Standardization in science: effects and issues of guidelines for biomedical reporting

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Chapter 8

Standardization in science. Discussion and conclusions

"Do organisms need an impact factor?"

(Piekniewska et al., 2025)

In this dissertation, I provided an in-depth look at the “preferred reporting items for systematic reviews and meta-analyses” (PRISMA), one of the most important reporting guidelines in biomedical research and addressed the question of how scholarly communication in biomedicine is standardized. We have traveled through important moments in the creation, dissemination, and usage of the guideline and have taken different perspectives, from the politics of guideline formation, procedural objectivity, and standardization of science up to a bibliometric perspective. In the following, I will develop some broader perspectives and ideas that emerged from the insights I gained throughout my research.

The most essential presupposition was the inevitableness of scholarly communication as a prerequisite for scientific knowledge, so that only reception (and comprehension) can turn information into knowledge. Against this background, reporting guidelines must be understood as interventions that attempt to (re-)contextualize scientific texts by connecting them with moments of comprehension and understanding. The developments in scholarly communication during the postwar era—the growth of the research sector and especially the pressure to publish—have created and nurtured an imbalance between the construction and the consumption of scientific knowledge. Research authors focused increasingly on the production of countable (and citable) documents, which came at the cost of publishing the most important information and writing in a thorough, clear, and comprehensible way. Rather, it seemed that science evaluators and research managers became the primary audience for scientific authors. This audience valued the contexts of production and codified forms of reception over the contents of a publication and the implications of scientific findings. As a result, the

confusion between scholarly communication and publishing journal papers became a general trend in biomedicine.

The introduction of reporting guidelines must be understood as an empowerment of the critical reader or consumer, who reads and comprehends publications to learn what others found out and which questions remain. Reporting guidelines re-emphasize the critical look at the contents of the publication, and they do so in a formalized and procedural way. As we have seen throughout this research, reporting guidelines postulate and represent a quality dimension in science which suggests being generally applicable in biomedicine while remaining independent of quality assurance that is based on peer review. Rather, reporting guidelines attempt to visualize transparency as a scientific quality in a systematic and non-exclusive way. They promise to guarantee the fulfillment of transparency irrespective of where, when and how a text was published. Therefore, they turn transparency into an open and visible criterion that can be achieved, assessed, or rejected. In other words, if scholarly communication is understood as a market, there were entry barriers (peer review) but no product standards. Reporting guidelines would introduce a minimum standard into such a market.

But we have seen how this development is far from being a self-evident exercise in reemphasizing ideals of the scientific method. Instead, the construction and dissemination of PRISMA was a socio-political act that had to be managed, as we have seen in chapter 3. Guideline documents are boundary objects in a contingent reform process that spans multiple intellectual and social dimensions in biomedical research. Guidelines consist of intellectual narratives about epistemic problems and potential solutions that are not only persuasive stories but rather become scientific stories backed up by formal investigation and (meta)scientific evidence. But scientific success does not always depend on just uttering the best argument backed by convincing results. For the guidelines to be successful, they had to be integrated into the social configurations of modern research. Guideline developers enrolled experts from different fields and practice domains, among them editors and publishing experts. The different perspectives and different backgrounds on the guideline promised to generalize its conceptualizations and statements and increase the robustness. But these ties also established a democratic and representational process to mitigate intellectual disputes and disciplinary politics and, in addition, increased the impact and visibility of the overall project.

The strategic use of science's social domains to make PRISMA successful is not a mere sociological interpretation. Guideline developers were aware of the importance of the

social dimensions of scholarly communication, either implicitly or explicitly. Especially the effortful, troublesome, and otherwise unethical multipublication of the guideline illustrates how PRISMA's developers knew very well that “being right” with regards to ideals of objectivity or rigor is not enough. Instead, new standards must be communicated to the different biomedical subfields through established communication channels, i.e., central journals or non-English languages. Social factors and local contingencies impact the application of PRISMA by research writers in a similar vein.

The PRISMA guidelines transformed the practices of writers and readers of systematic reviews, as well as the review documents themselves. Transforming the product, its social context, and epistemic characteristics, PRISMA has (re)shaped the systematic review genre. This can be illustrated in three points. First, as we have seen in chapter 3 and 4, compliance with PRISMA results in the homogenization of some document characteristics, such as the title or the structure of systematic review documents. Further, the addition of technical elements such as the flowchart and the PRISMA checklist, for which PRISMA provides templates, turns published systematic reviews into similar-looking documents that can be identified as systematic reviews *from afar*, i.e., without reading the text in detail. In chapter 7, we have seen how reporting guidelines reshuffle informational flows from the conduct of the project to the final manuscript. They redefine what counts as required, important, or superfluous information about the project. Consequentially, other codified information or documentation of the research process is published as adjacent documents, such as supplements, protocols, or preprints.

Second, with the changes in the characteristics of the genre come changes in the social and epistemic expectations that are tied to the genre. The very intention to improve the reporting of biomedical research by issuing formal standards can be considered as a normative dimension in this regard. In chapter 3 and 4, I have investigated the origin of PRISMA and we have seen how experts argued that systematic reviews lack in reporting quality and become less valuable for readers, such as doctors, policymakers, or other researchers. Thus, there was a deterioration of the relationship between the genre and its social expectations and promises. Further, the experts associated new characteristics and criteria that must be fulfilled to reinstate and repair this relationship.

Systematic review authors and PRISMA users accepted this normative authority of the reporting guideline. Even though chapter 7 has illustrated the many individual contingencies and complexities that can lead authors to deviate from the script, the normative story of the guideline remained intact. As such, authors widely believed that

the guidelines would improve their writing and that of their discipline as a whole. They further considered guideline compliance in their own reading or (peer) reviewing activities and thereby turned PRISMA into an evaluative criterion. But the guideline was not merely a technical script that would turn reading and peer reviewing into procedural tasks in such contexts. Moreover, the mere appearance of PRISMA usage can be understood as a sign of compliance and the goodwill of the author. Therefore, similar to how a PRISMA-based systematic review document can be identified as such without close reading, its reporting quality and compliance with the appropriate standards of doing science can be spotted from afar as well.

Lastly, the reformulation of the systematic review genre still involves some mechanical and more algorithmic social filters. We have identified many differences in how academic communities may react to the introduction of the guideline or how they implement it into their disciplinary canon. But at the same time, PRISMA-based reporting impacted some powerful dimensions in the social structure of science, if not the most powerful ones. First, PRISMA has been endorsed by many biomedical journals, in some of which it has even become a mandatory submission requirement. In this way, non-compliant systematic reviews simply do not become publishable anymore. Though there are alternative journals that might not formally require PRISMA compliance, so that an author always has the option to revert to such journals and avoid the standard.[58]

However, the current reward system in science and its evaluatory regime shift the attention of researchers towards high-impact venues and ways of doing research. In this way, published systematic reviews in high-impact journals become models or templates for future systematic reviews, even if these do not end up in the same journal. The bibliometric analyses in chapter 5 and 6 have indicated this relationship as they revealed the systematically higher citation impact of PRISMA-based systematic reviews. Although we have been careful with interpreting any sense of cause and effect, we nevertheless have seen how PRISMA is associated with high citation impact – a characteristic that has become all too important in modern biomedicine. All in all, standards can become ubiquitous and pervasive because authors emulate the highly standardized work of

[58] This is the reason why I have compared PRISMA-based standardization to soft-law regulation, or “Selbstzwang” in Chapter 1, rather than formal gatekeeping or “Fremdzwang” (Penders, 2022, p. 110) as in the case of industrial standards that mandate access to markets or ICT networks in a more technical or binary manner.

others. Slowly but steadily, forms of systematic reviewing that do not comply with reporting guidelines become less common.

