begun with serialization: the acceleration and dispersion of knowledge in bits and pieces (Csiszar, 2018, pp. 20, 286). Simultaneously, counterstrategies in the form of synthesizing genres—the "paradoxical media of scientific modernity"—were becoming less effective (Grote, 2020, pp. 201–203). After 1960 the holistic idea of a handbook comprehensively covering a field was no longer feasible.

Nothing represents accelerating seriality better than the growth rate of open-end subscription journals. Between 1947 and 1965 *BBA* doubled in size every four years (Daling, 2011, pp. 133–134, 250–251; Slater, 1986, Chapter 5; Fig. 3). The journal began with one volume of approximately 600 pages and published 2,400 pages in 1956. In 1965 eighteen volumes appeared. The annual number of published pages of *NP* grew from 1,200 pages in 1956 to almost 11,000 in 1966. The initial advantage of not charging authors turned into a liability in the mid-1960s. The editors of *BBA* started to receive complaints about the mounting subscription price. After the price for 1965 had been set at $288, one biochemist wrote: "I first subscribed to it, when it was in the range of $46.00 a year, but it soon got too rich for my blood [...]. Now we have gotten to the place where it looks to me like the Journal is going to take the majority of our funds" (as cited in Slater, 1986, p. 64). He ended his letter by rhetorically asking: "Is there anything that we could mention to you which would be useful or is this a commercial publication, handled by Elsevier, with you folks merely doing the handwork on the editing" (p. 65).

By the 1980s Elsevier had developed into the largest publisher of scientific journals in the world (at that time issuing around 600 periodicals; 2,600 in 2023). The same decade saw the onset of the still ongoing predicament that is commonly referred to as the "serials crisis": libraries have to cancel journal subscriptions because their budgets remain flat or decline while subscription prices outpace inflation (Jurchen, 2020, p. 161). Beginning in the 1990s, the movement for open access was supposed to provide a solution to this crisis by challenging the publishing industry with the help of the internet (Bartling & Friesike, 2013; Guédon, 2017; Jurchen, 2020).

The term serials crisis can also be understood to have more fundamental meanings. From the 1960s onward, the presumption of scientific objectivity became increasingly contested. Certainly by the 1980s, exacerbated by high-profile cases of scientific fraud, "the nature of Weber's and Merton's explanandum—the overall integrity of science [...]—was being influentially denied" (Shapin, 2008, p. 87). Concerns about the integrity of science inevitably went hand in hand with concerns about the state of the scientific literature, and about seriality's effects on the nature of the scientific life. A commentary by Chargaff in *BBA* 1000, published in 1989 as a commemorative volume (Fig. 4),[23] is a case in point. The biochemist lamented the "decline in the vitality and survival power of much of present research" (Chargaff, 1989, pp. 15–16). He reckoned that the "life expectancy" of published findings had dropped to three to five years, whereas "one should have thought that a scientific paper [...] published nearly forty years ago, would remain citable even now and that its results, for what they are worth, would be as 'true' now as they were when first revealed" (p. 15). They "should actually last forever," he added, "if the sciences really were what they claim to be." This decline and other conditions of postwar science had, in Chargaff's view, distorted researchers' satisfaction with their work: "The gratification he derives resembles that of an ancient Egyptian

[23] *BBA* 1000 consists of reprints of seminal papers from the journal's early history (1947–1967) accompanied by commentaries from the original author (s). The volume can be seen as a counter-memory aimed at demonstrating that biochemists—particularly those of *BBA*—had been doing molecular biology before there were molecular biologists.

