frontiers | Research Topics

Public research and private knowledge – science in times of diverse research funding

Edited by
Jon Leefmann, Julia Böttcher, Michael Jungert, Christoph J. Merdes and Sebastian Schuol

Published in
Frontiers in Research Metrics and Analytics

ISSN 1664-8714
ISBN 978-2-83251-564-8
DOI 10.3389/978-2-83251-564-8

About Frontiers

Frontiers is more than just an open access publisher of scholarly articles: it is a pioneering approach to the world of academia, radically improving the way scholarly research is managed. The grand vision of Frontiers is a world where all people have an equal opportunity to seek, share and generate knowledge. Frontiers provides immediate and permanent online open access to all its publications, but this alone is not enough to realize our grand goals.

Frontiers journal series

The Frontiers journal series is a multi-tier and interdisciplinary set of open-access, online journals, promising a paradigm shift from the current review, selection and dissemination processes in academic publishing. All Frontiers journals are driven by researchers for researchers; therefore, they constitute a service to the scholarly community. At the same time, the *Frontiers journal series* operates on a revolutionary invention, the tiered publishing system, initially addressing specific communities of scholars, and gradually climbing up to broader public understanding, thus serving the interests of the lay society, too.

Dedication to quality

Each Frontiers article is a landmark of the highest quality, thanks to genuinely collaborative interactions between authors and review editors, who include some of the world's best academicians. Research must be certified by peers before entering a stream of knowledge that may eventually reach the public - and shape society; therefore, Frontiers only applies the most rigorous and unbiased reviews. Frontiers revolutionizes research publishing by freely delivering the most outstanding research, evaluated with no bias from both the academic and social point of view. By applying the most advanced information technologies, Frontiers is catapulting scholarly publishing into a new generation.

What are Frontiers Research Topics?

Frontiers Research Topics are very popular trademarks of the *Frontiers journals series*: they are collections of at least ten articles, all centered on a particular subject. With their unique mix of varied contributions from Original Research to Review Articles, Frontiers Research Topics unify the most influential researchers, the latest key findings and historical advances in a hot research area.

Find out more on how to host your own Frontiers Research Topic or contribute to one as an author by contacting the Frontiers editorial office: frontiersin.org/about/contact

Public research and private knowledge – science in times of diverse research funding

Topic editors

Jon Leefmann — University of Erlangen Nuremberg, Germany
Julia Böttcher — University of Erlangen Nuremberg, Germany
Michael Jungert — University of Erlangen Nuremberg, Germany
Christoph J. Merdes — University of Erlangen Nuremberg, Germany
Sebastian Schuol — University of Erlangen Nuremberg, Germany

Citation

Leefmann, J., Böttcher, J., Jungert, M., Merdes, C. J., Schuol, S., eds. (2023). *Public research and private knowledge – science in times of diverse research funding*. Lausanne: Frontiers Media SA. doi: 10.3389/978-2-83251-564-8

Table of contents

frontiers | Frontiers in Research Metrics and Analytics

TYPE Editorial
PUBLISHED 15 December 2022
DOI 10.3389/frma.2022.1106343

OPEN ACCESS

EDITED AND REVIEWED BY
Caroline S. Wagner,
The Ohio State University,
United States

*CORRESPONDENCE
Jon Leefmann
jon.leefmann@fau.de

SPECIALTY SECTION
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

RECEIVED 23 November 2022
ACCEPTED 05 December 2022
PUBLISHED 15 December 2022

CITATION
Leefmann J, Böttcher J, Jungert M, Merdes C and Schuol S (2022) Editorial: Public research and private knowledge—Science in times of diverse research funding. *Front. Res. Metr. Anal.* 7:1106343. doi: 10.3389/frma.2022.1106343

Editorial: Public research and private knowledge—Science in times of diverse research funding

Jon Leefmann*, Julia Böttcher, Michael Jungert, Christoph Merdes and Sebastian Schuol

Center for Applied Philosophy of Science and Key Qualifications (ZiWiS), Friedrich-Alexander-Universität Erlangen-Nürnberg, Erlangen, Germany

KEYWORDS

research funding, scientific integrity, values in science, social epistemology of science, research ethics, biased research

Editorial on the Research Topic
Public research and private knowledge—Science in times of diverse research funding

The production and distribution of knowledge is a key process in scientific and scholarly inquiry. However, this process is not and has never been limited to universities and public research institutes alone, but extends to agents as diverse as the Research & Development departments of companies, citizen scientists, and private non-profit research institutes. In recent years, these agents have shown an increased interest in basic science, in particular in fields of rising social significance such as AI or biomedical technologies. These interests in turn direct attention to the sources of funding and the interactions and collaborations between academic systems and the private sector. But, what difference does it make who funds research? Who are the relevant providers of funding? Does the influence of private funding change the selection of research topics in epistemically and ethically (un-)desirable ways? Does it lead to a privatization of knowledge, and if so, what are the consequences?

These questions unite the eight multidisciplinary contributions to this Research Topic. Comprised of six research papers and two critical perspectives, the issue offers a complex and multifaceted picture of the current debate on research funding at the intersection of research policy, philosophy, sociology, and science and literature studies. It also serves as a showcase for contributions that were presented and discussed at the international conference "*Public research and private knowledge—Science in times of diverse research funding*" organized by the Center for Applied Philosophy of Science (ZiWiS) at FAU Erlangen-Nürnberg from July 21st to July 23rd, 2021.

The papers in this Research Topic approach the subject from various theoretical backgrounds and by using examples from research areas as diverse as pharmacology,

genetics, or literature—to name just a few—in order to reflect on the influence of funding sources on scientific practices. They can be broadly divided into three categories:

Research funding and the integrity of scientific research

A first group of papers discusses the influence of diverse funding on the integrity of science. These empirically informed analyses of specific research practices argue that funding is often distributed in ways that are epistemically detrimental and ethically problematic. In one or another way they seek to reveal social mechanisms that explain and justify their claims. To this end they offer analyses of the Open Science movement (Fernández Pinto), the debate on policy regulations for genome editing (Christian), and the pharmaceutical industry's strategy of managing the processes of research and publication (Sismondo). All of them find that funding from the private sector plays a critical role in the establishment and maintenance of epistemically problematic practices.

Biased assessment of private research funding

A second group of papers takes a critical stance toward these claims. Reviewing the literatures on private research funding in the philosophy of science and in research policy, one paper (Holman) finds that studies in research policy tend to evaluate relations between industry and academia primarily as beneficial, whereas the philosophy of science literature depicts such relations as generally corrosive for scientific inquiry. For a better assessment of the overall effect of industry funding on various research fields, it points to the origins of these contradicting perspectives and suggests venues for reaching a fruitful interaction of these distinct literatures. Similarly, another contribution (Sikimić) calls for a more nuanced take on the prospects and perils of industry-academic relations. Taking the example of different strategies to develop vaccines against COVID-19, it argues that funding schemes often do not fit the neat distinction between publicly and privately funded research. Publicly funded research can pose similar threats to the epistemic integrity of science as privately funded research and it may tend to promote elitism in science and the exclusion of research from institutions outside of Europe, Japan and North America. As the example of COVID-19 vaccine research shows, a perspective that is critical only about privately funded research is unduly simplistic.

Bias and values in science

The third group of papers deals with the theoretical framework of debating the epistemic and ethical effects of research funding. Since the beginning of the 20th century, scientific enquiry has been described as a process of empirically testing hypotheses that is free from non-epistemic values, i.e., prudential, moral or political judgements. This view, however, has been under constant attack at least since Thomas Kuhn's seminal work (Kuhn, 1977/2000). More recently, drawing on debates in philosophy of science from the early 1950s, Heather Douglas and others have made the case that various stages of the research process require determining the distribution of inductive risks (Douglas, 2000). Deciding which risks are worth taking, however, requires evaluating the consequences of one's decision. If this is correct, the distinction between biased and unbiased research cannot simply be grounded in the distinction between value-laden and value-free scientific practices. Two papers in this section address this issue through discussing Torsten Wilholt's methodological conventionalism as a possible solution to this problem (Wilholt, 2009, 2013). While the first (Ohnesorge) launches a critique of this view based on theoretical and practical considerations, the second (Leefmann) defends it against a competing version of empiricism arguing for its superior capacity to explain how financial power can create biases by distorting otherwise helpful social practices.

Contrasting this debate, a further contribution (Hempel) suggests a different methodological approach. This paper combines literary studies (science in fiction) with a sociological perspective and discusses two contemporary science novels. By analyzing how the concepts of autonomy and responsibility of science become manifest in two novels which both deal with research misconduct in biomedical research, it explores cultural understandings of these concepts and studies how the depiction of science in popular culture can offer conceptual insights into social actors, actor constellations, and interactions within and beyond the institution of science. Thus, it addresses the issue of research funding by a new approach that provides a valuable resource for further sociological and philosophical analysis.

As topic editors we believe that this Research Topic will provide the reader not only with exemplary analyses of the epistemic and ethical dimensions of research funding but will also highlight new directions for promising research and encourage interaction between different methodological and disciplinary approaches to address the topic.

Finally, we would like to sincerely thank all the authors, reviewers and external editors who contributed to the creation and compilation of these research papers. We also thank Frontiers in Research Metrics and Analytics for technical support and for publishing this collection as part of their Research Topic series.

Author contributions

All authors listed have made an intellectual contribution to the work and approved it for publication. The corresponding author's (JL) leadership in writing the editorial is acknowledged.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

References

Douglas, H. E. (2000). Inductive risk and values in science. *Philos. Sci.* 67, 559–579. doi: 10.1086/392855

Kuhn, T. S. (1977/2000). "Objectivity, value judgement, and theory choice," in *The Essential Tension: Selected Studies in Scientific Tradition and Change, 11th Edn.* (Chicago, IL, University of Chicago Press), 320–339.

Wilholt, T. (2009). Bias and values in scientific research. *Stud. Hist. Philos. Sci. Part A* 40, 92–101. doi: 10.1016/j.shpsa.2008.12.005

Wilholt, T. (2013). Epistemic trust in science. *Br. J. Philos. Sci.* 64, 233–253. doi: 10.1093/bjps/axs007

frontiers in Research Metrics and Analytics

ORIGINAL RESEARCH
published: 10 November 2020
doi: 10.3389/frma.2020.588331

Open Science for private Interests? How the Logic of Open Science Contributes to the Commercialization of Research

Manuela Fernández Pinto *

Department of Philosophy, Center of Applied Ethics, Universidad de los Andes, Bogotá, Colombia

Financial conflicts of interest, several cases of scientific fraud, and research limitations from strong intellectual property laws have all led to questioning the epistemic and social justice appropriateness of industry-funded research. At first sight, the ideal of Open Science, which promotes transparency, sharing, collaboration, and accountability, seems to target precisely the type of limitations uncovered in commercially-driven research. The Open Science movement, however, has primarily focused on publicly funded research, has actively encouraged liaisons with the private sector, and has also created new strategies for commercializing science. As a consequence, I argue that Open Science ends up contributing to the commercialization of science, instead of overcoming its limitations. I use the examples of research publications and citizen science to illustrate this point. Accordingly, the asymmetry between private and public science, present in the current plea to open science, ends up compromising the values of transparency, democracy, and accountability.

Keywords: commercialization of science, open science, open access, industry-funded research, democratization of science

OPEN ACCESS

Edited by:
Jon Leefmann,
University of Erlangen Nuremberg, Germany

Reviewed by:
Hans Radder,
Vrije Universiteit Amsterdam, Netherlands
James Britt Holbrook,
New Jersey Institute of Technology, United States

***Correspondence:**
Manuela Fernández Pinto
m.fernandezp@uniandes.edu.co

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *28 July 2020*
Accepted: *16 October 2020*
Published: *10 November 2020*

Citation:
Fernández Pinto M (2020) Open Science for private Interests? How the Logic of Open Science Contributes to the Commercialization of Research. Front. Res. Metr. Anal. 5:588331. doi: 10.3389/frma.2020.588331

INTRODUCTION

Science studies scholars, including a number of historians and philosophers of science, have raised important concerns regarding the current trend toward the privatization and commercialization of scientific research. Financial conflicts of interest, several cases of scientific fraud, and research limitations from strong intellectual property (IP) laws have all led to questioning the epistemic and social justice appropriateness of industry-funded research. At first sight, the ideal of Open Science, which promotes transparency, sharing, collaboration, and accountability, seems to target precisely the type of limitations uncovered in commercially-driven research.