Taken together, I have identified several dimensions and vectors that were played out in the successful attempt to reform the reporting of systematic reviewing in the biomedical sciences. So, what do the social laws of standardization look like? Unfortunately, this question cannot be answered universally. The case presented here rather verified the high dimensionality and context dependency of standardization, which has been proven several times before (Timmermans & Epstein, 2010). So, although we identified some important relationships that can be elaborated further, for example, the role of the seniority of researchers in the application and responsibility for standards, the complexity of the interrelated and mutually interdependent factors in science itself became visible.

In this regard, we have gotten a sense of how writing practice shapes disciplinary culture, disciplinary culture shapes research evaluation, and research evaluation shapes writing practices. Of course, this can happen in sequences or epicycles of methodological advancement, i.e., "self-perpetuating cycles of research, critique, and intervention" (Peterson & Panofsky, 2023, p. 160), or simultaneously and in parallel. Further, while one researcher complies with standards because she is convinced of their intellectual power and usefulness, another researcher is solely incentivized by the submission requirement of the journal. In this sense, it seems impossible to express the determinants of science reform with respect to individual cause and effect relationships or the agency and authority of certain actors. Therefore, one might consider the sociology of standardization as a shared set of methods and analytical frameworks, rather than as a canon of established theories and causal relationships that characterize standardization universally.

Consequently, I am inclined to view successful science reform as a quantitative phenomenon. This implies that an increasing number of scholars adapt to the new situation and change their behavior accordingly. Once this number becomes large enough, a tipping or transition point is reached. From this point onwards, the mutual interrelation of authors, conventions, and evaluators forms a self-sustaining and perpetuating movement that slowly but surely enrolls more individuals and types of actors, such as journals, evaluators, or funding bodies. At the same time, the new behavior or practice is perceived as the new norm or standard so that we can speak of a new quality that emerged in biomedical research. In that sense, science reform, as in the case presented here, is not as much a moment of scientific revolution as some of its proponents think. The changes in the biomedical documentation practices that come

with reporting guidelines are only little, yet it is their broadness of application that makes them fundamental. Thus, the prior and posterior states are neither incommensurable nor non-cumulative in the Kuhnian sense (Kuhn, 1962/1996). In addition, even the idea of a tipping point might be more of a metaphor or an *anticipated retrospection*, as turning points are always constructs of historization (Launius, 2007).

So far, I have concluded on the interaction between the PRISMA reporting guidelines, the genre of systematic reviews, and their respective social constituency. These conclusions follow more straightforwardly from the results of the individual chapters and their combination. But understanding PRISMA as a case of a broader cultural change in science, further abstractions and conceptualizations can be made. On the following pages, I will present some additional conclusions and outlooks on scientific change on a broader scale. I will do this in a more essayistic manner, which involves additional suppositions, the consideration of adjacent topics, and associations of potential developments. Although these perspectives go beyond the previously discussed material, they support its contextualization and assessment and suggest future avenues to explore.

8.1 The lone genius is dead, long live the lone genius!

This section will discuss aspects of the standardization of systematic reviewing with regard to sociological aspects of scientific work. In particular, I will briefly discuss how the fractionalization and professionalization of epistemic tasks show that research experts already employed a self-concept of research "without legend" (Kitcher, 1993, p. 3), i.e., by understanding their intellectual endeavors as forms of work.

The lone genius narrative is a famous public story about how science works. It spotlights individual scientists and their innovative spirit, individual capabilities, and devotion, which led to the major breakthroughs and scientific discoveries (Charney, 2003). Although being more of a romantic but wrong story about science, factors such as the vibe of the Nobel prizes, which are always awarded to very few individuals, or the academic visibility of extremely high-cited researchers nurture this narrative until today (Aksnes & Aagaard, 2021). But it is an evidential fact for the majority of contemporary researchers that doing research is teamwork and that no single expert is solely responsible for the success or failure of a project or a breakthrough discovery. Decades of science studies underpinned this conclusion with systematic evidence that likewise proved the lone genius narrative to be an illusionary story.

My research has shown how biomedical research experts arrived at the sober truth that the lone genius narrative is inappropriate. The transparency crisis and the way it is

addressed by the introduction of reporting guidelines show how biomedical research can be a human activity that is as unexciting and mundane as any other. First of all, the proclamation of the transparency crisis shows how individual researchers are believed to be fallible even with regard to tasks as basic as writing down the path of an experiment.[59] However, the crisis and the more constructive attempts to address it reveal how some experts developed a more modest and unenthusiastic view on scientific practice. Instead of messing around with the minds of geniuses, errors and quality issues are conceptualized as practical problems that can be addressed and solved. The introduction of reporting guidelines as checklists analogous to medical treatment protocols supports this interpretation.

The very idea behind reporting guidelines is based on the conception of science as work or craft. First, they depict scientific practice as a set of tasks that can be performed at different levels of experience, (technical) proficiency, or just valuation and attention. These tasks can be identified and abstracted, generalized, or understood as routine practices that defy any understanding of scientific practice as a highly individualistic, incomparable, and black-boxed type of intellectual endeavor. Instead, abstracting tasks enables method experts to discuss and formulate behavioral rules that subsequently influence the daily practices of thousands of systematic review authors, albeit to varying degrees. Thus, even though standards represent the process of scientific discovery in an idealized way, they nevertheless define practices below any black-boxed conceptions of intellectual inquiry or scientific method.

Reporting guidelines and the standardization of biomedical research reveal how research experts enact a down-to-earth error culture, rather than a naïve idealization of epistemology that we can find in their metascientific comments and papers. In this culture, negotiations of accountability and responsibility superseded scandalizations of mistakes in the long run. Even further, the very idea of *error* became differentiated into knowledge-context relationships that favor questions of relevance over questions of quality. This differentiation means that a certain type of evidence or process of evidence generation is not devalued as plainly wrong or erroneous, but rather irrelevant or improper to a certain context. The occasional depiction of science as an information market can be interpreted in similar vein. Although it focuses solely on quality, the

[59] Metascientists publicly debate the role of intentional fraud in addition to the fallibility of researchers in writing papers. However, intentional yet undetected fraud certainly falls under the concept of the lone genius. Similarly, it appears that the public is as interested in the big fraudsters of science as it is in Nobel Laureates or breakthrough inventors.

analogy nevertheless implies that there is a variance in product quality that is tied to different prices and must be considered as normal or even integral to the functioning of the market (e.g., Vazire, 2017). Or to put it more illustratively, when speaking of variance, we can conclude that *normal science* also implies a normal distribution in the quality and sophistication of scientific publications.[60]

So, what about the scandalizations and great accusations that I referenced in the introduction of this book? We have considered the many, often very vocal criticisms of medical research practices, for example, expressed by Doug Altman, John Ioannidis, or others. Most of them center around valid reservations and important issues with regard to industry-sponsored research, statistical practices and their reporting, and the consequences of the publish-or-perish culture in modern academia. I also think that most of these accusations are formulated with good intent and a more modest and informed picture about science than the crisis discourse suggests.[61]

However, I have shown how many of these accusations aim at political goals more than they serve epistemic or intellectual ones. Metascientists intend to move scientific culture and change the behaviors and beliefs of thousands, if not millions, of researchers worldwide. In that sense, their language might be better considered as a political device or marketing mechanism that legitimizes the wider science reform project inside and outside of science. In addition, bold and simplified statements often focus on the most essential part of an argument, while complexity and nuance increase the potential for misunderstanding. The discourse further established a metascientific discipline and set up modes for intervention and implementation, as we have seen it for reporting guidelines. It is then granted that there will be metascientists and science reformers who will stick to their strong language and remain convinced that variance is nothing less than fraud, misconduct, or unethical behavior of individual scientists.