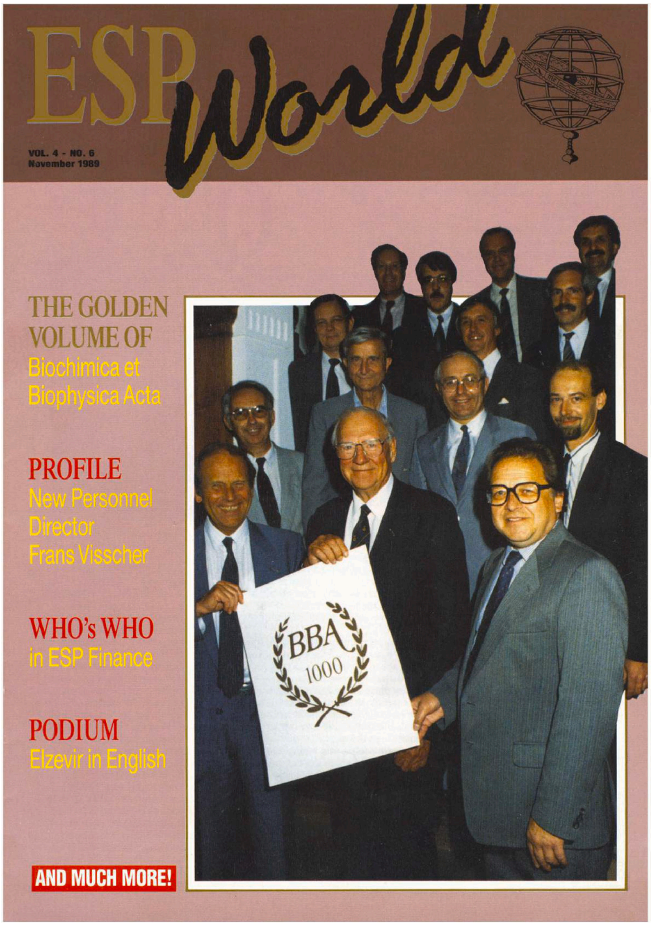

Fig. 4. The November 1989 cover of *ESP World*, a magazine for employees of Elsevier Science Publishers. In that year Elsevier's "flagship journal" *BBA* celebrated the publication of its 1000th volume. The cover features the managing editors of *BBA* with members of Elsevier's Biomedical Division at the publication party for *BBA* 1000. Standing in the middle of the front row is honorary managing editor Edward (Bill) Slater. Courtesy of Vereniging van Gepensioneerden Elsevier Ondernemingen, https://www.vgeo.nl/elsevieriana_espworld.php.

dragging a huge stone block towards a pharaoh's pyramid."

Conclusion: The serial format in a postserial world

The serialization of scientific print began around 1800 as an effort to challenge elite science and to make knowledge accessible to broader publics. Over the course of the nineteenth and early twentieth centuries, the scientific journal developed into the central institution of knowledge legitimization, bound up with discourses of objectivity, vocational dedication, and communal virtue. Since the last few decades, however, the journal has been at the heart of crisis narratives that warn of the erosion of science's moral basis and creative capacity. Competition, careerism, and perverse incentives—reflected in and produced by the serial format—have left the scientific self without a sense of calling, the "scientific community" without a sense of community, and the general public of science without a sense of trust. Twenty-first-century science finds itself "on the ruins of seriality" (Lerner, 2015, p. 132).

Yet there have hardly been any attempts to reimagine scholarly communication without the journal in a central position.[24] Notwithstanding vigorous debate on its (de)merits and intense experimentation with peer review and open publishing platforms, the scientific journal has proven to be a "sticky" institution. Even advocates of platforms admit that journals, "if they reflect well-identified research communities, their specific problems, instruments, data and models," can continue to "help navigate the scholarly archive," but without "ever own[ing] any document" and without "reference to evaluation" (Guédon, 2021).[25] And although in the digital world the journal's constitutive nature as a *serial* format is becoming less and less relevant, it is still primarily the paper—as the base unit of scientific publication—that conditions the modalities of scientific research, writing, and reading, and orients conceptions of scholarly selfhood in both the scientific and the general culture.

The commercial publishers have also demonstrated their stickiness. The open access movement has posed a serious challenge, but all in all the publishing companies have been able to integrate demands for "openness" into their business models (just as the scientific societies were able to adapt to the rise of commercial publishing in the postwar period). Elsevier states on its website that it is "one of the fastest-growing Open Access publishers in the world," offering "a wide range of open access options to fit the diverse needs of institutions, funders, academic societies and researchers […] without ever compromising on the things they trust us for: quality, rigorous peer review and research integrity" (Elsevier, 2023). So, despite predictions that "networked brains" would revolutionize scientific communication and produce "an unprecedented public good" (Guédon, 2017), open access has essentially come to mean "pay to publish," that is, a return to the situation before the ascendancy of the subscription journal (see also Noel, 2020).