Despite these laudable goals, the plea to open science has primarily focused on publicly funded research. In this paper, I argue that this particular focus challenges the appropriateness of the Open Science movement. As the philosophical analysis of this paper shows, if advocates of Open Science promote the openness of publicly funded research to foster, at least in part, new business opportunities and joint private-public ventures, as well as new markets for the development of online information and communication technologies (ICTs), then Open Science ends up contributing to furthering the commercialization of science, without addressing any of the epistemological and social justice concerns that have been identified. Accordingly, the asymmetry between private and public science, present in the current plea to open science, ends up compromising, not promoting, the values of transparency, democracy, and accountability. In

other words, insofar as Open Science fails to acknowledge, analyze and evaluate the structural connections between public science and private interests, it also fails to fulfill its goal of making scientific practices more transparent, democratic and accountable.

In order to show this, the paper is divided into four sections. The first section explains the epistemological and social justice limitations of industry-funded research. The second section introduces Open Science as an ideal that, at first glance, has the potential of overcoming some of the limitations highlighted in the first section. The third section problematize this claim, showing that Open Science, at least in the way it has been implemented, contributes at least in three different ways to the goals of industry-funded science. Finally, the fourth section illustrates how this has happened in two aspects of the scientific process: research publications and citizen science projects.

CONCERNS REGARDING PRIVATELY FUNDED SCIENCE

Over the past decades, scientific research has undeniably moved into the private sector to the extent that nowadays the majority of scientific research is both conducted and funded by the private industry (Eurostat, 2018; National Science Board 2018). As a reaction, a number of scholars have expressed concern about this current trend (Greenberg, 2001; Bok, 2003; Slaughter and Rhoades, 2004; Wise, 2006; Greenberg, 2007; Resnik, 2007; Radder, 2010; Holman and Elliott, 2018). Their main fear is that industry-funded research might have a negative influence on scientists, who, attracted by generous funding schemes, might compromise, perhaps irreparably, the quality of scientific research (Wise, 2006: 1266). The commercialization of science is thus taken as a major threat to scientific rationality, as it possibly puts in jeopardy the normative standards of the scientific enterprise.

Following a number of red flags, industry-funded science has come under increasing scrutiny. The first and most salient warning sign is perhaps the number of scandals in scientific research tied to corporate sponsorship, including the tobacco industry's cover-up of the health hazards of smoking, the petro-chemical industry's support of climate skeptics, and the pharmaceutical industry's manipulation of data in clinical trials, such as in the Vioxx case, among many others (Markowitz and Rosner, 2002; Sismondo, 2007; McGarity and Wagner, 2008; Michaels, 2008; Oreskes and Conway, 2010; Proctor, 2011).

A second red flag, and even more significant from a scientific point of view, is related to results from randomized controlled trials and systematic reviews showing that financial conflicts of interest have a statistically significant effect on research results (Bekelman et al., 2003; Lexchin et al., 2003; Sismondo, 2008; Lundh et al., 2017), in which design bias and publication bias seem to play an important role (Smith, 2005; Doucet and Sismondo, 2008). This empirical evidence suggests that the private funding of scientific research can have an almost imperceptible effect on research results in favor of the commercial interests at stake. Moreover, the most recent meta-analyses show that industry bias can impact research results, even when the quality of the study, measured by standardized statistical tools, is not compromised (Lundh et al., 2017). While this evidence focuses on medical research, conflicts of interest have also been identified as deeply problematic for scientific research more generally (Resnick, 2007; Elliott, 2008).

A third red flag is related to intellectual property rights. As some have suggested, and contrary to what's expected, strong IP rights, including patent law, can actually inhibit scientific research (Biddle, 2014). IP rights give private companies control over industry-funded research, which has led in turn to coverups of research results that would have been both epistemically and socially relevant (see, e.g., Biddle, 2007; Turner et al., 2008), as well as to impeding or restricting research initiatives (see, e.g., Waltz, 2009; Sappington et al., 2010).

As a consequence, industry-funded research faces important epistemic challenges, insofar as we have good reasons (or at least a number of red flags) to question the influence it has on research outcomes. In addition, many of these epistemological shortcomings have a clear societal impact. For instance, scientific fraud, as well as the subtler mechanisms that have been used by the private industry to obstruct the production of knowledge, in cases such as tobacco smoking, drug development, and climate change (Fernández Pinto, 2017), do not only impact the quality of the knowledge produced, but also the people who depend on that knowledge. As a result, the lack of proper knowledge has led to major human and environmental harms. Accordingly, industry-funded research also faces an important social justice challenge to prove that it can be both commercially-driven and socially responsible (Fernández Pinto, 2018).

These concerns become even more salient, given that the vast majority of scientific research today is both funded and performed in the private/business sector, as has been previously acknowledged. According to the most recent National Science Board indicators of 2018, 72% of scientific research and development (R&D) in the US is performed in the business sector, and 67% is funded by the business sector. One can find a very similar trend in the European Union (Eurostat, 2018) and also worldwide (UNESCO, 2015). In the US, 83% of business R&D performance can be accounted for by five sectors, which include chemical manufacturing (pharmaceuticals), and information (including software publishing industry). If the majority of scientific research is both funded and performed in the private sector, and if commercially-driven science faces important challenges, then we should have some concerns about the current organization of scientific research.

OPEN SCIENCE TO THE RESCUE?

Before examining the potential of Open Science for countering the problems found in commercially driven research, it is important to notice that the concept of open science is not

used unequivocally.[1] Sometimes the concept is used as an ideal to be obtained, a "scientific culture characterized by its openness" (Bartling and Friesike, 2014). In this sense, Open Science is a goal for research, which promotes transparency, accessibility, and collaboration, among other values.[2] Other times the concept is used to describe a movement within the scientific community, which also promotes certain values, but is mainly in charge of building the structures and designing the policies that would eventually lead to openness, as well as of advocating for the ideal and convincing others of its importance, e.g., "the immediate challenge for the Open Science movement is its ability to change the culture of science itself, which continues to operate within a print-based, proprietary, and closed framework for scientific discovery and communication" (Sidler, 2014: 82). Yet other times the concept is used to describe the set of policies that should be implemented to promote the core value of openness (e.g., Levin et al., 2016). Additional uses might also be at play, such as the idea of open science as a project or as a research strategy, among others. Part of the issue at hand here is related to the fact that there is a disconnect between the ideal of Open Science, and how this ideal is implemented through Open Science policies and promoted by the Open Science movement. As it will become clear in what follows, the argument of this paper does not question the ideal per se, but instead it questions the particular way the ideal has been conceived and implemented by the Open Science movement, as well as the way it has been brought about through Open Science policies. In this sense, the faulty logic of open science that I aim to highlight in the paper refers precisely to the inconsistency between the ideal and its current implementation.

At first sight, the ideal of Open Science, promoting what one might consider traditional democratic values, such as transparency, accessibility, collaboration and accountability, and arguing for a more inclusive, diverse, and pluralistic science, seems to target precisely the type of limitations uncovered in commercially-driven research. After all, these are the values that scientific research done in the private sphere sorely lacks, and that presumably have led to some of the main methodological and social justice issues that have been uncovered. More transparent and open venues for data collection and storage, peer review of methodological decisions and experiment designs, opening the peer-review process for paper publication, and open venues for publications themselves, all seem to point in the right direction for counteracting the state of secrecy and protection that characterizes commercially-driven research today.

In principle, the three main problems highlighted in the first section could benefit from more openness. First, scandals in industry-funded science, such as the tobacco industry's denial of the health hazards of smoking or the many episodes of data manipulation and undisclosed results from the pharmaceutical industry (e.g., in the cases of Bextra, Celebrex, Fen-Phen, Redux, and Vioxx), would be less encouraged in an environment where the research process and results are submitted to open peer evaluation and accountability (Maurer 2007: 426; Royal Society, 2012: 8). In this sense, initiatives to open up the research process from the early stages, e.g., through peer evaluation of experimental design, as well as strategies to evaluate published research through post-publication open venues, would keep scientific results under surveillance, buttressing the self-regulatory aims of science (Meskus et al., 2018).

Second, the pervasive financial conflicts of interest and its influence on research results, for which we now have strong evidence (Lundh et al., 2017), would also benefit from more transparency. If nothing else, more transparency means at least disclosure of conflicts, the first step toward plausible management strategies (Elliott, 2008). Moreover, the biasing mechanisms that might be in place in cases where industry-funding influences research results without compromising the quality of the studies, which are mostly imperceptible through standard quality assessment tools, might be easier to identify or even completely avoided through Open Science policies. For instance, problems arising from cherry-picking significant outcome measures post hoc (Andrade, 2015), could be countered by open registration of study design and protocols (such as, ClinicalTrials.gov), which includes determining primary outcome measures before the research starts.

Finally, strategies to open research in the strong IP regime we currently live in would encourage more scientific research on patent protected materials, as well as more non-protected data and publications, which in the long run is expected to achieve better and more reliable knowledge (Royal Society, 2012). Additionally, in all three aspects, citizen participation in the form of real interaction between industry and stakeholders regarding the social relevance and benefits of the research pursued would importantly contribute to making commercially-driven research more socially responsible.

Undeniably, the values that inspire the ideal of Open Science are promising guidelines to face the epistemic and social justice challenges of research done in the private sphere. In fact, much of the rhetoric use to promote and encourage Open Science explicitly targets some of the epistemic and social justice problems mentioned before. For instance, according to the OECD (2015) report "Making Open Science a Reality":

> Open search tools increase the efficiency of research as well as of its diffusion. Greater access to scientific inputs and outputs can improve the effectiveness and productivity of the scientific and research system, by: reducing duplication costs in collecting, creating, transferring and reusing data and scientific material; allowing more research from the same data; and multiplying opportunities for domestic and global participation in the research process. Scientific advice can also benefit from the greater scrutiny offered by

[1] I would like to thank one of the anonymous reviewers for pointing out this equivocal use of the concept of open science, as well as the importance of this discussion.

[2] The ideal might also emphasize different aspects of the research process depending on the school of thought that promotes it (Fecher and Friesike, 2014).

> open science, as it allows a more accurate verification of research results (. . .) Open science also allows the closer involvement and participation of citizens (OECD, 2015: 10).

In this way, Open Science is presented as a policy strategy to achieve better and more efficient scientific knowledge, as well as closer citizen participation in the scientific process.

Openness is also frequently portrayed as a core value of modern science, with an almost mandatory appeal to Robert Merton's ethos of science (Schroeder, 2007). According to Merton's rule of communism or communalism, "The substantive findings of science are a product of social collaboration and are assigned to the community," making secrecy "the antithesis of this norm" (Merton, 1974: 271). The ideal of Open Science is thus aligned with the traditional scientific ethos, only to be further supported by ICT revolution. In other words, Open Science policies in the 21st century would instantiate the scientific value of communalism granting access through different types of ICTs, such as open access journals (e.g., PLOS), open electronic archives (e.g., arXiv), collective intelligence projects (e.g., Polymath), public computing projects (e.g., Rosetta@home), citizen science projects (e.g., the Galaxy Zoo Project), collaborative research environments (e.g., Open Science Grid), academic social networks (e.g., ResearchGate and academica.edu), and social reference managers (e.g., Mendeley and Zotero), among others.