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[60] In fact, the traditional statistical approach taken in prolific metascientific studies supports this picture. Because most hypotheses in medicine and psychology are accepted or rejected with regard to generous p-value thresholds, some of the results are wrong simply due to chance: "Publication bias may be defined simply: significant results are preferred for publication. Attention was drawn to it as early as 1963, and it has been 'rediscovered' several times since" (Newcombe, 1987, p. 657; for the latest rediscovery, see Ioannidis, 2005).

[61] This accounts for the more experienced systematic reviewers and guideline developers I interviewed. I enjoyed talking to metascientific experts about these issues during conferences or summer schools, and I often encountered more nuanced and informed beliefs about scientific practice compared to what they write in their papers and essays.

Scholars from science studies should take these norms and narratives seriously. In this regard, we have encountered the important and performative role of such normative depictions, or narratives of proper science, promoted by metascientific advocates throughout this book. However, the case of reporting guidelines also shows that such narratives must be investigated with respect to actual research practices, interventions, and implementations that are based upon them. Studying science reform and its practical implications should not overfocus on the narratives of reform alone. In that regard, I argued in chapter 7 how overly idealized narratives can do more harm than good, but at the same time, such narratives have proven to be less impactful on the actual working practices of systematic reviewers.

However, we should avoid criticizing and rejecting these narratives just because they appear exaggerated and incorrect. Idealized narratives become problematic in the moment when they are performative, i.e., drive or justify problematic actions and behaviors. But they are less of a problem when the interventions based thereon are inclusive and more sensible for the nuances and complexities of day-to-day research practice. Reporting guidelines provide an illustrative example in this regard. On one hand, a moral obligation towards transparency justifies them. They allow for deviation and are not mandatory, but they become pervasive in a more indirect and less controversial way. Therefore, scholars from the social studies of science should not overfocus or even fetishize criticisms of metascientists or the science reform movement solely based on their idealized and inappropriate stories about scientific discovery.

All in all, we have seen how scientists start to understand their endeavor as work or craft, rather than a holy reading of the bible of nature.[62] If the behavior of researchers can be steered by protocols, and the introduction of such protocols does not spur broader moral panics about the creative freedoms in science, the overall business might be less the course of brilliant minds than mere acts of intellectual work. However, understanding research as a form of work may not seem particularly revolutionary. In fact, the science studies traditionally viewed doing research practice from the perspective of producing or manufacturing knowledge or spoke of an industrialization of science in more general patterns of interpretation (Erlach, 2022; Knorr-Cetina, 1981).

[62] Portrayal of the famous metaphor that science is reading the book of nature. This phrase was used in the history of science to justify and negotiate the coexistence of theological and scientific authority, e.g., to address the conflict between Galileo Galilei and the Catholic Church. The former is eligible to express *facts* about the spiritual world, while the latter can *only* approach the natural world (Finocchiaro, 2009).

However, my research has revealed how trends and innovations in science impact professions and career opportunities on a wider scale.

We have seen how the guideline co-constructs the professional ethos of systematic reviewing. In doing so, it also manifests a version of science in which a rather broad and abstract concept of knowledge generation is disassembled into individual scientific tasks that can be outlined, separated, and professionalized. Similar moments of such professionalization have been mentioned anecdotally. Consider the roles of statisticians, librarians, information specialists, data stewards, and research software developers. Similarly, I have suggested that new roles and responsibilities can emerge adjacent to systematic reviewing, for example, with regard to managing supplemental information or protocol preregistration. In this reading of open science, teamwork as opposed to lonely masterminding around means much more than just joint resources and efforts. Rather, it means that epistemic processes and the degree of professionalism with which they are carried out become open and plannable. If we examine this further, we might realize that future project designs will express their resource needs through their demand for specific intellectual or epistemic practices. This perspective gives rise to further questions, which I will sketch out in brief.

First, will the emergence of epistemic specialists and new types of research technicians challenge the model of the disciplinary scholar? Scholars from the science studies have shown how standardized combinations of theory and method are important packages for academic career building, and my research has uncovered some of the factors that turn systematic reviewing into a novel career opportunity. But like data and software stewarding, systematic reviewing is not a scientific discipline in the traditional sense. Rather, it is a profession that can and must be performed in collaboration with different biomedical domains. In contrast to more service oriented roles in science, however, its characterization as “secondary research” (see Glass, 1976; Moreira, 2007) suggests more proximity to what we might consider as genuine research practice. The fact that its outcomes are countable and citable journal papers support this perspective.

However, we might nevertheless question the future of professional roles in science more generally. If more of such secondary tasks become professionalized career paths, the disciplinary scholar and her research questions might end up being surrounded by epistemic technicians who share little or nothing of her domain knowledge. Such settings give rise to questions of social power and negotiation or change dominant interpretations of what interdisciplinarity means. In this regard, we should put more emphasis on the interplay of techniques, methods, and skills, rather than theories, concepts, or fields when we study scientific collaborations and interdisciplinarity.

Ironically, this would rather refuel the impression that the domain expert, as we romanticize her in public stories of science, will be alone again.

Although the idea of the lone genius is fundamentally wrong, it preserved the concept of *the researcher* as a category or boundary role in modern society until today. It a narrative for the wider research profession that is effective in wider society and protects the legitimization and autonomy of the academic sector (Abbott, 1988). We find ourselves in a contradictory situation: sociologists and scientists alike know too well that *doing research* is not an existing task or practice. Rather, we collect and analyze data, develop research software, design and run analysis code, or write scientific papers. Thus, we are data collectors, analyzers, statisticians, developers, or writers, from time to time or simultaneously. Some researchers may favor one of these roles over the others. Yet we want to be researchers, because the individual roles sound smaller, less defined, and of more uncertain societal relevance. Career-wise, these roles also come with much less acclaim and visibility in contemporary academia. An illustrative example is the domain of research software engineering, which, on the one hand, becomes increasingly professionalized as it strives for academic legitimization and resources, but which also finds many reservations against this role, as it can mean being recognized (and paid!) as a technician or service worker, rather than as a researcher (Baxter et al., 2012).