Seriality's epistemic virtues, too, have remained relatively constant. The criticism of open access has centered on the journal as a seat of value—both commercially and with regard to science's system of reward. The goals of the movement resemble in many ways what John Desmond Bernal tried to accomplish already in the 1940s—rationalizing the apparatus of producing and accessing the periodical literature—with the main difference that its efforts have been aimed at removing the control of the commercial publishers instead of the scientific societies. For their part, open science and other trajectories of the moral economy of repair focus on exposing bias and discouraging biased work. The future of knowledge communication is therefore at risk of being determined by movements that seek to reaffirm disinterestedness and Truth (with a capital "T") as well as the journal's position as the lynchpin of the scientific enterprise. This is a risk because disinterested research committed to truth-seeking, while still valuable as a regulative ideal, cannot be taken as an accurate description of practical epistemology and the published record—not in the past and especially not now. It would be impossible to disentangle science from power and profit, and to reverse its transformation into a career. We also do not live anymore in a world that subscribes to serial representations of nature and knowledge accumulation.

Stickiness and stabilization are, however, not the only features that characterize the serial format. Through its diverse genres, the journal also stands for differentiation and destabilization. After World War II scientists and commercial publishers based in or originating from

[24] But see de Mourat et al. (2020) for an experiment that "actively play[s] with scholarly formats to gather collectives of concerned participants in new ways" (p. 106), and Peters et al. (2021) for a reappraisal of the edited collection as "a scholarly model of community based on collaboration, trust, and mutual obligation in pursuit of a wider good" (p. 284).

[25] Meaning that the individual researcher and "print-derived notions such as 'authorship'" take second place to the overall system of communication (Guédon, 2017).

continental Europe worked together to pursue their own agenda of openness. In contrast with Bernal's proposals, their conception of openness was not so much focused on rationalizing the publishing apparatus, but rather on redefining what counted as internationally significant research. The key element of their agenda was to counterbalance the hegemony of American and British journals. The key strategy was to transfer the cost of journal publishing from the author to the user through the introduction of a subscription-based system. As a result, the periodical literature became more inclusive of knowledge produced by new or marginalized scientific communities: in emerging research fields, in formerly Nazi-occupied Europe, in the Eastern Bloc, with Marxist leanings, with nostalgias for prewar conditions of science, and, to a limited extent, in the Global South.

In line with the studies of Shapin (2008) and Csiszar (2018)—and with other scholarship that has drawn attention to the heterogeneity of (scientific) modernity—my exploration of mid-twentieth-century scientific publishing has shown that notions of good science and proper publication have been fluid and shaped by epistemic and non-epistemic values. Additionally, "external" commercial actors changed scholarly publishing not just for the worse (from today's point of view) but also for the better. Improving scholarly communication is therefore not simply a matter of purifying and rectifying the ways in which knowledge claims are warranted, disseminated, and recorded. It also requires a reconsideration of the purpose and public of science. As the myth of scientific objectivity has been widely recognized in the public culture (Shapin, (2019, December 2).), paying lip service to public engagement and communicating scientific results more transparently and excessively will not suffice. Open science should be more open to explaining how science works on an everyday basis while still being a (if not the most) reliable way of knowing, and to understanding and integrating public conceptions of the goals of the scientific enterprise. Moreover, considering that formats of scientific publication and presumptions about the nature of the scientific life have always been fluid, open science should allow for a plurality of formats and vocations. Perhaps this might even help scientists find a new sense of calling.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

References

Abir-Am, P. G. (1980). From biochemistry to molecular biology: DNA and the acculturated journey of the critic of science Erwin Chargaff. *History and Philosophy of the Life Sciences, 2*(1), 3–60. https://www.jstor.org/stable/23328242.

Abir-Am, P. G. (1992). The politics of macromolecules: Molecular biologists, biochemists, and rhetoric. *Osiris, 7*, 164–191. https://doi.org/10.1086/368709

Abir-Am, P.G., & Elliott, C.A. (Eds.). (1999). Commemorative practices in science: Historical perspectives on the politics of collective memory [Special volume]. Osiris, 14.

Adair, R. K. (2008). *Physical Review Letters*: Sam Goudsmit's vision. *Physical Review Letters, 100*(2), 1–4. https://doi.org/10.1103/PhysRevLett.100.020001

Albornoz, D., Huang, M., Martin, I. M., Mateus, M., Touré, A. Y., & Chan., L.. (2018). Framing power: Tracing key discourses in open science policies. *ELPUB, 2018*. https://doi.org/10.4000/proceedings.elpub.2018.23

Anderson, B. (1998). Nationalism, identity, and the world-in-motion: On the logics of seriality. In P. Cheah, & B. Robbins (Eds.), *Cosmopolitics: Thinking and feeling beyond the nation* (pp. 117–133). Minneapolis: University of Minnesota Press.

Andriesse, C. D. (2008). *Dutch messengers: A history of science publishing, 1930–1980*. Leiden: Brill.