OPEN SCIENCE AS A BUSINESS STRATEGY

Despite the endorsement of the ethos of science and the promising strategies to open science through ICTs, I argue that Open Science, at least in the way it has been implemented, contributes at least in three different ways to the goals of industry-funded science. First, Open Science has been initially conceived only for publicly funded science, leaving it open for the private industry to join or not at its convenience. Second, Open Science has also been conceived to respond to the demands of the private sector. And third, Open Science also seems to foster a new way of commercializing science through development of new ICTs. Let me explain these in turn.

Opening Publicly Funded Science

The Open Science movement and its policies target primarily publicly funded research, while remaining silent about the problems already uncovered in commercially-driven science. Evidence for this claim can be found in multiple venues. Michael Nielsen, the author of *Reinventing Discovery: The New Era of Networked Science* (Nielsen, 2011a) and a strong advocate of Open Science, has stated a number of times, including during his TED talk "Open Science now!," that "any publicly funded science should be open science" (Nielsen, 2011b, 13:14–13:20). In a similar vein, the OECD defines Open Science as "the efforts by researchers, governments, research funding agencies or the scientific community itself to make the primary outputs of *publicly funded research* results—publications and the research data—publicly accessible in digital format with no or minimal restriction as a means for accelerating research" (OECD, 2015: 7, emphasis mine). Perhaps even more explicitly, in 2013 the G8 Science Ministers agreed on the following statement: "To the greatest extent and with the fewest constraints possible *publicly funded scientific research data should be open*, while at the same time respecting concerns in relation to privacy, safety, security and commercial interests, while acknowledging the legitimate concerns of private partners" (G8 Summit, 2013, emphasis mine), making clear that their support of open science was limited to publicly funded research and respectful of agreements with the private sector.

A comparable view is commonly turned into justification for granting open access to scientific publications, arguing that "research funded by tax-payers should be made available to the public free of charge so that the tax-payer does not in effect pay twice for the research—first for the research to be done and then to read the results" (Phelps et al., 2012: 1). Given that tax-payers pay for publicly-funded research, they have a right to access the results of such research. However, as the reader can see, the argument only applies to publicly-funded research, leaving all forms of privately-funded and privately-performed research outside the scope of Open Access. The recently approved Guidelines on Open Access to Scientific Publications and Research Data in Horizon, 2020, according to which the EU endorses the open access of scientific publications and research data, states that "under Horizon, 2020, each beneficiary must ensure open access to all peer-reviewed scientific publications relating to its results" (European Commission, 2016: 5). The policy is however mandatory only for beneficiaries of H2020 grants, which again restricts open access to publicly funded research.

In the meantime, private companies remain in the privileged position of adopting openness as they see fit. Pharmaceutical companies, for example, might benefit from Open Data strategies, e.g., developing public data bases, and strengthening international collaborations and networks. Given the amount of time and resources that is required to obtain marketable treatments from raw data, pharmaceutical companies might benefit more from Open Data than from maintaining data confidential. As Leonelli claims:

> . . .many rich laboratories have found that data donation offers the opportunity to participate in international networks and receive help with data analysis, thus accruing their own prestige, visibility, and productivity. Even major pharmaceutical companies like GlaxoSmithKline and Syngenta are contributing to the development of public databases, in the hope of outsourcing their R&D efforts, improving their public image, and gaining from the availability of data produced through public funding (Leonelli, 2013: 9).

Presumably, private companies might be less likely to share data analyses that are unfavorable to the industry, as has

happened a number of times with pharmaceutical companies covering up research results that can impact their market sales. A clear example of this is the infamous case of research on antidepressants. According to a study conducted by Turner and colleagues, where they compared the published literature against the studies reported by the US Food and Drug Administration, selective publication of clinical trial on 12 antidepressants resulted in a 32% overestimation of effect size in the published literature (Turner et al., 2008: 255–56). In other words, due to the fact that the industry decided to publish only favorable results, the efficacy of antidepressants was importantly overestimated. As this case shows, the industry has huge market incentives to maintain unfavorable research results in the dark.

In sum, while the ideal of Open Science endorses key values to counteract the epistemic and social shortcomings uncovered in commercially-driven research, the Open Science movement and its policies have primarily focused its efforts in opening publicly funded research, leaving the private industry free to decide about openness as it finds convenient.

Responding to Business Demands

Limiting Open Science to publicly-funded research should not come as a surprise. After all, it is easier to argue, legally at least, that tax payers have the right to access scientific results obtained through government funding than to make the case for opening research done in the private sector. Accordingly, one might claim that opening publicly funded science is a first and firm step toward a more transparent and accountable scientific process in general. Efforts against Open Science on the contrary would not lead science in the right general direction.

However, one could also argue that Open Science has also been conceived to respond to business demands. In fact, the plea for opening publicly funded science is commonly supported by the possible commercial ventures that opening science might encourage. The OECD and UNESCO both use this argument in favor of Open Access policies: "Scientists and academics are not the only groups that can benefit from greater open science efforts. The demand from the *business sector* and individual citizens to access research results is significant" (OECD, 2015: 11, emphasis mine; see also UNESCO, 2012: 29). In fact, Open Science policies are commonly encouraged as a way to grant access to scientific knowledge not only to other researchers and the public, but also to private industry. The H2020 goals of the EU make exactly this point: "This means making publicly-funded scientific information available online, at no extra cost, to European researchers, *innovative industries* and the public, while ensuring that it is preserved in the long term" (European Commission, 2016: 5, emphasis mine).

The resulting document from the *Open Science—From Vision to Action* EU presidency conference hosted by the Netherlands in April 2016 explicitly states at the outset that Open Science is good for business:

> Open science also increases business opportunities. The speed at which innovative products and services are being developed is steadily increasing. Only companies (. . .), entrepreneurs and innovative young people that have access to the latest scientific knowledge are able to apply this knowledge and to develop new market possibilities (EU Presidency, 2016: 4).

In addition, many advocates also see potential commercial uses as a reason to favor Open Science initiatives. For instance, Open Knowledge International, an international network advocating Open Science, states as one of its four core principles that:

> The use of licenses which limit commercial re-use or limit the production of derivative works by excluding use for particular purposes or by specific persons or organizations is STRONGLY discouraged. These licenses make it impossible to effectively integrate and re-purpose datasets and prevent commercial activities that could be used to support data preservation.
>
> *If you want your data to be effectively used and added to by others it should be open as defined by the Open Knowledge/Data Definition—in particular non-commercial and other restrictive clauses should not be used.* (Murray-Rust et al., 2010.)

As the principle suggests, restricting commercial uses of data would be counteractive for open data. In sum, major efforts to implement Open Science have focused on publicly funded research, making results from this research widely available, while leaving aside or simply ignoring the lack of openness in the private sector. At the same time, many of these efforts identify potential commercial ventures as a desirable outcome of Open Science. Or in other words, if opening publicly funded research helps support private, commercial, or industrial endeavors, then the more reasons we have to favor Open Science policies.

Creating New Ways of Commercializing Science

Furthermore, Open Science also seems to foster a new way of commercializing science, for the opening of science in the 21st century does not come in the form of public forums in the agora or through open access to public libraries worldwide. The Open Science movement is very clear in this respect: we ought to take advantage of the unrestricted possibilities that ICTs give us, especially through online platforms, to take open science at a new level—what is also known as Science 2.0. But this means that opening science today comes together with an increasing number of online open access platforms, an "e-infrastructure" as Schroeder (2007) has called it, mostly developed through a Silicon Valley startup model, aiming at the likes of Facebook and Google; i.e., another form of venture capitalism under the rhetoric of democracy and citizen participation.

A new "knowledge industry," as Fecher and Friesike (2014) have called it, is slowly but surely emerging from implementing open science. One only needs to look at the number of different types of ICTs developing new business models for open science. As Mirowski (2018) has documented, Open Science seems to

operate through the new business model of platform capitalism, in which all the contents of the research process, from study design, to data collection, to peer-review, and publication, are expected to be available on online platforms. We are already witnessing how a number of platforms compete to be the go-to online repository of a specific aspect of the research process. Take, for example, academia.edu and ResearchGate competing to be the mandatory Facebook of science.

In sum, Open Science, with its focus on publicly funded science and its encouragement of new ICTs and new commercial ventures, is not merely overlooking commercial research, but actually contributing to strengthen it. While some might consider this an asset of Open Science, given that it seems to promote both transparency and business opportunities, I prefer to be cautious about this win-win reading of the situation. As I mentioned in "*Concerns Regarding Privately Funded Science*" section, commercial interests have had a worrisome influence on the scientific process, moving science away, not toward the ideals of transparency, democracy, and accountability, promoted by open science. In the next section, I examine two further examples to illustrate this point, i.e., the cases of publication planning and of citizen science projects.

THE PROBLEM ILLUSTRATED BY TWO CASES

If the previous analysis is correct, the Open Science movement has an asymmetric view of private and public research, according to which openness has only been applied to publicly funded science; and this asymmetry sets up publicly funded science for further commercial gain. If this is so, Open Science has not really contributed to ameliorating the epistemic and social justice problems in commercially driven research, but instead seems to contribute to them. The question arises whether Open Science is properly aligned with the values of transparency, democracy, and accountability that the movement fiercely promotes, or if it ends up compromising such values. In order to address this question, let us examine two cases of interaction between Open Science and commercial interests: first, the interaction between open access and publication planning, and, second, the interaction between citizen science projects and participatory research.

Open Access and Publication Planning

Although Open Science is a much broader project than the implementation of open access to publications and data sets, open access is certainly one of the main pillars of Open Science. In order to set the common ground, let's start with a fairly standard definition of Open Access:

> Open Access (OA) is the provision of free access to peer-reviewed, scholarly and research information to all. It requires that the rights holder grants worldwide irrevocable right of access to copy, use, distribute, transmit, and make derivative works in any format for any lawful activities with proper attribution to the original author. Open Access uses Information and Communication Technology (ICT) to increase and enhance the dissemination of scholarship. OA is about Freedom, Flexibility and Fairness (UNESCO, 2012: 6).

The idea of open access is thus closely connected to Merton's norm of communalism or the idea that scientific knowledge does not belong to anyone in particular, but to all, i.e., to the human community at large. In this sense, every single person has a right, not only to access scientific results, but also to use that knowledge for "any lawful activities." Through Open Access scientists (or whoever has the IP rights) grant the public the right to use the knowledge produced at no cost.

Open access comes in different flavors, but two of the most common ways to implement it are through the "gold" and "green" models. Gold Open Access works basically like traditional publishing going through the peer-review process, only that authors (or their institutions) do not wave their IP rights to the journals, but instead pay a fee for publication. For example, PLOS, one of the most successful venues in this respect, charges a fee ranging between 1,495 and 2,900 dollars per published article. In contrast, the green model encourages authors to upload their articles in a public repository, which can be done pre-print and without peer-review, or post-print after the traditional peer-review process. Some of the most commonly used venues are, for example, arXiv, ResearchGate, and academia.edu.

Consider now the interaction between these open access practices and a commonly used strategy for successful publishing in the pharmaceutical industry, i.e., publication planning. As Sismondo has documented in detail, the pharmaceutical industry frequently uses publication planning firms to ensure that their articles are published in the best medical journals, reaching the vast majority of doctors who would potentially prescribe their medications. The process is carefully handled from the very early stages. Pharmaceutical companies out-source clinical trials through contract research organizations and publication planning firms make sure that the articles are (ghost)written in industry-friendly ways and that they are signed by "independent" researchers. Sismondo describes the process as follows:

> Most sponsored clinical trial research is handled by contract research organizations (CROs), the data they produce is typically analyzed by pharmaceutical company statisticians, papers are written by medical writers, and the whole process is guided and shepherded through to publication by planners and planning teams [. . .]. To gain the most commercial value from research, the papers publicizing it are written under the names of independent medical researchers [. . .] (Sismondo, 2009: 172).