But there are also broader implications, which are illustrated by the professionalization of systematic reviewing. Traditionally, reviewing the literature, concluding on the current state of science, and defining the future research questions are done by experienced and eminent scholars. In this regard, eminent scholars can be interpreted as black boxes that carry broad and deep knowledge about a scientific topic (Restrepo Forero, 2003; see also Zuckerman & Merton, 1972). But the emergence of systematic reviewing and its professionalization turned the synthetization and aggregation of knowledge into a technical task that can be performed by anyone who acquires the required skills. Chapters 6 has shown how especially scholars with less publishing experience increasingly perform systematic reviews and make heavy use of PRISMA standards. In a similar vein, new standardizations and science reform proposals are developed into profession-based tasks that have been directed towards early career researchers (e.g., Debowski, 2012; Kent et al., 2022; Kohrs et al., 2026). These shifts from experienced-based into profession-based tasks challenge our understanding of the social rules and cultural conventions in science.

The complete redistribution of intellectual tasks can mean that epistemic experts are likewise on their paths to loneliness. In chapter 7, we discussed how the emergence and professionalization of new tasks and roles comes with a redistribution of responsibilities.

Now, it is the data manager who is responsible for having the data clean and ordinarily published. Likewise, it is the duty of the information specialist to develop a thorough but efficient literature search string for the systematic review. This means that errors (and potential breakthroughs, too) become traceable to individual tasks and work packages. Recent developments in the redefinition of scientific authorship illustrate this tendency. For instance, the "Contributor Roles Taxonomy" (CRediT) aims to reveal the intricate details of individual tasks involved in a publication, facilitating the equitable distribution of rewards but also responsibilities (Brand et al., 2015; Larivière et al., 2021). Against this background, we can imagine how accusations of scientific misconduct will be redirected to single individuals within the authorship team of a publication. But we might also ask if future research projects can be acclaimed for individual epistemic tasks and the contribution by highly specialized epistemic experts, in contrast to the overall research result.

Another yet related question is whether the proliferation of epistemic professions adjacent to domain research drives out disciplinary domain knowledge. If tasks such as systematic reviewing become professions, i.e., viable career paths in academia, those who follow these paths are not required or incentivized to become proficient in the domain in which they apply their expertise. Instead, systematic reviewing comes with its own agreed-upon set of concepts, assumptions, and theories.

In the case of statistics, it has been argued, though, that only a proper combination of domain knowledge and technical expertise can turn good statistics into a highly rewarded statistical analysis (e.g., Brownstein et al., 2019). But technical professions might establish a different type of theoretical knowledge, which requires a likewise deep understanding and reflection. For example, the systematic reviewing profession has agreed on theories about validation, robustness, and types of objectivity such as positivism and quantification. Most of these theories might be more implicit because they impregnate the epistemology of biomedicine much more broadly and generally. Others are unique to the profession and range from underdeveloped folk sayings such as “garbage in, garbage out” in knowledge synthesis (Egger et al., 2001, p. 478) to more profound theories, such as about how the epistemic superiority of meta-analysis is an inevitable result of quantitative methods (Hunt, 1999).

Acquiring theoretical and epistemic knowledge about systematic reviews and meta-analysis takes time. This means that professionalizing systematic reviewers cannot pay the same amount of attention towards domain knowledge compared to their colleagues. Moreover, theoretical knowledge about systematic reviewing is a form of epistemology itself. It thus sets the boundaries and constraints on what can be proven,

validated, or refuted in general. As a result, we might understand future developments in different fields with regard to theoretical and epistemic trends that emerge from technical professions, such as systematic reviewing, statistics, or research software development. While the extent to which such influences will transform individual research domains is hard to assess, the postwar trend towards outcomes research and interventional evaluations that permeated all major disciplines in medicine provides an example (Tanenbaum, 1994; see also Cochrane, 1972/1999).

In the case of systematic reviewing, or the development of reporting standards, the topics of this theoretical language are reminiscent of the issues and observations that are debated by metascientists today. In that sense, the professionalization of systematic reviewing can provide its own perspective on the role and emergence of the metasciences. I will sketch out and discuss this perspective in the next section.

8.2 From meta-analysis to meta-science

My research joined recent trends in the sociology of science to understand the emergence and function of the metasciences or similar endeavors that currently emerge in various disciplines. These movements have been prominently analyzed as social movements or scientific intellectual movements that formed around stories of scientific crisis and intend to propose and implement solutions (Peterson & Panofsky, 2023). Such framings focus on the analysis of dominant narratives, key figures and actors, resources and community infrastructures, and the mobilization of academic spheres. While they are useful in understanding emerging actor networks, for example, by drawing attention to the concentration of research funds or the founding of new research centers, they fall short of describing overall trends on a macro level or with respect to more abstract intellectual patterns (see also Nelson, 2024). By contrast, previous methodologies have emphasized the resurgence of traditional scientific principles, particularly the Mertonian ethos and Popperian Falsificationism (Derksen, 2019), the industrialization of science driven by ideals of economic efficiency (Erlach, 2022; Peterson & Panofsky, 2021), and a scientific etiquette incorporating bureaucratic reforms (Penders, 2022).

Other accounts identified an intellectual relationship between regulatory science and metascience, arguing that the latter basically absorbed the epistemic values and norms of the former. The proposed *ethos of regulatory science* emphasizes the effectiveness of planned action, the creation of epistemic validity by procedurality, the universality of approaches over context-driven specificity, and the importance of documentation and reporting. While this ethos originates in regulatory science, which is performed by

government agencies with goals such as regulation, standardization, or risk assessment, it backlashed or retroacted into spheres of public science (Keuck, 2022; Nelson, 2024).

However, I have encountered two different perspectives in how to interpret and make sense of the emergence of the metasciences and their intellectual configuration. First, in chapter 3, we have seen how intellectual predecessors to reporting guidelines can be found in the emergence of clinical practice guidelines with the rise of evidence-based medicine in the 1990s. There, I concluded that the idea of guidelines for researchers provides an intriguing reversal of the translation vector that is traditionally directed from the bench to the bedside. Subsequently, one might look out for discursive references, such as criticisms of cookbook science (see also Knaapen, 2014). Secondly, the practice of systematic reviewing itself serves as an intellectual precursor to the evaluation and validation of individual research. In the rest of this section, I will briefly spell out this argument.

The criticisms and problematizations of scientific reporting have a long tradition. In chapter 1.2, we have seen how the failure to reproduce scientific results or basically understand what researchers did solely on the published report has been criticized since the emergence of the experimental report. However, I have described it as an unbridgeable gap between conduct and reporting, which is a philosophical problem that is fundamental to scientific writing, and which is not solvable finitely but rather requires constant renegotiation. But with the developments in postwar biomedicine, such as the growth of science and scientific knowledge, the private and public desire for *relevant* research results, and the *publish-or-perish* culture in science, this gap became an effective and sizable problem for scholarly communication. We have seen how especially the 1980s witnessed a first wave of systematic assessments of reporting qualities of scientific papers. These studies were motivated by the growing awareness of a publication bias in science (e.g., Gøtzsche, 1987; Davidson, 1986; Andrew et al., 1994). Already in 1979, the International Committee of Medical Journal Editors (ICMJE) issued guidance on the formats of scientific papers. But what are the intellectual motivations that originally drove these studies? In fact, we must assume that the emergence and professionalization of systematic reviewing laid the intellectual groundwork for systematically assessing the quality of primary research.