Baneke, D. (2011). Synthetic technocracy: Dutch scientific intellectuals in science, society and culture, 1880–1950. *The British Journal for the History of Science, 44*(1), 89–113. https://doi.org/10.1017/S000708741000004X

Bartling, S., & Friesike, S. (Eds.). (2013). *Opening science: The evolving guide on how the internet is changing research, collaboration and scholarly publishing*. Cham: Springer Open.

Brown, G. E., & ter Haar, D. (1962). Editorial preface. *Physics Letters, 1*(1), 3. https://doi.org/10.1016/0031-9163(62)90256-1

Chargaff, E. (1963). First steps towards a chemistry of heredity. In E. Chargaff (Ed.), *Essays on nucleic acids* (pp. 109–125). Amsterdam: Elsevier.

Chargaff, E. (1963). Amphisbaena. In E. Chargaff (Ed.), *Essays on nucleic acids* (pp. 174–199). Amsterdam: Elsevier.

Chargaff, E. (1978). *Heraclitean fire: Sketches from a life before nature*. New York: The Rockefeller University Press.

Chargaff, E. (1987). Engineering a molecular nightmare. *Nature, 327*, 199–200. https://doi.org/10.1038/327199a0

Chargaff, E. (1989). In retrospect: A commentary. *Biochimica et Biophysica Acta (BBA): General Subjects, 1000*, 15–16. https://doi.org/10.1016/S0006-3002(89)80004-6

Cohoon, J., & Howison, J. (2021). Norms and open systems in open science. *Information & Culture, 56*(2), 115–137. https://doi.org/10.7560/IC56201

Csiszar, A. (2010). Seriality and the search for order: Scientific print and its problems during the late nineteenth century. *History of Science, 48*(3/4), 399–434. https://doi.org/10.1177/007327531004800306

Csiszar, A. (2017). The catalogue that made metrics, and changed science. *Nature, 551*, 163–165. https://doi.org/10.1038/551163a

Csiszar, A. (2018). *The scientific journal: Authorship and the politics of knowledge in the nineteenth century*. Chicago: University of Chicago Press.

Daling, D. (2006). The encyclopaedia as pioneer of the journal: The early years of Elsevier's Scientific Publishing Company, 1936–1956. In M. van Delft, F. de Glas, & J. Salman (Eds.), *New perspectives in book history: Contributions from the Low Countries* (pp. 31–48). Zutphen: Walburg Pers.

Daling, D. (2011). *Stofwisselingen: Nederlandse uitgevers en de heruitvinding van het natuurwetenschappelijke tijdschrift, 1945–1970*. Zutphen: Walburg Pers.

Daston, L., & Galison, P. (2007). *Objectivity*. New York: Zone Books.

de Chadarevian, S. (2002). *Designs for life: Molecular biology after World War II*. Cambridge: Cambridge University Press.

de Mourat, R., Ricci, D., & Latour, B. (2020). How does a format make a public? In M. P. Eve, & J. Gray (Eds.), *Reassembling scholarly communications: Histories, infrastructures, and global politics of open access* (pp. 103–112). Cambridge: MIT Press.

East, H. (1998). Professor Bernal's "insidious and cavalier proposals": The Royal Society Scientific Information Conference, 1948. *Journal of Documentation, 54*(3), 293–302. https://doi.org/10.1108/EUM0000000007172

Edelman, H. (2004). Maurits Dekker and Eric Proskauer: A synergy of talent in exile: Part I. *Logos, 15*(4), 188–193. https://doi.org/10.2959/logo.2004.15.4.188

Faye, J. (2019). Copenhagen interpretation of quantum mechanics. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy*. https://plato.stanford.edu/archives/win2019/entries/qm-copenhagen/.

Fidler, F., & Wilcox, J. (2021). Reproducibility of scientific results. In E. N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy*. https://plato.stanford.edu/archives/sum2021/entries/scientific-reproducibility/.

Fredriksson, E. H. (2001). The Dutch publishing scene: Elsevier and North-Holland. In E. H. Fredriksson (Ed.), *A century of science publishing: A collection of essays* (pp. 61–76). Amsterdam: IOS Press.

Fyfe, A. (2016). Journals and periodicals. In B. V. Lightman (Ed.), *A companion to the history of science* (pp. 387–399). Chichester: John Wiley & Sons Ltd.

Gordin, M. D. (2015). *Scientific Babel: How science was done before and after global English*. Chicago: University of Chicago Press.