One might be tempted to suggest that this is just an "a few bad apples" case, but the evidence suggests that publication planning is at play in about 40% of reports of clinical trials on new drugs

(Sismondo, 2009: 172). Also, this approach to the publishing process is driven undeniably by commercial interests, particularly by the pharmaceutical industry's interest of positioning their drugs in the market. For what other reason would pharmaceutical companies spend thousand and even millions of dollars buying reprints of articles and sending them to doctors worldwide, if it did not significantly contribute to profit? As Richard Smith, former editor of *BMJ*, claims:

> Finally, companies purchase large numbers of reprints of these trials. Sometimes they will spend more than $lm on reprints of a single study, and the profit margin to the publisher is huge. These reprints are then used to market the drugs to doctors, and the journal's name on the reprint is a vital part of that sell (Smith, 2003: 1204).

Mainstream academic publishing is thus a major burden for pharmaceutical companies to advertise the relevant research. It is slow and costly. In this sense, open access to scientific publications, would be a great gain for Big Pharma. Even with the gold model, pharmaceutical companies have a huge advantage: they can pay just 3,000 dollars for their article to be published in a well-established medical journal and then distribute it widely without any of the high costs of reprints; certainly, a huge gain.

The epistemic and social problems that arise from publication planning—e.g., the use of authors who did not even contribute to the design and research process, and the conflicts of interest that permeate pharmaceutical research—remain however untouched. In fact, since Open Access policies are only encouraged for publicly funded science, the pharmaceutical companies can open their research to the extent that they find favorable, keeping the publication planning process tightly closed. In other words, they are in a position to take advantage of strategies to open science when they see fit, while maintaining the research process closed when they do not. Furthermore, in this particular case, open access allows pharmaceutical companies to achieve a more efficient publication process, at lesser cost, contributing to strengthening this type of commercialized science.

Citizen Participation and Citizen Science

An important argument supporting Open Science stems from the idea that science should be more democratic and that the scientific process ought to be open to citizen participation. If we live in a democratic society and science is a key institution for democratic societies to flourish, then scientific projects and scientific results ought to be clearly aligned with society's needs. This plea for socially responsible science is not constrained to the Open Science movement but has also been a concern of philosophers of science lately (Kitcher, 2001; Kitcher, 2011; Douglas, 2009; Kourany, 2010). In addition, different types of participatory and collaborative methodologies have been developed, especially in the social sciences, in order to include substantive participation of stakeholders in scientific research, where the extent of citizen involvement varies, ranging from mere consent to engaged reciprocity (Wylie, 2015; see also; Koskinen and Mäki, 2016).

The rationale behind these participatory practices is both social and epistemic. On the one hand, participatory research aims to include the views of stakeholders who have been traditionally marginalized in the research process. Opening up the process contributes to increasing the diversity of views, thus reaching better and more reliable knowledge (on the epistemic advantages of diversity see: Longino, 2002 and Harding, 2015). On the other hand, participatory research also aims at social inclusion for those traditionally marginalized in the research process, fostering equality and social justice.

So far, citizen participation within Open Science has been very different from this ideal. Instead of substantive inclusion of stakeholders (their aims and needs), what we have seen is the development of different "citizen science" projects. The name citizen science might be confusing, for it can be understood both as a type of science driven by the concerns and needs of citizens or as scientific projects run by professional scientists, where citizens contribute to data gathering (Elliott, 2019). Let us focus on the latter, the type of citizen science projects that raise the most concern in terms of future commercialization. In these cases, citizen science projects are top-down approaches in which scientists open up the research process selectively, so that citizens can contribute free labor to the project through puzzle-solving or data-gathering. A clear example of this is the Rosetta@home project in which common citizens lend computer processing power while they are not using their devices, to help speed up the effort of protein folding (see https://boinc.bakerlab.org/).

As it turns out, protein folding is incredibly difficult to achieve through mere computer processing, where the computer keeps trying a very large number of possibilities until it finds a proper one. Apparently, the human mind is much faster in coming up with right answers to protein folding problems. For this reason, the Rosetta@home project was rapidly followed by other citizen science projects such as Foldit (see https://fold.it/portal/), a crowdsourcing computer game, where citizens can contribute to finding possible solutions to protein folding using their "human puzzle-solving intuitions." The website encourages this type of citizen collaboration, claiming that participants will contribute to better understanding disease-related proteins, which could eventually lead to curing diseases, such as HIV, cancer, or Alzheimer's.

Without critiquing the laudable goals of such projects (no reasonable person would be against finding the cure of mortal diseases and stop human suffering), they are far away from the sort of substantive citizen participation that advocates of democratizing science have in mind. As Powell and Collin claim, "[m]ost participatory exercises do not engage citizens beyond an event or a few weeks/months, and they do not build citizens' participatory skills in ways that would help them engage with scientists or policy makers independently" (Powell and Collin 2009: 327).

In addition, opening science through citizen science projects is also likely to contribute to further the commercialization of research. Although these projects are for the most part not for

profit, run by major research institutions (e.g., University of Washington), in collaboration with government agencies (e.g., The US National Institutes of Health and the US National Science Foundation) (again, publicly-funded research), once relevant results are obtained and potential medications appear in the horizon, the door is open for pharmaceutical companies to buy the results and process the patent. Here again, it seems that the incentives are in place for publicly funded research to do the hard work, now with the help of citizen's free labor, only for pharmaceutical companies to come in late in the process and profit. As long as universities are able to patent and sell results from government funded research (possible since Bayh-Dole), they have a huge financial incentive to do so, and contribute to the process of commercialization. In this sense, opening research for citizen science projects does not necessarily render socially responsible results.

CONCLUSION

In this paper, I have provided a philosophical analysis of Open Science, focusing on the asymmetrical treatment that the Open Science movement gives to public and private research. At first sight, the ideal of Open Science, which promotes the values of transparency, sharing, collaboration, and accountability, seems a promising guideline to address some of the epistemic and social justice problems that have emerged with the rampant commercialization of scientific research. The plea to open science, however, has primarily focused on publicly funded research, leaving research in the private sphere untouched. In fact, advocates of Open Science have used the business opportunities that will potentially emerge from opening publicly funded science as an argument in favor of Open Science, making clear that they are not particularly concerned with the problems of commercialized science. Given that the majority of scientific research is both funded and performed in the private sector today, and that commercially-driven science has important shortcomings that ought to be addressed, the argument of this paper shows that the Open Science movement should seriously consider the way it indirectly supports this commercialization. Taking an explicit stance for opening ALL science, would be more appropriately aligned with the values of transparency, accountability, inclusion, and democracy that the ideal of Open Science endorses.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

As the single author of the paper, MFP is responsible for the paper in its entirety.

FUNDING

Funding for this research project was provided by Universidad de los Andes.

ACKNOWLEDGMENTS

The author would like to thank the Helsinki Center for Digital Humanities at the University of Helsinki for their invitation to the Philosophy and History of Open Science Conference for which the first sketches of this paper were prepared. Special thanks to Jon Leefmann and the Friedrich-Alexander-Universität Erlangen-Nürnberg for their invitation to the Public Research and Private Knowledge Conference, for which the present version of the article was prepared. Also thanks to Phil Mirowski, for our conversations on the topic of Open Science, and to the two anonymous reviewers for their useful comments.

REFERENCES

Andrade, C. (2015). The primary outcome measure and its importance in clinical trials. *J. Clin. Psychiatr.* 76 (10), e1320–e1323. doi:10.4088/jcp.15f10377

Bartling, S., and Friesike, S. (2014). "Towards another scientific revolution," in *Opening science: the evolving guide on how the internet is changing research*. Editors S. Bartling and S. Friesike (Dordrecht, Netherlands: Springer), 3–16.

Bekelman, J., Li, Y., and Gross, C. (2003). Scope and impact of financial conflicts of interest in biomedical research: a systematic review. *J. Am. Med. Assoc.* 289 (4), 454–465. doi:10.1001/jama.289.4.454

Biddle, J. (2007). Lessons from the Vioxx debacle: what the privatization of science can teach us about social epistemology. *Soc. Epistemol.* 21 (1), 21–39. doi:10.1080/02691720601125472

Biddle, J. (2014). Can patents prohibit research? On the social epistemology of patenting and licensing in science. *Stud. Hist. Philos. Sci.* 45 (1), 14–23. doi:10.1016/j.shpsa.2013.12.001.

Bok, D. (2003). *Universities in the marketplace: the commercialization of higher education*. Princeton, NJ: Princeton University Press.

Doucet, M., and Sismondo, S. (2008). Evaluating solutions to sponsorship bias. *J. Med. Ethics.* 34 (8), 627. doi:10.1136/jme.2007.022467.

Douglas, H. (2009). *Science, policy, and the value-free ideal*. Pittsburgh, PA: University of Pittsburgh Press.

Elliott, K. (2008). Scientific judgment and the limits of conflict-of-interest policies. *Acc. Res.* 15 (1), 1–29. doi:10.1080/08989620701783725.

Elliott, K. (2019). The Kaleidoscope of citizen science. *Narrat. Inq. Bioeth.* 9 (1), 47–52. doi:10.1353/nib.2019.0017

EU Presidency (2016). Amsterdam call for action on open science. Available at: https://f-origin.hypotheses.org/wp-content/blogs.dir/1244/files/2016/06/amsterdam-call-for-action-on-open-science.pdf (Accessed July 23, 2020).

European Commission for Research and Innovation (2016). Guidelines on open access to scientific publications and research data in horizon 2020. Available at: http://ec.europa.eu/research/participants/data/ref/h2020/grants_manual/hi/oa_pilot/h2020-hi-oa-pilot-guide_en.pdf (Accessed July 23, 2020).

Eurostat. (2018). The EU in the World. Available at: https://ec.europa.eu/eurostat/en/web/products-statistical-books/-/KS-EX-18-001 (Accessed July 23, 2020).

Fecher, B., and Friesike, S. (2014). "Open science: one term, five schools of thought," in *Opening science: the evolving guide on how the internet is changing research*. Editors S. Bartling and S. Friesike (Dordrecht, Netherlands: Springer), 17–47.

Fernández Pinto, M. (2017). To Know or better not to: agnotology and the social construction of ignorance in commercially driven research. *Sci. Technol. Stud.* 30 (2), 53–72. doi:10.1177/0306312714565491

Fernández Pinto, M. (2018). Democratizing strategies for industry-funded medical research: a cautionary tale. *Philos. Sci.* 85 (5), 882–894. doi:10.1086/699720

G8 Summit. (2013). *Science Ministers statement*. Available at: https://www.gov.uk/government/publications/g8-science-ministers-statement-london-12-june-2013 (Accessed July 23, 2020).

Greenberg, D. (2007). *Science for sale: the perils, rewards, and delusions of campus capitalism*. Chicago, IL: University of Chicago Press.

Greenberg, D. S. (2001). *Science, money, and politics: political triumph and ethical erosion*. Chicago, IL: University of Chicago Press.

Harding, S. (2015). *Objectivity and diversity: another logic of scientific research*. Chicago, IL: University of Chicago Press.

Holman, B., and Elliott, K. (2018). The promise and perils of industry-funded science. *Philos. Compass.* 13, e12544. doi:10.1111/phc3.12544

Kitcher, P. (2011). *Science in a democratic society*. New York, NY: Prometheus Books.

Kitcher, P. (2001). *Science, truth, and democracy*. Oxford, UK: Oxford University Press.

Koskinen, I., and Mäki, U. (2016). Extra-academic transdisciplinarity and scientific pluralism: what might they learn from one another? *Eur. J. Philos. Sci.* 6 (3), 419–444. doi:10.1007/s13194-016-0141-5

Kourany, J. (2010). *Philosophy of science after feminism*. Oxford, UK: Oxford University Press.