I propose that systematic reviewers have been the first metascientists. While more narrative reviews or meta-analyses mean to synthesize the results or conclusions of a paper, systematic reviewing means to consider a study's methodological quality too. As explained above, the general rationale for systematic reviews was to account for potential publication bias (Bohlin, 2012; I. Chalmers et al., 2002). For example,

systematic reviewers report their search equations and retrieval procedures, try to identify unpublished studies and lacking data, or make an informed judgement about missing knowledge. But most importantly, a systematic reviewer makes a technical assessment of the epistemic power of the included studies.

The technical assessment of the primary study involves several dimensions and questions. What type of randomization has been performed in the trial? Was there concealment of group allocation so that not only the patient and the doctor but also the researcher are unaware? What is the statistical power of the analysis? All these questions are addressed even before the reviewer starts to aggregate or meta-analyze the results of the study, which is why the Cochrane Handbook's chapter on data aggregation strongly advises, "do not start here" (Higgins et al., 2019, p. 242). However, if the primary studies do not meet the quality criteria set out by the systematic reviewer, they are assigned a higher risk of bias and are assigned less weight in the overall conclusion of the study. Of course, such questions are not open to interpretation but have become standardized evaluation criteria developed and defined in systematic review handbooks and elsewhere (Cook et al., 1995; Higgins et al., 2019; Jadad et al., 1996).

For the systematic reviewer, other factors such as peer review or the prestige of the journal and authors lose their traditional evidential and certificatory status. Instead, evidential certification is subverted by method. In that sense, systematic reviewing can itself be interpreted as standardization of scientific reading or knowledge consumption, which is why Tiago Moreira described this as the process of disentanglement in his ethnography of a systematic review project, though with respect to the contents or results of included studies (Moreira, 2007).

Systematic reviewers developed an analytical understanding of epistemic characteristics. The formal assessment of primary studies involves schemes and typologies of study characteristics that can be used as epistemic scales, as we have seen during this research. With regard to the previous example, systematic reviewers had to develop an understanding of different randomization techniques, how they are commonly described and referred to, and what their strengths and weaknesses are. Based on these findings, they develop typologies of primary research practices and accompanying theories about their epistemic strengths, as suggested in the previous section. Such typologies are then further used to develop standardized evaluation schemes for the strength of evidence or risk of bias of primary studies or the reviews themselves (Solomon, 2015; Whiting et al., 2016). Thus, systematic reviewers had to get a systematic overview of the different primary research practices and develop an

analytical scheme to compare and weigh them against each other. Recent developments in guidelines for systematic reviews go even beyond and consider open science practices such as the publication of data as assessable criteria (see Wilkinson et al., 2023).

In the same sense, metascientists perform evaluations of the methodological quality of scientific studies, irrespective of their content. David Peterson and Aaron Panofsky (2023) have described the metasciences as a social intellectual movement that originates in a scientific field and involves scientific practices to achieve its aims. This means that parts of the movement resemble scientific research programs that investigate scientific practices and scientific methods, similar to sociologists or historians of science, yet with a stronger focus on practical application and interventionist intentions. In that sense, metascientific studies consist of diagnosing problems and developing new guidelines, techniques, or educational interventions as solutions to these problems (Freese & Peterson, 2018). They also show that metascientists were very successful in acquiring research funds, which led to the establishment of new research centers and the further institutionalization of the field.

Therefore, we can see how both domains, systematic reviewing and metascientific research, are similar in many regards. Even more, we are compelled to consider systematic reviewing as the intellectual predecessor of the metasciences. For instance, publication bias towards positive results has been an observation in meta-analyses in different fields (Fanelli, 2010). Illustratively, systematic review experts introduced the term “meta-epidemiology” (Naylor, 1997, p. 617) to describe their methods in identifying, assessing, and controlling the many biases that can influence primary studies or systematic reviews (Bae, 2014). Likewise, David Moher, the most prolific expert in the development of reporting guidelines, has illustrated how systematic reviews uncovered many of the issues that are addressed by reporting guidelines:

> *As noted by the International Committee of Medical Journal Editors, 'In return for the altruism and trust that make clinical research possible, the research enterprise has an obligation to conduct research ethically and to report it honestly'. In addition, research reports must be useful to readers – articles should include all the information about methods and results that is essential to judge the validity and relevance of a study and, if desired, use its findings. Journal articles that fail to provide a clear account of methods are not fit for their intended purpose. A vast literature over several decades has documented persistent failings of the health research literature to adhere to those principles. Systematic reviews are a prime source of evidence of these failings …. In*

> *addition, hundreds of reviews of published articles, especially those relating to randomized controlled trials (RCTs), have consistently shown that key information is missing from trial reports.*
>
> *(Moher et al., 2014, p. 4)*

But what can we learn from this relationship? First, we must admit that the issues and problematizations that drive the metascientific community are not novel in an intellectual sense but rather new or recent with respect to a specific context. This can be illustrated by the commonly made relationship between the metasciences and the replication crisis in the psychological sciences. This crisis became increasingly visible during the early 2000s, culminating in the exposure of Diederik Stapel's research misconduct around 2011 (Field & Derksen, 2021). Others have argued how different histories about the science reform movement can be written, especially when biomedicine is taken into perspective (e.g., Nelson, 2024).[63] My research has shown how the intellectual patterns as well as reform actions of metascientific practice can be dated back to the origins of research synthesis. One benefit of such comparative backtracking is the opportunity for cross-community learning. But other possibilities lie in the sociological viewpoints on the metascientific movement. I will address both points in the following.

Metascientific reformers might become able to inform their programs or projections and avoid errors that have already been made elsewhere. From the perspective of formal scholarly communication, there are two important reporting issues that were addressed by guideline developers in the 1990s and more recently by the metascientists. First, there is external reporting bias, which is commonly called “publication bias” and means that negative or mediocre research results are not published at all. This has been addressed by the implementation of registries for clinical trials in which the project plans are published in the form of study protocols, and which have gained substantial attention by the metascientific movement too (Field et al., 2020). Second, there is internal reporting bias, which means that crucial information is lacking from published

[63] Although the biomedical research community uses similarly alarming language, as shown in the introduction, they speak of a crisis less frequently. For example, the German Scientists Survey has revealed that there are disciplinary differences in how reliable researchers identify the knowledge in their field (Fabian et al., 2024).

studies (I. Chalmers, 1990; McGauran et al., 2010). This has been addressed by reporting guidelines such as PRISMA, which was extensively studied here.

Comparing both problems and their solutions, we observe a high rate of usage and the absence of any noteworthy critical debate in the case of reporting guidelines, while there is much more of a critical debate on the rationales and values of the preregistration of studies, especially when preregistration is not just optional or not applicable, as reporting guidelines often are, but becomes a mandatory requirement to publish, as it has been in clinical medicine (e.g., Huser & Cimino, 2013). My research has shown how reporting guidelines were co-developed by publishing professionals and domain experts so that acceptability and usability of the guideline became a major design choice. Hence, there was more of the practical reality of researchers inscribed in the reporting guideline, in contrast to preregistration. Therefore, we can compare both approaches based on how they are implemented. Reporting guidelines that were developed against the background of scientific practice and preregistration that was driven by the fulfillment of epistemic norms. Preregistration and reporting guidelines are fundamentally different things, yet abstractions of their social lives can help to contextualize metascientific perspectives and even support reform initiatives.