Goudsmit, S. A. (1962). Editorial: *Physics Letters*. *Physical Review Letters, 8*(8), 303. https://doi.org/10.1103/PhysRevLett.8.303

Grote, M. (2020). Total knowledge? Encyclopedic handbooks in the twentieth-century chemical and life sciences. *BJHS Themes, 5*, 187–203. https://doi.org/10.1017/bjt.2020.11

Gruber, M., Steyn Parvé, E. P., Wyckoff, R. W. G., Cori, C. F., Roche, J., Brachet, J., … Slater, E. C. (1965). Hendrik Gerrit Koob Westenbrink, 1901–1964. *Biochimica et Biophysica Acta (BBA): General Subjects, 97*(1), i–xix. https://doi.org/10.1016/0304-4165(65)90261-8

Guédon, J.-C. (2017, February 23). Open access: Toward the internet of the mind. https://www.budapestopenaccessinitiative.org/boai15/open-access-toward-the-internet-of-the-mind/.

Elsevier. (2023). Open access. https://www.elsevier.com/open-access.

Eve, M.P., & Gray, J. (Eds.). (2020). Reassembling scholarly communications: Histories, infrastructures, and global politics of open access. Cambridge: MIT Press.

Guédon, J.-C. (2021, April 21). Scholarly communication and scholarly publishing. Open Access Scholarly Publishing Association. https://oaspa.org/guest-post-by-jean-claude-guedon-scholarly-communication-and-scholarly-publishing/.

Harrington, A. (1996). *Reenchanted science: Holism in German culture from Wilhelm II to Hitler*. Princeton: Princeton University Press.

Hobsbawm, E. (1999). Preface. In Swann, B., & Aprahamian, F. (Eds.), J.D. Bernal: A life in science and politics (pp. ix–xx). London: Verso.

Hopwood, N., Schaffer, S., & Secord, J. (2010). Seriality and scientific objects in the nineteenth century. *History of Science, 48*(3/4), 251–285. https://doi.org/10.1177/007327531004800301

Jurchen, S. (2020). Open access and the serials crisis: The role of academic libraries. *Technical Services Quarterly, 37*(2), 160–170. https://doi.org/10.1080/07317131.2020.1728136

Landé, A. (1957). An anti-review: Comments on the review of *Foundations of quantum mechanics: A study in continuity and symmetry*. *Nuclear Physics, 3*(1), 132–134. https://doi.org/10.1016/0029-5582(57)90062-7

Lerner, B. R. (2015). Seriality and modernity: *L'almanach des Mystères de Paris*. *L'Esprit Créateur, 55*(3), 127–139.

Meinel, C. (Ed.). (1997). *Fachschrifttum, Bibliothek und Naturwissenschaft im 19. und 20. Jahrhundert*. Wiesbaden: Harrassowitz.

Law, G., & Patten, R.L. (2009). The serial revolution. In McKitterick, D. (Ed.), The Cambridge history of the book in Britain: Vol. 6. 1830–1914 (pp. 144–171). Cambridge: Cambridge University Press.

Merton, R.K. (1973). The normative structure of science. In Merton, R.K., The sociology of science: Theoretical and empirical investigations (N.W. Storer, Ed.) (pp. 267–278). Chicago: University of Chicago Press. Original article published 1942 as Science and technology in a democratic order.

Mirowski, P. (2018). The future(s) of open science. *Social Studies of Science, 48*(2), 171–203. https://doi.org/10.1177/0306312718772086

Muddiman, D. (2003). Red information scientist: The information career of J. D. Bernal. *Journal of Documentation, 59*(4), 387–409. https://doi.org/10.1108/00220410310485677

Noel, M. (2020). Back to disciplines: Exploring the stability of publication regimes in chemistry: The case of the Journal of the American Chemical Society (1879–2010). *Humanities and Social Sciences Communications, 7*, 1–13. https://doi.org/10.1057/s41599-020-00543-6

Pais, A. (1997). *A tale of two continents: A physicist's life in a turbulent world*. Oxford: Oxford University Press.

Penders, B., de Rijcke, S., & Holbrook, J. B. (2020). Science's moral economy of repair: Replication and the circulation of reference. *Accountability in Research, 27*(2), 107–113. https://doi.org/10.1080/08989621.2020.1720659

Peters, M. A., Jandrić, P., & Hayes, S. (2021). Revisiting the concept of the edited collection: Bioinformational philosophy and postdigital knowledge ecologies. *Postdigital Science and Education, 3*, 283–293. https://doi.org/10.1007/s42438-021-00216-w

Pflüger, E. (1877). Die Physiologie und ihre Zukunft. *Archiv für die gesamte Physiologie des Menschen und der Tiere, 15*, 361–365. https://doi.org/10.1007/BF01628355

Reinisch, J. (2000). *The Society for Freedom in Science, 1940–1963* [MSc dissertation, Imperial College London]. https://www.academia.edu/1088487/The_Society_for_Freedom_in_Science_1940_1963.