Leonelli, S. (2013). Why the current insistence on open access to scientific data? big data, knowledge production, and the political economy of contemporary biology. *Bull. Sci. Technol. Soc.* 33 (1–2), 6–11. doi:10.1177/0270467613496768

Levin, N., Leonelli, S., Weckowska, D., Castle, D., and Dupré, J. (2016). How do scientists define openness? exploring the relationship between open science policies and research practice. *Bull. Sci. Technol. Soc.* 36 (2), 128–141. doi:10.1177/0270467616668760.

Lexchin, J., Bero, L., Djulbegovic, B., and Clark, O. (2003). Pharmaceutical industry sponsorship and research outcome and quality: systematic review. *Br. Med. J.* 326, 1167–1170. doi:10.1136/bmj.326.7400.1167

Longino, H. E. (2002). *The fate of knowledge*. Princeton, NJ: Princeton University Press.

Lundh, A., Lexchin, J., Mintzes, B., Schroll, J. B., and Bero, L. (2017). Industry sponsorship and research outcome. *Cochrane Database Syst. Rev.* 2, 33. doi:10.1002/14651858.MR000033.pub3.

Markowitz, G., and Rosner, D. (2002). *Deceit and denial: the deadly politics of industrial pollution*. California: Milbank Books on Health and the Public.

Maurer, S. (2007). Open source drug discovery: finding a Niche (or maybe several). *UMKC Law Rev.* 76, 405–434. doi:10.2139/ssrn.1114371

McGarity, T. O., and Wagner, W. E. (2008). *Bending science: how special interests corrupt Public health research*. Cambridge, MA: Harvard University Press.

Merton, R. (1974). *The sociology of science: theoretical and empirical investigations*. Chicago, IL: The University of Chicago Press.

Meskus, M., Marelli, L., and D'Agostino, G. (2018). Research misconduct in the age of open science: the case of STAP stem cells. *Sci. Cult.* 27 (1), 1–23. doi:10.1080/09505431.2017.1316975.

Michaels, D. (2008). *Doubt is their Product: how industry's assault on science threatens your health*. New York, NY: Oxford University Press.

Mirowski, P. (2018). The future(s) of open science. *Soc. Stud. Sci.* 48 (2), 171–203. doi:10.14293/s2199-1006.1.sor-life.clccall.v1

Murray-Rust, P., Neylon, C., Pollock, R., and Wilbanks, J. (2010). Panton principles, principles for open data in science. *Panton Principles*. Available at: https://pantonprinciples.org/ (Accessed July 23, 2020).

National Science Board (2018). *Science and engineer indicators 2018. NSB-2018-1*. Alexandria, VA: National Science Foundation. Available at: https://www.nsf.gov/statistics/indicators/ (Accessed July 23, 2020).

Nielsen, M. (2011a). Reinventing discovery: the New Era of networked science. Princeton, NJ: Princeton University Press.

Nielsen, M. (2011b). Open science now. TED Talk. Available at: https://www.ted.com/talks/michael_nielsen_open_science_now?utm_campaign=tedspread&utm_medium=referral&utm_source=tedcomshare (Accessed July 23, 2020).

OECD (2015). *Making open science a reality. OECD science, technology and industry policy papers, no. 25*. Paris: OECD Publishing. Available at: https://www.oecd-ilibrary.org/science-and-technology/making-open-science-a-reality_5jrs2f963zs1-en (Accessed July 23, 2020).

Oreskes, N., and Conway, E. M. (2010). *Merchants of doubt: how a handful of scientists obscured the truth on issues from tobacco smoke to global warming*. New York, NY: Bloomsbury Publishing.

Phelps, L., Fox, B. A., and Marincola, F. M. (2012). Supporting the advancement of science: open access publishing and the role of mandates. *J. Transl. Med.* 10 (13), 34–49. doi:10.1186/1479-5876-10-13

Powell, M., and Colin, M. (2009). Participatory paradoxes: facilitating citizen engagement in science and technology from the top-down? *Bull. Sci. Technol. Soc.* 29 (4), 325–342. doi:10.1177/0270467609336308

Proctor, R. N. (2011). *Golden holocaust: origins of the cigarette catastrophe and the case for abolition*. Berkeley, CA: University of California Press.

Radder, H. (2010). *The commodification of academic research: science and the Modern University*. Pittsburgh, PA: University of Pittsburgh Press.

Resnik, D. B. (2007). *The price of truth: how money affects the norms of science*. Oxford, UK: Oxford University Press.

Royal Society (2012). Report No.: 02/12 DES2482. Science as an open enterprise. Available at: https://royalsociety.org/~/media/policy/projects/sape/2012-06-20-saoe.pdf (Accessed July 23, 2020).

Sappington, T. W., Ostlie, K. R., DiFonzo, C., Hibbard, B. E., Kurpke, C. H., Porter, P., et al. (2010). Conducting public-sector research on commercialized transgenic seed. *GM Crops.* 1 (2), 55–58. doi:10.4161/gmcr.1.2.10833

Schroeder, R. (2007). E-research infrastructures and open science: towards a new system of knowledge production? *Prometheus* 25 (1), 1–17. doi:10.1080/08109020601172860.

Sidler, M. (2014). "Open science and the three cultures: expanding open science to all Domains of knowledge Creationin" in *Opening science: the evolving Guide on How the Internet is changing research*. Editors S. Bartling and S. Friesike (Dordrecht, Netherlands: Springer), 81–85.

Sismondo, S. (2007). Ghost management: how much of the medical literature is shaped behind the scenes by the pharmaceutical industry? *PLoS Med.* 4 (9), 286. doi:10.1371/journal.pmed.0040286

Sismondo, S. (2008). Pharmaceutical company funding and its consequences: a qualitative systematic review. *Contemp. Clin. Trials.* 29 (2), 109–113. doi:10.1016/j.cct.2007.08.001.

Sismondo, S. (2009). Ghosts in the machine: publication planning in the medical sciences. *Soc. Stud. Sci.* 39 (2), 171–198. doi:10.1177/0306312708101047.

Slaughter, S., and Rhoades, G. (2004). *Academic capitalism and the new economy: markets, state, and higher education*. Baltimore, MD: Johns Hopkins University Press.

Smith, R. (2003). Medical journals and pharmaceutical companies: uneasy bedfellows. *Br. Med. J.* 326 (7400), 1202–1205. doi:10.1136/bmj.326.7400.1202.

Turner, E., Matthews, A., Linardatos, E., Tell, R., and Rosenthal, R. (2008). Selective publication of antidepressant trials and its influence on apparent efficacy. *N. Engl. J. Med.* 358, 252–260. doi:10.1056/nejmsa065779

UNESCO (2012). Policy guidelines for the development and Promotion of open access. United Nations Educational, Scientific and Cultural Organization. Available at: http://unesdoc.unesco.org/images/0021/002158/215863e.pdf (Accessed July 23, 2020).

UNESCO (2015). UNESCO science report: towards 2030. United Nations Educational, Scientific and Cultural Organization. Available at: https://en.unesco.org/unescosciencereport (Accessed October 15, 2020).

Waltz, E. (2009). Under wraps. *Nat. Biotechnol.* 27 (10), 880–882. doi:10.5040/9781770917255.00000005

Wise, N. (2006). Thoughts on the politicization of science through commercialization. *Soc. Res.* 73 (4), 1253–1272. doi:10.1007/978-90-481-9051-5_17

Wylie, A. (2015). "A plurality of Pluralisms: collaborative practice in archaeology," in *Objectivity in science: new perspectives from science and Technology studies*. Editors F. Padovani, A. Richardson, and J. Tsou (Dordrecht, Netherlands: Springer), 189–210.

Conflict of Interest: The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

ORIGINAL RESEARCH
published: 14 December 2020
doi: 10.3389/frma.2020.599506

The Limits of Conventional Justification: Inductive Risk and Industry Bias Beyond Conventionalism

Miguel Ohnesorge *

Department of History and Philosophy of Science, University of Cambridge, Cambridge, United Kingdom

This article develops a constructive criticism of methodological conventionalism. Methodological conventionalism asserts that standards of inductive risk ought to be justified in virtue of their ability to facilitate coordination in a research community. On that view, industry bias occurs when conventional methodological standards are violated to foster industry preferences. The underlying account of scientific conventionality, however, is insufficient for theoretical and practical reasons. Conventions may be justified in virtue of their coordinative functions, but often qualify for posterior empirical criticism as research advances. Accordingly, industry bias does not only threaten existing conventions but may impede their empirically warranted improvement if they align with industry preferences. My empiricist account of standards of inductive risk avoids such a problem by asserting that conventional justification can be pragmatically warranted but has, in principle, only a provisional status. Methodological conventions, therefore, should not only be defended from preference-based infringements of their coordinative function but ought to be subjected to empirical criticism.

Keywords: values in science, conventionalism, methodological conventions, inductive risk, randomized controlled trials, industry bias

OPEN ACCESS

Edited by:
Jon Leefmann,
University of Erlangen Nuremberg, Germany

Reviewed by:
Cornelis Menke,
Johannes Gutenberg University Mainz, Germany
Gussai H. Sheikheldin,
Science, Technology and Innovation Policy Research Organization (STIPRO), Tanzania
David Harker,
East Tennessee State University, United States

***Correspondence:**
Miguel Ohnesorge
mo459@cam.ac.uk

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *27 August 2020*
Accepted: *23 November 2020*
Published: *14 December 2020*

Citation:
Ohnesorge M (2020) The Limits of Conventional Justification: Inductive Risk and Industry Bias Beyond Conventionalism. Front. Res. Metr. Anal. 5:599506. doi: 10.3389/frma.2020.599506

INTRODUCTION

In 2018, more than 68% of all R&D funding in the United Kingdom stemmed from private donors (National Office of Statistics 2020), a number that still appears moderate when compared to 76% in China and 78% in South-Korea (Eurostats 2019). As Bennett Holman and Kevin Elliot rightly note in a recent meta-review, philosophers have overwhelmingly taken this prevalence of industry funding to be worrisome. It has been argued recurrently that industry funding causes epistemically detrimental "industry bias" across various fields of scientific research (Holman and Elliott 2018, 2). Miriam Solomon, echoing the *British Medical Journal* and *Institute of Medicine*, has recently warned that industry bias poses "the greatest known systematic threat to the objectivity of medical research" (Lo and Field, 2009; Moynihan et al., 2019; Solomon 2020, 439).

However, such strong normative claims require robust epistemological grounds. Surely, industry funded research produces different results, i.e., such that are useful for the respective industry actors. But for what reasons exactly are we holding these differences to be epistemically detrimental? For external interests to qualify as epistemic threats, we ought to have strong reasons that the epistemic standards of our research would be higher without them. The appeal to any such standard, however, sits unwell with recent philosophical claims about the prevalence of so-called inductive risks in various internal stages of scientific research. Inductive risk, for

now, can be defined as the risk of "wrongly accepting or rejecting a hypothesis on the basis of evidence" (Biddle 2016). Inductive-risk judgements, so the usual story goes, are non-epistemic value judgements about research design, conduct, and communication. If non-epistemic considerations are so ubiquitous in scientific practice, how can we identify epistemic (as opposed to ethical) standards on which "industry bias" infringes?

Torsten Wilholt has developed a conventionalist solution to this problem, which he later embedded in a position called *methodological conventionalism*. For a methodological conventionalist, conventional standards of inductive risk are epistemically justified due to their ability of facilitating coordination in a scientific community, thereby improving the collective pursuit of knowledge. In other words, sharing conventional methodologies, which implicitly determine standards of inductive risk, is necessary for scientists to engage in coordinated inquiry. Industry bias, on such an account, may be described as the infringement of such conventions for increasing the likelihood of a result preferred by industry funders. Wilholt's conventionalism has received a warm reception in recent literature on industry funded science and inductive risk.[1]

I will argue, however, that it faces two interlinked problems. First, the conventionalist concept of bias is not able to account for one of the most central epistemic flaws of industry funded research: empirically ineffective conventions. On my reading, this practical problem results from a second, theoretical weakness of Wilholt's position. The role of conventional justification in science is not characterized sufficiently through appeals to coordination. Although research methodologies can be justified in virtue of their coordinative function, scientists should subject conventional choices to posterior empirical criticism. Based on this insight, I propose a *permissive empiricism*. For a permissive empiricist, the conventional justification of standards of inductive risk can be permitted in light of contextual constraints but is always provisional, i.e., it ought to be substituted by empirical justification at a later point of inquiry. My account thereby preserves Wilholt's notion of industry bias, while, permitting empirical criticism of *structural industry bias*. The latter results from the institution or perpetuation of empirically ineffective conventions that serve industry interests.