The analysis of how reporting guidelines reformed scientific writing can inform our interpretation of how the metasciences contribute to the intellectual progress of science. I will sketch out this perspective in brief as well.

Systematic reviews emerged to encounter the wealth of published biomedical information and to synthesize it into valuable pieces of knowledge. As such, systematic reviews are tools to handle the modern economics of information, which usually overburden individual consumption and comprehension. The most important tool of a systematic reviewer in controlling the amount of information that must be aggregated is the selection of primary studies by their content (see also M. Clarke, 2004). Modern systematic reviews and meta-analyses are expected to answer a specific research question based on a set of highly similar research. This means that reviewers must ensure the homogeneity or consistency of the studies so that they do not compare highly heterogeneous studies, or “mix apples and oranges" (Hunt, 1999, p. 61; see also S. Goodman, 1991; Higgins et al., 2003). This is done by controlling the inclusion criteria of the review, which determine the relevance of the potentially eligible primary research, as mentioned above. For example, reviewing studies about the effectiveness of a certain medical therapy can be narrowed down by focusing on a specific age group. By choosing a threshold, a systematic reviewer excludes all publications that did not

cover this age group in the study. As a result, the reviewer is confronted with a smaller set of eligible studies, which must be aggregated (see also Epstein, 2007).

The same logic can be applied to methodological criteria, because the epistemic power or risk of bias in primary studies can be likewise used as filters. For example, systematic reviewers can formulate minimum levels of evidence or exclude whole study designs and research genres. In that sense, Cochrane reviews are expected to only aggregate results from randomized-controlled trials (I. Chalmers, 1993; see also Bohlin, 2012). Limiting a review to include only results from randomized controlled trials reduces the amount of information that must be absorbed because there are usually many other study types and perspectives about a medical condition, for example, with regard to epidemiology, diagnostics, or clinical care. Systematic reviews that do not synthesize a single study illustrate the relationship between inclusion criteria and the amount of information that must be consumed (Lang et al., 2007; Yaffe et al., 2012).

But methods and standards for knowledge syntheses not only provide the tools to encounter the overwhelming economics of information. These methods can also reduce the effort required for knowledge consumption and lower the production costs associated with knowledge synthesis in a very practical sense. Already in the 1970s, the first meta-analysts and systematic reviewers criticized the state of primary research reporting. Back then, lacking information about published or unpublished studies meant that reviewers had to mail or phone the original authors. This meant making dozens of individual contacts, which may never respond, respond only after a substantial delay, or respond but do not provide more information on the studies for various reasons (I. Chalmers, 1990; Hunt, 1999). Thus, finding ways to uncover unpublished studies and ensure that published studies include all the information required for a review means making knowledge synthesis, such as systematic reviewing, more efficient and feasible. In this regard, the reliability and validity of meta-analyses was mentioned in early discussions about trial registries (e.g., Newcombe, 1987) and the first reporting guidelines (e.g., Begg et al., 1994). With regard to the body of scholarly literature, the standardization and homogenization of methodological criteria make the consumption of information more efficient and feasible. Or, in other words, standardizing and systematizing methodological criteria made the production of literature synthesis easier and cheaper.

What insights can we gain from this perspective to better understand the metascientific movement? Quality assessments and value judgments on research can be seen as filters to tackle the information overload in biomedical research. Metascientific interventions are then to be interpreted as economic interventions on knowledge markets (see also

Woelert, 2013). Irrespective of their intellectual justification and legitimization, metascientific interventions provide tools to handle the information overload. Subsequently, the idea that metascientific interventions reform or (re-)standardize scientific practices in a normative way can be reformulated. Rather, these interventions create epistemic frameworks or analytic schemes that can be used to classify, sort, and filter the available biomedical knowledge.

In fact, the reformers who developed the PRISMA guidelines were aware of the variance in the epistemic characteristics and methodological rigor. But while accusations of misconduct and proclamations of crisis were most vocal, the developers of methodological standards knew that globally distributed research will always bear a continuum in quality and methodological rigor. In other words, they were aware that researchers are not geniuses but that there is a normal distribution in research quality, as I described it earlier (see also Simmons et al., 2011; Wicherts et al., 2016). Therefore, the aim of understanding and improving research practices is not targeted for its own sake. Rather, it provides the tools and techniques to retrieve the information that is most relevant to a given context.

Concluded more provocatively, the way biomedical research is (re-)standardized suggests that the more important effect of metascientific interventions is not the increase in research quality but rather the decrease of the quantity of information that has to be consumed. Thus, from the perspective of the sociology of information, the recurring emergence of science reforms and attempts to narrow down the definition of what constitutes acceptable science become inevitable or even vital to the course of human knowledge.

8.3 Open Science, documentation practices and the future of research evaluation

In the concluding section, “8.1 The lone genius is dead, long live the lone genius!,” I argued how the standardization of research tasks results in a fractionalization of what we consider epistemic practice on the one hand, but also a professionalization and emergence of new roles on the other. While I developed this perspective against the background of the professionalization of systematic reviewers, I also indicated how metascience is likewise evolving into a new profession that defies the traditional disciplinary logic. In this section, I want to discuss fractionalizations of research in a more abstract manner.

Turning away from research practice, I propose that codified epistemic standards translate epistemology, or (ideals of) *the scientific method,* into a tangible and fractionized research item. Such an item recedes to be a characteristic of the study or epistemic standard that has been met but becomes an authoritative and somewhat independent actor in the actor-network of modern biomedical research projects. While the reification of previously abstract qualities allows for the standardization, communication, and negotiation of qualities on a global scale, it also provides a toehold for formal modes of evaluation and the construction of transparency metrics. To approach this proposal, I will start by reconsidering the role of codification and formal communication.

My research is based on the inevitability of scholarly communication, which was elaborated from three different angles. First, throughout the first chapter I laid out briefly how scientific documents represent epistemic endeavors to some extent and co-construct what we might consider the body of scientific knowledge. Further, documents and document types are equipped with social capacities and expectations that establish links between biomedical researchers, but also doctors, drug companies, and healthcare policymakers. Although being a realist perspective on science, social arrangements such as the Cochrane Collaboration, which only produces systematic reviews illustrate this idea.

Second, reporting guidelines such as PRISMA are culmination points for the scientific reform movement and follow a similar logic. They provide tangible objects in the discourse of the transparency crisis and its solution, as shown throughout this book. On the level of their content, guidelines construct and disseminate stories of reporting issues and their solutions, they provide definitions of scientific quality and epistemic value judgements, and they suggest concrete practices that manifest these definitions. But on the document level, reporting guidelines also co-construct the domain of metascience or science reform, as described above. Only when there are reporting guideline documents can research experts become guideline developers or transparency experts.