Rheinberger, H.-J. (1997). *Toward a history of epistemic things: Synthesizing proteins in the test tube*. Stanford: Stanford University Press.

Roberts, E. A. (2005). From the history of science to the science of history: Scientists and historians in the shaping of British Marxist theory. *Science & Society, 69*(4), 529–558. https://doi.org/10.1521/siso.2005.69.4.529

Rosemann, R., Bleibtreu, B., & A., & Runkel, F.. (1929). Zu Eduard Pflügers 100. Geburtstag am 7. Juni 1929: Festreden gehalten im Auditorium maximum der Universität Bonn. *Pflügers Archiv für die gesamte Physiologie des Menschen und der Tiere, 222*, 548–574. https://doi.org/10.1007/BF01755143

Rosenfeld, L. (1956). Editor's notice. *Nuclear Physics, 1*(1), 1. https://doi.org/10.1016/0029-5582(56)90124-9

Rosenfeld, L. (1956b). [Review of the book Foundations of quantum theory: A study in continuity and symmetry, by A. Landé]. *Nuclear Physics, 1*(2), 133–134. 10.1016/0029-5582(56)90067-0.

Rosenfeld, L. (1956c). [Review of the book Multipole fields, by M. E. Rose]. *Nuclear Physics, 1*(5), 379. 10.1016/0029-5582(56)90012-8.

Rosenfeld, L. (1956d). [Review of the book Elementary theory of nuclear shell structure, by M. G. Mayer, & J. H. D. Jensen]. *Nuclear Physics, 1*(9), 670–671. 10.1016/0029-5582(56)90078-5.

Rosenfeld, L. (1957). Editor's note. *Nuclear Physics, 3*(1), 132. https://doi.org/10.1016/0029-5582(57)90062-7

Rosenfeld, L. (1964). Indexing of papers on nuclear physics. *Nuclear Physics, 54*, n.p.

Rosenfeld, L. (1966). Reorganization of the journal. Nuclear Physics, 87(1), v–viii. 10.1016/0029-5582(66)90354-3.

Shapin, S. (2008). *The scientific life: A moral history of a late modern vocation*. Chicago: University of Chicago Press.

Science periodicals in the nineteenth and twenty-first centuries [Special issue]. Notes and RecordsShuttleworth, S., & Charnley, B. (Eds.). *The Royal Society Journal of the History of Science, 70*(4), (2016). https://doi.org/10.1098/rsnr.2016.0026

Simon, J. (2022). Scientific publishing: Agents, genres, technique and the making of knowledge. *Histories, 2*, 516–541. https://doi.org/10.3390/histories2040035

Skaar Jacobsen, A. (2012). *Léon Rosenfeld: Physics, philosophy, and politics in the twentieth century*. Singapore: World Scientific.

Slater, E. C. (1986). *Biochimica et Biophysica Acta: The story of a biochemical journal*. Amsterdam: Elsevier.

Tollebeek, J. (2015). Commemorative practices in the humanities around 1900. *Advances in Historical Studies, 4*(3), 216–231. https://doi.org/10.4236/ahs.2015.43017

van Leeuwen, J. K. W. (1980). The decisive years for international science publishing in the Netherlands after the Second World War. In A. J. Meadows (Ed.), *Development of science publishing in Europe* (pp. 251–268). Amsterdam: Elsevier.

Verdoorn, F. (1948). The development of scientific publications and their importance in the promotion of international scientific relations. *Science, 107*, 492–497. https://doi.org/10.1126/science.107.2785.492

Weber, M. (1919). *Wissenschaft als Beruf*. München: Verlag von Duncker & Humblot. Original lecture given 1917.

Sarkowski, H. (1996). *Springer-Verlag: History of a scientific publishing house: Part 1. 1842–1945: Foundation, maturation, adversity* (G. Graham, Trans.). Berlin: Springer-Verlag.

Shapin, S. (2019, December 2). Is there a crisis of truth? *Los Angeles Review of Books*. https://lareviewofbooks.org/article/is-there-a-crisis-of-truth/.