The plan is as follows. First, I reconstruct Wilholt's conventionalist account of industry bias and its motivation in the argument from inductive risk (AIR). Second, I review whether his position, methodological conventionalism, is well motivated in light of recent work on AIR. Third, I use the case of pharmaceutical trials to show how methodological conventionalists are unable to adequately assess structurally flawed conventions. I close by proposing an empiricist account of standards of inductive risks which preserves Wilholt's definition of bias while allowing for the empirical criticism of flawed conventions.

WILHOLT ON INDUCTIVE RISK AND INDUSTRY BIAS

"Industry bias," roughly, describes epistemically detrimental effects that industry preferences have on the conduct of scientific research. However, as "bias" is certainly a polysemic concept (Resnik 2000), it is necessary to distinguish between (at least) two different senses in which it is used in recent philosophical literature. In a broad sense, industry bias subsumes all factors that increase the likelihood of research to produce results preferable to its industry funders. As such, even the intentional spreading of misinformation in a research community or the outright fabrication of results display instances of so-called "intransigent bias" (Holman and Bruner 2015). In a narrower sense, "industry bias" operates more subtly, as biased researchers select research designs, data interpretations, and ways of communicating that are more likely to produce outcomes preferable to their industry funders. Indeed, statistical findings on biomedical and chemical studies indicate that industry funded studies are significantly more likely to obtain results that serve the interests of their funders (Davidson 1986; Barnes and Bero 1996; Barnes et al., 2006; Schott et al., 2010; Volz and Elliott 2012; Lundh et al., 2017). It is this latter form of industry bias which will be my concern in what follows.

In an influential article, Torsten Wilholt has argued that the argument from inductive risk (AIR) challenges the epistemically detrimental nature of industry bias (Wilholt 2009). Discussions of inductive risk, popularized by Richard Rudner and Carl Hempel, are based on the assumption that the choice of a level of evidential confirmation for accepting a hypothesis is epistemically underdetermined (Rudner 1953; Hempel 1965).[2] The threshold of evidence that scientists accept as sufficient for a claim to be confirmed displays the risk of possible inductive error that they are willing to take. In making a risk-judgement, researchers face a trade-off between the risk of accepting a claim that is in fact false (false positive) and the risk of rejecting a hypothesis that is in fact true (false negative). Based on Carl Hempel's account, Heather Douglas has advocated what is usually taken to be the strongest version of AIR, which holds that if false-negatives or false-positives entail non-epistemic consequences, scientists must base their risk-judgements on non-epistemic values. Moreover, Douglas argues, experiments are permeated by such decisions about inductive risk at multiple internal stages, namely when scientists make methodological choices about statistical significance, qualitatively characterize their evidence, and interpret their results. In all these steps, researchers ultimately have to make epistemically underdetermined decisions for which they ought to estimate the consequences of potential false positives and false negatives based on non-epistemic values (Douglas 2000, 577–578).

Whether Douglas's version of AIR in fact offers a prescriptive or solely a descriptive claim need not concern us further here, as it is the descriptive part of her argument that motivates Wilholt's

[1]The only criticism may be found in Betz (2013) and is discussed explicitly in section "*Is Methodological Conventionalism Well Motivated?*".

[2]For the statistical origin of inductive risk see: Wald (1942), Churchman (1948).

risk conventionalist account of industry bias (Wilholt 2009, 94–95). He discusses two case-studies of inductive risk decisions, which would typically be characterized as instances of industry bias:

- Bisphenol A, which is used as a monomer in polycarbonate plastic and has toxic effects due to its similarity to human estrogen, was shown to be carcinogenic in many government-funded studies. However, industry funded experiments were continuously conducted on a strain of laboratory rats known to be insensitive to estrogen, effectively establishing the non-toxicity of bisphenol A (vom Saal and Hughes 2005).
- Exposure to vinyl chloride, likewise used in the production of polycarbonate plastics, is correlated with a rise in mortality rates and a significant increase in liver and brain cancer risk. The Chemical Manufacturers' Association funded various studies based on which the legal regulation of vinyl chloride was prevented. In one of those studies, the famous British epidemiologist Richard Doll dismissed a previous review that claimed to have shown that exposure to vinyl chloride is correlated to liver and brain cancer, and would therefore raise the risk of mortality significantly. Doll argued that all brain cancer cases must be excluded from the mortality risk calculation, as the link between vinyl chloride and brain cancer was only postulated in the very same study, thus having not gone through independent testing (Sass et al., 2005).

Given that AIR holds, Wilholt's argument goes, industry biases in research on bisphenol A and vinyl chloride may be re-interpreted as potentially justified non-epistemic value judgements about an appropriate level of inductive risk. Both substances, industry researchers might argue, have important applications, as they are widely used to produce polymers for the manufacture of pipes, medical instruments, plastic wraps or wall covers. Non-industry researchers, thus, may in turn be accused of accepting an unduly high risk of false positives. Therefore, the supposedly biased laboratory rat selection and unusually high demand for robustness could be justified in recourse to possible non-epistemic consequences. A similar reasoning, Wilholt shows, can be applied to justify substandard pharmaceutical trials in which drugs are tested against placebos instead of their most effective alternatives (Wilholt 2009, 93).

Surely, there are many *ethical* objections to be raised against these non-epistemic arguments. Cancer risk may be agreed upon to carry more ethical weight than a shortage of plastic wraps or the use of costlier alternative polymers in medical instrument manufacturing (Chiellini et al., 2013). Likewise, we might agree that "public risks" are to be taken more seriously than producer risks (Biddle and Leuschner 2015). However, given that AIR holds, we seem to be left with the conclusion that there is no *epistemic* objection to the supposedly biased research on bisphenol A and vinyl chloride.

However, both findings were not only overturned by the toxicological research community but were deemed to be instances of epistemic failure. Thus, toxicologists invoked a methodological standard that was infringed by the risk decisions in question. Given the considerations above, such apparent standards, and the levels of inductive risk they implicitly or explicitly determine, need an epistemic justification. While Wilholt thinks that there is such a justification, he claims it cannot be grounded in classical individualist epistemology. Rather, it emerges from the *social* epistemology of scientific research. While Wilholt concedes to AIR that we have no purely epistemic reasons to select one specific balance of risk, he contests that methodological standards are nonetheless needed to facilitate the trust of researchers in each other's results. In turn, trust is *epistemically* warranted because it is required for collective empirical success. In other words, settling on a standard is a "problem of coordination" (Wilholt 2013, 233). Problems of coordination are not solved by empirical evidence or individual rationality but settled by the establishment of a conventional equilibrium between the conflicting utilities of agents. Even if the exact shape of that equilibrium (i.e., the exact balance of inductive risk) cannot be determined rationally, settling on *some* equilibrium maximizes the utilities of all epistemic agents involved. Thus, following conventional methodologies is in the interest of all researchers, as it facilitates mutual trust. As Wilholt's argument offers a trust-based justification of the standards of inductive risk inherent in conventional research methodologies, he called his view *methodological conventionalism*. For a methodological conventionalist, industry bias can be defined (and criticized) as the "the infringement of an explicit or implicit *conventional* standard of the respective research community in order to increase the likelihood of arriving at a preferred result" (Wilholt 2009, 99).

More recently, Wilholt has presented a more sophisticated version of this argument that distinguishes between conventions simpliciter and epistemic trust. Scientists rely on conventional methodologies to avoid constant deliberations about such implicit risk decisions as highlighted by Douglas. However, even standardized methodologies leave leeway for active value-judgementsto be taken by researchers. In such cases, "reliance presupposes much more than just that other scientists work dependably and professionally in keeping with the rules of the trade. It presupposes that they have the right attitude toward what they are doing—an attitude whose absence might be considered not just regrettable but to a certain degree blameworthy." (Wilholt 2013, 249)

Wilholt's modified view suggests that epistemic trust characterizes a stronger kind of reliance that extends beyond implicit standards to include active value judgements. In both versions of his argument, however, the underlying logic remains a conventionalist one. When considerations about non-epistemic consequences enter methodological decision processes, conventional standards or attitudes are needed to fix *some* equilibrium in the trade-off between the risks of false positive and false negatives. As Douglas shows convincingly that such considerations cannot be fully eliminated in scientific research, social coordination can only be achieved if researchers follow methodological conventions.

IS METHODOLOGICAL CONVENTIONALISM WELL MOTIVATED?

So far, I have shown that methodological conventionalism is motivated by the prevalence of non-epistemic risk decisions in scientific research. Before discussing the conventionalist account of bias, I will review this motivation in light of recent responses to AIR. In principle, there are two ways of avoiding the conclusion that the ubiquity of inductive risks in scientific research warrant non-epistemic value judgements. One may either show that 1) there is a non-conventional justification of a certain standard of inductive risk or 2) that inductive risk decisions can be sufficiently avoided by scientists.

Strategy 1) is usually based on Bayesian considerations. Wilholt himself is concerned with this, discussing Isaac Levi's objection to the mid-20th century variant of AIR (Levi 1962). Levi argued that to have a long run approximation of the value of our priors toward *de facto* truth (Pr [H] = 1) or falsity (Pr [¬H] = 1) of hypotheses, there is a purely epistemic demand to have a fixed identical threshold *L* for the acceptance of the truth or falsity of a claim, outlawing any trade-offs. However, this oversimplifies the aims of scientific activities, as researchers are not simply looking for true claims. Indeed, there is a nearly infinite amount of arbitrary truths that are scientifically uninteresting. Instead, they are looking for true claims that are significant relative to broader epistemic goals. Even in "basic research" such epistemic significance is indicated by certain epistemic values such as fruitfulness, explanatory scope, or predictive accuracy. Between different epistemic aims or values, however, there can exist context-dependent or systematic trade-offs, casting doubt on the practical realizability of a generally fixed *L*-value (Kuhn 1977; Longino 1996, 44). Instead, an ideal researcher can, at best, follow a utility matrix that prioritizes communicating the truth (i.e., genuinely true or genuinely false claims) over refusing communication or miscommunicating. Such a "weak" commitment to truth, however, leaves leeway to significant trade-offs and consequently does not serve to rationally set appropriate standards of inductive risk.

After Wilholt's first conventionalist proposal, however, philosophers have scrutinized whether inductive risk might not be sufficiently avoidable after all, thus opting for strategy 2). Such arguments defend the operability of what has been called the value-free ideal (VFI). VFI can be defined as the demand that "the justification of scientific findings should not be based on non-epistemic (e.g., moral or political) values" (Betz 2013, 208). If the operability of VFI can be rescued, one might think, the motivation of risk conventionalism collapses, as any industry preferences can be identified as non-epistemic intrusions.