Third, individual researchers and biomedical research authors must incorporate the guidelines into their day-to-day practice and local contexts of research. This goes beyond an in-principle relationship and involves interaction with the document as a research item. When complying with PRISMA, a local version of the guideline document becomes manifest, which turns PRISMA into a part or resource of the local research project. Being a codified and independent resource, it resembles other research items or actors in an actor-network, such as specimens, technical apparatus, or previous literature. But in

contrast to other resources, which are often depicted as research resources or inputs (e.g., Rheinberger, 1997), codified versions of standard compliance, such as guideline checklists, communicate value judgements. Against the background of the many requirements and expectations towards formal scholarly communication, e.g., as expressed by funders, journals, or scientific peers, authors become able to commodify the epistemic qualities of their work. This form of codification or reification resembles the relationship between abstract and contingent research processes and their transformation in the form of codified journal publications, which has been described as the commodification of knowledge (Mirowski, 2018; Radder, 2010; see also Birch, 2017). In other words, filled-out PRISMA checklists are situated between a paper and an abstract notion of knowledge, similar to how journal papers sit between a research project and an abstract notion of knowledge.

A commodification of epistemic qualities into objects as part of the entity network of a study alienates epistemic questions from the study itself. While epistemic characteristics are traditionally coalesced with the study, they are now becoming part of the studies' outside worlds. As such, formerly integrated and intimate aspects of the study now become part of its public assets that are openly accessible and communicable. In this way, epistemic characteristics of a study become independent objects—currently mostly documents—which may be referenced, published, downloaded, and critically assessed. In this regard, I have discussed the *map of documents* in chapter 7, which transforms scientific reporting from information containers into representations of actor-networks.

Epistemic commodities or assets inhibit epistemic valuations that can be traded or exchanged in the knowledge market. Thinking of scholarly communication as a quasi-regulated sphere, epistemic assets represent credits that can provide entry into academic journals, funding schemes, or discourse communities in a more abstract sense.[64] In this regard, we have seen in chapters 3 and 4 how PRISMA guidelines transform transparency into an achievable epistemic quality that can be assessed by peer reviewers or readers in a likewise efficient manner, as shown in chapter 7.

But what are the consequences of the commodification of epistemic qualities? A more direct consequence is that novel forms of scientific outputs become assessable to formal evaluation. The commodification of epistemic qualities that comes with open science

[64] For example, by less formalized ways, when other scholars and academic peers perceive an author as a competent systematic reviewer who complies with the accepted standards.

offers new dimensions and possibilities for formal science evaluation. Consequently, we must lower our expectations of what open science reforms can do about that part of the scientific crisis that has been fueled by the pressures uttered by research evaluation and the publish-or-perish culture. Open science reforms are often seen as saviors against the unintended consequences stemming from that culture (Mirowski, 2018). As such, the publication of data, software, and checklists is thought to uncover and prevent sloppy research that was performed in a hurry to publish, rather than perish. Although these objects might provide some access to the research as it was performed, they may lull academia into more counting, metrification, and assessment. In that regard, more critical accounts have criticized the fetishization of documentation that comes with open science. While there is a new scientific ethos coming with open science, many of its practical aspects are reduced to an ever-growing bouquet of documentation practices and scientific products (Leonelli, 2023).

Open science is no effective counter-development against a metric-based research evaluation. On the one hand, we have investigated the positive aspects of it, e.g., how a bigger bouquet makes a broader range of practices and professions in science visible and can empower currently underrepresented groups in research, such as librarians, statisticians, or systematic reviewers.[65] But this also means that different dimensions of research become metrifiable. If one believes that it is not only the misuse of science metrics that has fueled the publish-or-perish situation in modern academia but the very introduction of indicators and metrifiable characteristics, then open science cannot be understood as a counter development against metrification.

Moreover, we must reassess the significance of evaluative bibliometrics as part of a cultural shift in modern science. I have shown in the introduction how bibliometrics suggests to make aspects of scientific work and success accessible to anyone, including disciplinary outsiders. By means of counting publications and citations, librarians and research evaluators make judgments about the scientific performance of countries, organizations, and even individuals. Bibliometricians have long since turned their attention to open science and developed new data sources and metrics that address dimensions beyond the number of papers and citations. Most prominently, the shift towards open access publishing has been addressed extensively in the bibliometric

[65] More common topics in the open science discourse are the documentation practices and valuations with regard to data publications (Li & Jiao, 2022) or research software engineering, in which formal publication is understood as an instrument for academic legitimization (Koeser, 2015).

community with the development of new indicators and monitoring systems (Barbers et al., 2018).

But others point at an even more profound shift. Benedikt Fecher and Sascha Friesike (2014) argue how the search for alternative metrics has become one of five key pillars of the open science movement. In that regard, bibliometricians turned their attention to production metrics such as patent metrics or metrics about novel types of outputs such as datasets and code (H. Park & Wolfram, 2019; Robinson-Garcia et al., 2017). Notably, metadata schema of novel PID providers such as DataCite include non-traditional resource types such as dataset, software, instrument, "ComputationalNotebook", service, and even "PhysicalObject" (DataCite Metadata Working Group, 2026), suggesting that almost any research object can become a digital entity that can be counted and cited. Relatedly, novel types of reception metrics focus on the number of downloads or citations in blog posts and newspaper items (Leckert, 2021), citations by policy papers, or medical treatment guidelines (Aman & Sorgatz, 2023). Thus, it seems that the trend towards commodification and metrification of various dimensions of science is independent of what the dimensions are.

The commodification of epistemic qualities must be considered as a project that is inherent to the idea of evaluative bibliometrics and the methodological craft of that discipline. Doing bibliometrics is then more an act of commodification and of constructing tangible qualities of research than of calculating and weighing numbers and sizes. Similarly, metascientists create commodification by operationalizing the dimensions of science. They create new types of document corpora, analyze them, assess the state of the corpora against quality judgments, and develop solutions. Crucial to such works is the definition, identification, and classification of epistemic objects, e.g., documents. As these classifications and definitions are often spontaneous and experimental in the metasciences, the quantitative outcomes and correlations are often seen as byproducts that do not take center stage before the classifications have become widely established. We have seen examples such as analyses of registered protocols, evaluations of PRISMA compliance, or analyses of journal submission guidelines throughout this research.

Therefore, the way how bibliometrics, scientometrics, and the metasciences fuel the reification and commodification of epistemic qualities must be considered a fundamental form of scientific change. Moreover, its impact on the digital transformation of science surpasses what we genuinely assign to the influence of the open science movement and its implications for the ethos of science. Put in other words, while new norms and principles help us understand in which direction the open science

transformation moves, the technicalities of the information market are what actually drive this transformation. Against this background, future analyses and interpretations benefit from taking digital infrastructures and the overall information market in science (and in general society!) into focus (see also Woelert, 2013). Based on this assumption, we can envision a digital transformation of science that is more far-reaching than commonly assumed, which leads to another, more indirect consequence of the commodification of epistemic qualities.

The commodification of epistemic qualities can go far beyond what we today observe as forms of documentation, formal scholarly communication, or valuable scientific contribution. So far, open science advocates have focused on the production, sharing, and evaluation of novel types of documents or forms of documentation. I have argued how this might go beyond traditional categories such as publications, datasets, or code. However, the general trend in standardization offers an even wider range of entities, characteristics, and qualities that might be commodified in the future. Contemporary accounts of standardization have shown how the idea of single, universal standards has been abandoned in favor of more inclusionary forms of standardization. For example, Steven Epstein (2007) showed how the standard human was replaced by a set of classifications that were more diverse in terms of race, gender, ethnicity, and other categories. [66] Likewise, we have seen how scholars and evaluators attempt to operationalize a multifaceted concept of research quality, for example with regards to philosophical notions of knowledge that employ a set of different yet interlinked epistemic values and norms. But speaking of commodification, we can see how this development already left the abstract spheres of philosophy of science.