The most influential recent argument for VFI was put forward by Gregor Betz, explicitly targeting, among other views, Wilholt's adherence to a variant of AIR (Betz 2013, 208). While conceding that the inductive risk in making binary judgements about accepting or rejecting a hypothesis cannot be eliminated, he denies that scientists must make judgements of that form. Instead of acceptance/rejection assertions, scientists can qualify their results by stating all instances of inductive risk in their research. For example, they may hedge their claims by elucidating the statistical certainty or significance of their findings (e.g., *x* is correct "with a certainty of 95%" or x is significant "to a *p*-value of 0.04"). Likewise, they may modalize or conditionalize their conclusions more generally (e.g., "it is possible/unlikely/plausible" that x is correct or "if we admit a possible error of *y*/a model organism *z*," *x* is correct). While one may contest that every hedged claim comes with second-order risk as "probabilistic hypotheses are just as open to inductive risks as others" (Brown 2013, 834; Douglas 2009, 85), it is not clear whether this actually poses a threat for Betz's defense of the VFI. His argument does not aim to show that inductive claims can be completely free of risk, but qualified "beyond reasonable doubt." If he is correct about this, scientists may avoid the impact of non-epistemic considerations on their results by acknowledging and stating the risks of their inductive inferences in form of probabilistic, modal, or conditional claims. Contra Wilholt, industry bias may, on this account, be identified as epistemically detrimental if it inhibits the proper communication and/or minimization of inductive risks (Betz 2013, 216).

For Betz's proposal to have any bearing on Wilholt's use of AIR, it needs to be shown that it can in fact be operationalized successfully to avoid value-judgements and thus offers an operable epistemic standard against which industry biases can be assessed. However, the validity of Betz's own example for value freedom in action, the *Guidance Note for a consistent treatment of uncertainties* by the Intergovernmental Panel on Climate Change (IPCC) (Mastrandrea et al., 2011), has been strongly contested. The Guidance Note tries to map inductive risk based on "states of scientific understanding," by reference to which scientists involved in a global evaluation of climate research are supposed to qualify their findings. However, as Katie Steele has argued based on an older IPCC report, such confidence scales (e.g., range of 1–10, intervals of 1) are too coarse-grained to properly communicate inductive risks. This situation is complicated further by the underdetermined classifying of some of those intervals as displaying "high," low," or "medium" confidence (Steele 2012). The problem of applying Betz's hedging principle is not limited to the case considered by Steele, but persist within the newer IPCC reports (Steel 2016; Frisch 2020). Moreover, Stephen John has shown that the IPCC even commits to inductive risk judgements by including or excluding peer-reviewed papers. In its fourth report, scientists refrained from making a prediction regarding the melting of the West Antarctic Ice Shield because they excluded a study not yet gone through peer-review (O'Reilly et al., 2012; John 2015, 7).

Beyond its lack of applicability to the IPCC-case, however, John argues that a modest version of the VFI (VFI_{modest} in what follows) can be rescued from Betz's proposal. It seems that the idea of hedging claims is a fruitful one in principle, as it decreases inductive risks. He proposes that hedging claims "beyond reasonable doubt" is enough to consolidate VFI_{modest}, even if it is less reliable in eliminating non-epistemic value judgements than Betz concedes. Reformulating Duncan Pritchard's epistemic-safety definition of knowledge, John argues that "knowledge" itself can be roughly defined as our body of

claims that are true beyond reasonable doubt. If scientists hedge their claims beyond reasonable doubt, they thus pursue a purely epistemic aim, namely genuine knowledge (Pritchard 2005; John 2015, 163). One problem for VFI_{modest} approximates canonical issues around Popperian testability. The history of science gives evidence that proliferating doubtful hypotheses, contrary to norms of epistemic caution, can be epistemically beneficial in the long run (Lakatos 1999; Feyerabend 2002; Chang 2014, ch.2). Thus, scientists have, in certain cases, purely epistemic reasons for not following VFI_{modest}. Beyond such broader issues, VFI_{modest} certainly does not offer an epistemic standard strong enough to criticize the cases of industry bias that Wilholt discusses. Richard Doll dismissed the health hazards of vinyl chloride precisely by appealing to epistemic caution (Wilholt 2009, 93). Thus, the adherent of VFI_{modest} will continue to be puzzled by the actions of the toxicological community which not only overturned Doll's research findings, but retrospectively deemed their initial acceptance an instance of epistemic failure (Sass et al., 2005).

THE PROBLEM OF FLAWED CONVENTIONS

I have argued that the motivation of methodological conventionalism by appeal to inductive risk decisions in various stages of scientific research is robust in light of recent criticism of AIR. Remember that Wilholt further intends his position to justify epistemic criticisms of industry bias as "the infringement of an explicit or implicit *conventional* standard of the respective research community in order to increase the likelihood of arriving at a preferred result" (Wilholt 2009, 99). Some of the most strongly voiced criticism of industry influence on scientific research, however, does not target infringements on methodological conventions, but explicitly points to flaws *in* such conventions. For example, a recent collaborative article in the British Medical Journal states:

"Sponsoring companies have obvious financial incentives to overstate product benefits and downplay harms. But these incentives are enabled by our imperfect methods of evaluation, which can be exploited in myriad ways, consciously or unconsciously, at all stages of the process." (Moynihan et al., 2019, 2).

To better understand the implications of flawed standards for methodological conventionalism, let us look at an illustrative example: pharmaceutical drug trials involving human subjects. As Jacob Stegenga has argued at length, the current organization of the system of randomized controlled drug trials (RCTs) hinders the detection of harms. RCT testing is split up in three separate phases (P_1-P_3). Only drugs that were successful (i.e., harm-free) in P_1 successively enter P_2 and P_3 trials. However, an estimated 95% of P_1 results remain unpublished by the pharmaceutical companies owning the studies' publishing rights (Decullier et al., 2009). Thereby, Stegenga argues, relevant evidence about the harmfulness of the tested molecules, the broader classes of molecule they belong to, as well as medical drugs overall is lost:

"Any tested molecule x is a member of the class of molecules of type T, and this class is itself a member of the class of all drugs D. Evidence from a phase 1 trial on x is relevant, obviously, to the harm profile of x, but is also relevant to the harm profile of T (albeit more indirectly), and is also relevant to the harm profile of D (more indirectly still)." (Stegenga 2018, 138).

The unavailability of a majority of the evidence about the harms of *x*, *T*, and *D* constrains the reference class based on which the prior probabilities for harmful effects in future drug trials are determined. More formally: the conditional probability Pr(K|E) of *x* being harmful in a future P_1, P_2, or P_3 trial, where K is the hypothesis that *x* is harmful and *E* the relevant new evidence, will always be unduly low due to the constrained reference class of K. Moreover, due to the constrained evidence about the harm profile of *T* and *D*, the value of prior probabilities in future trials involving molecules of the same class as *x*, and, in a less significant manner, any other molecule, will decrease. Overall, the prevalence of harms of any specific drug and of pharmaceuticals in general can therefore be expected to be way higher were it not for the withholding of evidence from P_1 trials (Stegenga 2018, 138–139). Stegenga identifies an apparent trade-off between two forms of statistical power, where "statistical power" refers to "the sensitivity of a trial to detect an effect of the intervention under investigation, when there is such an effect to be detected" (Stegenga 2018, 141). Statistical power is a function of a trial's effect size, the number of subjects under investigations, and the variability of the data. In the case of P_1, P_2, and P_3 statistical trials, the $power_H$ to detect harms partially trades-off with the $power_B$ to detect benefits of drugs.

It is possible to directly connect the RCT case to Wilholt's AIR-based line of reasoning. In fact, one can easily recast the choice of a balance between the two types of statistical power as a choice of a standard of inductive risk. If $power_H$ increases, we face a higher risk of false positives (harmless drugs that are wrongly assessed to be harmful), if $power_B$ increases, we face a higher risk of false negatives (harmful drugs that were wrongly assessed to be harmless). Now, with a bit of counterfactual reasoning, it is possible criticize the empirical performance of the current standard of inductive risk while avoiding an ethical judgment about the appropriate balance of risks. Consider the possibility of publishing more P_1 results. As a consequence, the absolute power to detect harms in RCT system will increase *without* decreasing the absolute power to detect benefits. While the *relative* balance between $power_H$ and $power_B$ tips toward $power_H$, our methodological decision would not decrease the ability of pharmaceutical drug trials to detect beneficial drugs. The changes in the standard of inductive risk are not the result of weighing ethical consequences, nor solely of changing coordinative conventions. If all P_1 results were to be published, we would improve the *empirical* performance of the research methodology in question. The currently operative conventional standard of risk in the pharmaceutical RCTs, it follows, has been *empirically ineffective*.

Now, methodological conventionalism only licenses epistemic criticism of those preference-based decisions that infringe on conventional standards of inductive risk. The testing for harms by

pharmaceutical companies, however, does not infringe on the current conventions but exploits and perpetuates their inherent flaws. As a consequence, it does not qualify as being biased in the way defined earlier. In fact, Wilholt admits "that it might be claimed that sometimes the conventional standards of a research community are themselves distorted by interests and preferences in an epistemologically problematic way" (Wilholt 2009, 2). If all that would be at stake is the adequate domain for the definition of bias as infringements on conventions, such a disclaimer might suffice to avoid the problem. We could simply exempt flawed conventions from our definition of industry bias and treat them as a separate kind of epistemic problem. Methodological conventionalism, however, goes beyond a conventional concept of bias, as it aims to offer a general account of the justification of standards of inductive risk. The RCT case offers a counterexample to such a view as it illustrates that inductive risk equilibria that are set by conventionally accepted methodologies can be epistemically criticized beyond their ability to facilitate coordination. As it stands, conventionalists appear to be unable of accounting for the purely empirical target of such criticism even if we (quite artificially) separate them from the problem of industry bias.

Wilholt himself seems aware of the problem, as he offers a reworked account of conventionality in a 2016 paper, which appears to be more promising regarding its ability to deal with the problem of flawed conventions (Wilholt 2016). There, he argues that discussions of AIR have unduly neglected the rate at which varying methodological conventions lead researchers to empirical results. While inductive risk judgements are generally taken to involve a trade-off between the reliabilities of negative and positive results, Wilholt now takes them to involve an additional third dimension (see also: Steel 2016). He characterized the latter as a method's "power", defined as the rate at which it "generates definitive results, given a certain amount of effort" (Wilholt 2016, 227) Hence, what is desirable in a method of inquiry (from an epistemic perspective) can only be captured by considering all three dimensions: the reliability of positive results, the reliability of negative results, and the method's power. For each method, these three magnitudes form a triple that I will call the inquiry's distribution of inductive risks (Wilholt 2016, 227).

Thus, the adoption of methodological standards is not solely coordinating the risks involved in positive and negative reliability but is likewise constrained by its function in delivering new findings. Could a methodological conventionalist, then, account for cases like the RCT system by invoking changes in such three-dimensional *distributions of inductive risks*? I do not think so. While Wilholt's focus on inquiries' absolute power correctly acknowledges that different conventional standards of inductive risk do vary in their empirical effectiveness, he does not concede that the latter can be improved without sacrificing the reliability of positive or negative results:

"The three dimensions of the vector are antagonistic to each other in the sense that each of them alone can easily be increased at the cost of one or both of the others, so that any methodological choice involves a trade-off between the three dimensions." (Wilholt 2016, 228) (my italics).

In light of the above discussion, the claim that "any methodological choice" trades-off against positive and negative reliability of standards of methodologies seems incorrect. In fact, it appears to be possible to *increase* the RCT system's ability to 1) deliver empirical results and 2) to avoid false negatives, without thereby *decreasing* its ability to avoid false positives. By not acknowledging the possibility of such empirical improvements, methodological conventionalism fails to license a robust criticism of empirically ineffective conventions and so does not offer sufficient grounds to discuss the epistemic dangers of industry funded science.

CONVENTIONS AND EMPIRICAL CRITICISM

The problem of flawed conventions indicates the shortcomings of methodological conventionalism. As an account of the justification of methodologies choices, it does not license criticisms of purely empirical (as opposed to coordinative) flaws in conventionally justified standards of inductive risk. In what follows, I want to propose an alternative account, dubbed *permissive empiricism.* My proposal constructively departs from methodological conventionalism by introducing a more sophisticated account of conventionality in science. In doing so, I aim to preserve the merits of Wilholt's focus on the role of conventions in discussing inductive risk decisions and identifying industry bias. In fact, I hope that the position that I will develop can offer a two-fold constructive *improvement* on methodological conventionalism's theoretical and practical weaknesses. Theoretically, I hope to offer a more sophisticated analysis of conventionality that aligns Wilholt's insights with an uncontroversial empiricism. Practically, my position should offer a more powerful framework to identify instances of industry bias.