The commodification of epistemic qualities has been driven forward by the ongoing digitization of research and research information. In the background of this research, I have discussed how the digital representation of scientific resources fueled the information overload but also provided tools to encounter it. Digital libraries and platforms have created curated metadata, cataloging standards, keyword sets, and classifications to support information retrieval. While much of this is the outcome of digital curation practices, recent developments turned their attention to a more

[66] Similarly, we have seen how extensions and forks of PRISMA make it more inclusionary for different review types, and similar trends have been observed elsewhere. For example, in the case of the FAIR principles for making research data **F**indable, **A**ccessible, **I**nteroperable, and **R**eusable. These have been extended to account for different topics and research domains, but also for other digital research assets such as software, tools, algorithms, and workflows (Huerta et al., 2023).

formalized and automatic processing of textual data. These developments suggest the deconstruction of scientific information of journal publications and a reassemblage into semantic networks and knowledge graphs (Auer, 2018). These networks consist of interrelated concepts such as organisms, compounds, illnesses, chemicals, or patient characteristics. Literature-based discovery then means drawing novel conclusions based on the recombination of previous findings, but without collecting new data or running additional analyses.[67] While many of these initiatives are based on the mining of textual contents, others have asked, "Why bury it first and then mine it again?" (Mons, 2005, p. 1) and made the case for an unprecedented level of concept standardization and commodification in the life sciences. But what has the standardization to do with epistemic qualities and their commodification?

Semantic publishing and knowledge graphs suggest a decomposition and recomposition of knowledge into its informational parts. Scientific quality and methodological rigor have been conceptualized analogously. For example, the Rigor & Transparency Index has been developed, which analyzes preclinical research papers based on their reporting of standardized concepts such as sex, blinding, randomization, and statistical power (Menke et al., 2020). In turn, an index can be assigned to each paper or aggregated over a whole journal. Other approaches turned their attention towards phrases and descriptions rather than concepts, for example, to check if a study reports its limitations (Kilicoglu et al., 2018). While such perspectives focus on concepts of relevance and quality stemming from open science, other approaches employ traditional metrics such as the citation impact of standardized concepts and entities (e.g., Piekniewska et al., 2025). While much more about knowledge graphs and semantic representations of epistemic qualities can be said, which would go beyond the scope of this argument, we can already witness how the commodification of epistemic qualities becomes a tangible and technically formalized phenomenon, rather than a more tacit and abstract one.

The commodification of epistemic qualities and their semantic representation in the information economy of science pose several risks. First, the commodification can fuel the bureaucratization of science as it has already been described with regards to documentation practices and the adherence of standards (Penders, 2022; Peterson & Panofsky, 2021). This means that any form of scientific change, either content-wise or epistemic reform, must involve technical and social restandardization and

[67] For example, A→C might be based on A→B and B→C. The same principle is the basis for the already established method of network meta-analysis. Notably, bioinformaticians have also called literature-based discovery "meta-analysis" (Mons, 2005, p. 1).

implementations. Consequently, scientific researchers will increasingly deal with standardizations and standardized concepts. Scientific change then requires starting with establishing new categories and standards. In this vein, we will find more standardized concepts, guidelines, and protocols that attempt to inform or reform scientific practice in accordance with particular norms and value judgments.

Creating new concepts and standards will become a valuable form of research work that substantially reshuffles the current logics of the scientific reward system. However, we can imagine how this could create false incentives, as impact-oriented researchers may focus more on developing and promoting new standards and concepts in order to attract scholarly attention and additional funding. Although such consequences remain speculations, we might legitimately ask how much real-world scientific progress depends on the digitization and infrastructurization of research assets and epistemic objects. In addition, my research has also shown how the standardization of reporting is only briefly informative about the often messy and contingent practices of documentation. Taken together with the fact that research documentation, in turn, represents only a limited portion of what researchers do and the knowledge they have acquired, we should avoid equating scientific practice and documentation.

I want to end this chapter with two outlooks or suggestions for future research on open science, nevertheless. First, much of what was said during this concluding chapter are interpretations based on the results of my research and under consideration of adjacent observations. To me, these thoughts are sufficient to justify further research in that direction and to do it in a different frame compared to current trends and major topics in the science studies. Instead of turning our attention to the minute details of the social, technical, and political dimensions of open access publishing, broader perspectives and interpretations are valuable. Thus, I suggest approaching open science and the digitization of research by explicitly excluding publishing and formal scholarly communication.

Second, the way I just framed the commodification of epistemic qualities suggests that actors engaged with standardization have a substantial interest in standardization. While scientific qualities and values are dynamic and will surely be renegotiated in the future, we might observe that only reified and standardizable forms of quality judgments get a ticket to such negotiations. More tacit and abstract conceptions of epistemic qualities become incommensurable, or non-exchangeable, in a zone that lies between research, technical infrastructures, and the interest of research evaluators. Against the background that the constituency promoting research metrics has reshaped what is considered valuable or relevant, we must remain attentive to how

standardization constituencies shape the future of epistemic objects and commodified research qualities.

8.4 Closing remarks

In the first part of this concluding section, I revisited many aspects of my original investigation and addressed the broader research questions. Since then, I have revealed and conceptualized many details of how biomedical research is standardized in practice. However, an equally large number of new phenomena and intriguing research questions have also emerged. In this regard, it will be valuable, if not necessary, to reconsider the topic in the future in order to understand the trajectories and pathways of standardization. One question to be addressed is whether PRISMA standards and compliance with them will become invisible parts of, or infrastructures within, formal scholarly communication as suggested in the traditional literature. In contrast, we might witness an increasing reification and objectification of formerly abstract and intangible information and a growing bouquet of standards. In addition, I expect a significant increase in practice guidelines or behavioral guidelines, such as reporting guidelines. An impressive example are the FAIR guidelines, which already have built an impressive social and political capital and constantly grow in their adaptations and extensions. Its developer community goes even further. It envisions an "Internet of FAIR Data & Services (IFDS)" that provides an underlying network infrastructure for semantic objects and processes (GO FAIR, 2024). This implies an internet of scientific information that connects researchers, apparatus, databases, computing centers, and digital objects around the globe. While this sounds impressive with regards to the evolution of digital infrastructure, it also gives rise to fears of partitioning and isolating digital worlds, i.e., science from society, or the scientific community from the rest of the galaxy, as done in Isaac Asimov's Foundation trilogy.

The ongoing digitalization and infrastructurization of research suggest that a substantial fraction of future scientific work and scientific reasoning will happen in digital spaces. Hence, we should draw more attention to digital ethnography and digital sociology. Moreover, we must consider that these trends do not just imply learning programming or analyzing social media data. Rather, we must be attentive to the possibility of completely novel social and epistemic qualities that emerge from the digital world but lack any analogous antecedent.

With this, I have completed my journey through the PRISMA-based standardization of biomedical writing. I approached this not only in a historical or teleological manner—from the creation to the application of the guidelines—but also in a holistic manner,

taking theoretical, empirical, conceptual, and discursive perspectives. I hope that, in doing so, I have shed light on some contemporary developments in biomedical research and hinted at some of its future trajectories and important trends.