Recall that Wilholt understands the choice of standards of inductive risk as a "problem of coordination" (Wilholt 2013, 233). If scientists do not trust each other to take similar risks in their research, the research community's coordination suffers, which negatively impacts its overall epistemic success. Thus, it is epistemically warranted that the risk decisions of "information producers [. . .] and information users [in the research community] are (approximately) the same" (Wilholt 2013, 248). As the RCT case in section four shows, this leaves the balance of inductive risk 1) a matter of convention, and, therefore, 2) not liable to direct empirical criticism. Although I agree with Wilholt that methodological standards of inductive risk can be justified in virtue of their conventional function in facilitating coordination, I will argue that 2) does not follow, as conventions are not immune to empirical criticism. In fact, scientist *ought* to aim at providing such criticism to avoid perpetuating empirically ineffective conventions.

The long history of conventionality in the philosophy of science offers a good starting point to understand how standards of inductive risk can be subjected to empirical

criticism. Ernst Mach originally introduced the problem of coordination in a discussion of thermometric intervals. As thermometric intervals are given as a function of the expansion rate of a thermoscopic substance, he argued, we ought to choose a substance expanding as uniformly with increasing temperature as possible. However, "uniformity of expansion" presumes the thermometric scale we want to define, given that we have no scale-independent possibility of operationalizing "temperature". Thus, we face an undetermined decision between different standards of measurement which will all fulfill a conventional purpose of facilitating coordination (Mach and McGuiness, 1986, 52). Such problems seem to require different responses than the decisions typically faced by scientists. Generally, we would want scientists to choose explanations, theories, or methods based on some form of available or expected empirical evidence. When mid-nineteenth century physicists chose whether to base the thermometric intervals on the expansion rates of mercury, air, or alcohol, however, they had no reason to think that any one of those would perform better empirically, i.e., record more accurately the changes in absolute temperature.[3] If chosen as standard, each thermoscopic substance would simplify certain kinds of measurement situations while making others more complicated. By side-stepping this underdetermined choice and agreeing on *some* standard, however, physicists could establish an equilibrium between those partially conflicting utilities.[4] Thereby, they did not settle conclusively the scientific problem of mapping "temperature" onto the physical world, but epistemically improved the social pursuit of temperature research.

Acknowledging that some aspects scientific practice involve conventionality in the above sense, however, is widely accepted and does not yet ground a conventional*ism*. As David Lewis points out, conventional*ism* entails some additional beliefs about the *power* of conventions. Thus, subscribing to a conventionalism about a goal-oriented social practice *x*, expresses a view about the extent to which the organization of x is settled by coordinative equilibria–as opposed to appeals to empirical evidence (Lewis 1969, 4). Thus, if we agree with Wilholt that scientists can avoid making inductive risk decisions by following methodological conventions, it remains open how far-reaching the implications of that insight are. Flawed standards as in the RCT-case discussed above, in particular, force us to evaluate the degree to which standards of inductive risk remain a matter of social coordination and to which degree the methodologies by which they are entailed can be criticized empirically. Here, a return to the well-studied role of conventionality in thermometry proves insightful. As Hasok Chang notes in his canonical study on the subject, thermoscopic substances were only chosen for conventional reasons *initially*, yet meticulously tested on variations against each other afterward (Chang 2004, 59). That is, although first attempts at standardization were based on conventional decisions, thermometric standards could eventually be subjected to comparative empirical scrutiny. In the long run, the relative performance of alternative ways of standardizing temperature (i.e. in reference to different thermoscopic substances) could be compared based on the substances' relative performance. While choices of measurement standards were thus based on both their conventional ability to facilitate coordination and their empirical success, the *power* of conventionality in such choices *decreased over time*. Thus, a conventional judgment about the fixation of a coordinative equilibrium successively made room for a growing body of empirical evidence.

Now recall the case of harm detection in RCTs. On the conventionalist account, a standardization of inductive risk, i.e. a fixed balance between power_H and power_B, is epistemically warranted and the methodological choices constituting that standard can be justified in virtue of their conventionality. However, I take Stegenga's analysis to show that a modification of the current methodological conventions promises to be epistemically beneficial. Thus, if we require the publishing of all P_1 results, thereby modifying the standard of risk, we can expect more empirical success in the actual detection of harms without decreasing the trials' capacity to detect benefits. In line with the temperature analogy above, the performance of the current standard can be epistemically assessed based on its comparative empirical success. Of course, there is a crucial difference between the two examples. In the case of thermometry, physicists were able to conduct a comparative evaluation of alternative conventions based on their actual empirical success, whereas epistemic criticism of the current RCT system is based on counterfactual reasoning. Both, however, indicate that strongly conventional methodological decisions can, be subjected to empirical criticism, at a later stage. Such criticism does not consist of a weighing of inductive risks, but of an analysis of the empirical performance of the operative methodological conventions that set the respective standards of risk.

Given these examples as well as Lewis's distinction between conventionality and conventional*ism*, my worry about the unwarranted implications of Wilholt's proposal can be stated more succinctly. In scientific practice, conventionally justified methodological choices often qualify for empirical criticism (or justification) at a later stage of research. If scientists can limit conventionality by extending the scope of empirical scrutiny, they *ought* to do so. While Wilholt's notion of bias as preference-based infringements on methodological conventions offers an important epistemological criticism, caution is needed before generalizing it into an "-ism" of any sort. Standards of inductive risks that are determined by conventional methodologies are epistemically necessary and should be defended against preference-based infringements. However, pace Wilholt, coordination offers merely a *preliminary* form of justification, which should be substituted by empirical arguments wherever possible. In theory, such arguments may consist of counterfactual criticism (as offered by Stegenga in the RCT case), or even become actualized in comparisons of empirical performance (as in the thermometry example).

[3]This is, of course, at best an oversimplification of the complicated history of thermometry, which merely serves to illustrate, not to substantiate, Mach's argument.

[4]This framing in terms of equilibria stems from Lewis, (1969).

TOWARD A PERMISSIVE EMPIRICISM

I have argued that methodological conventionalism cannot adequately address the problem of flawed methodological standards and even runs the risk of providing them with a justification in terms of their conventional utility. I have traced the cause of that problem to an insufficient analysis of scientific conventionality. While scientists avoid inductive risk decisions by following methodologies that are not justified empirically but in terms of their coordinative function, such conventional justifications are merely preliminary. In scientific practice, conventional methodological choices can be subjected to posterior criticism by comparing their respective empirical performance. In such processes, conventional justification is substituted by empirical justification. By stressing that such substitutions are warranted, we can avoid the problem of flawed conventions.

Given these qualifications, we can preserve Wilholt's insights in a straight-forward and, I hope, uncontroversial empiricist position. All the standard empiricist has to concede is that standards of inductive risks can be provisionally justified in virtue of their coordinative function. Such a *proviso* accounts for practical constraints that are an ineliminable element of scientific practice, like a shortage of information about a new domain of inquiry or a lack of financial or instrumental resources. As Wilholt has shown convincingly, socio-epistemic coordination warrants *some* standard. However, conventional justifications should, in principle, be regarded as merely *preliminary*. As such, they ought to be subjected to posterior empirical criticism. To illustrate this in another case, take Wilholt's own example of toxicological research into the health risks of exposure to bisphenol A. The selection of model organisms for testing bisphenol A, of course, comes with non-epistemic risks due to the potential health hazards that missing legal restrictions would cause to humans. A conventionalist would now object (epistemically) to the preference-based choice of a specific rat strain outside of a conventional class of model organisms. However, we can also make a straight-forward empirical argument about the comparative suitability of the different rat strains available. If the toxicity of bisphenol A is linked to its similarity to estrogen, an estrogen-insensitive rat strain will have smaller relative empirical success in detecting the potential harms of exposure to humans. Thus, not only did industry research infringe on the toxicological conventions, but it negatively impacted their empirical performance. While the former criticism identifies epistemically detrimental effects on the *collective* coordination of inquiry, the latter would show how one *particular* (potential) standard is empirically ineffective. I concede that, in this case, the empirical criticism does not target a methodological convention. I hope, however, that it sufficiently illustrates the relevant difference. Afterall, the same argument applies if the *conventional* class of model organisms (instead of the organism used in a *particular* experiment) would be composed of estrogen-insensitive rats.

In the debates on value-freedom and inductive risk, such a permissive variant of empiricism offers a compromise between methodological conventionalism and the VFI. In line with the former, it asserts that most inductive risk judgements are not made by individual scientists, but are settled implicitly by conventionally justified standards. Thus, conventional justification may only be accepted if ignorance or financial and experimental constrains keep us from testing the relative empirical performance of different standards of inductive risk.[5] In line with defenders of the VFI, however, permissive empiricism maintains that an effective elimination of non-epistemic risk decisions is generally warranted. This normative aim, however, is not achieved through specific forms of communication, but by means of posterior empirical criticism of conventional standards.

If the reader has followed my arguments this far, she might still find my alternative somewhat less elegant than either the 1) VFI or 2) methodological conventionalism. I concede that both offer a simpler response to the problem of inductive risk and, moreover, a single accompanying strategy to identify epistemically detrimental bias. I hope to have shown, however, that both run into serious difficulties, as they are either 1) practically inoperable or 2) implicitly allow for the justification of flawed standards. Beyond these negative arguments, moreover, the inconvenience introduced by a permissive empiricism is smaller than it might appear on first sight. While it permits not one but two kinds of justifications for standards of inductive risk (conventional and empirical), neither of them is grounded in any particularly uncomfortable or even novel epistemological claim. They simply pay tribute to the fact that scientific inquiry has a social *and* an empirical dimension. As such, research needs both 1) effective organization (i.e., coordination qua conventions) and 2) sensibility to the behavior of its corresponding target-systems (i.e., empirical utility). It should not be too controversial to regard arguments pertaining to 2) as the stronger form of justification or criticism. After all, corresponding to these two dimensions, we found industry influence to have two forms of epistemically detrimental consequences. Financial incentives infringe on the necessary conventions of a field or perpetuate empirically ineffective conventions in that field. The former epistemic danger is captured by industry bias in Wilholt's sense, while the latter might be dubbed *structural industry bias*, as it is the result of structurally flawed standards.

CONCLUSION

I have offered a criticism of methodological conventionalism. Wilholt's proposal is an important intervention in both the debates on value-freedom and industry bias. Not only does it highlight the neglected role of social conventions in handling inductive risk, but its definition of bias as preference-based infringements of conventional standards licenses criticisms of a crucial epistemic danger in industry funded science. However, I have argued that it suffers from a theoretical and a resulting practical weakness. *Theoretically*, it offers an underdeveloped

[5]Such as the interpretation of autopsy slides of laboratory rats in dioxin cancer studies discussed in Douglas (2000), 569–570.

analysis of scientific conventions, which fails to highlight how scientists are able to eliminate conventionality through posterior empirical criticism. Pure coordination problems that are confronted by settling a conventional equilibrium often become solvable on the basis of empirical evidence at a later stage of research. In such processes, a weaker type of justification and criticism is substituted by a stronger alternative. *Practically*, the neglect of this facet of conventionality makes methodological conventionalism unsuited to deal with what I dubbed the problem of flawed conventions. Some of the epistemically most detrimental consequences of industry preferences are not infringements on conventions, but the institution or perpetuation of empirically ineffective conventions.

In a constructive departure from methodological conventionalism, I tried to offer an account that preserves its insights while including a more qualified notion of conventionality. *Permissive empiricism*, as I dubbed it, is the following two-partite view on the justification of standards of inductive risk. Methodological choices that determine certain balances of inductive risk can be provisionally justified in virtue of their conventional function in setting coordinative equilibria. Such justifications, however, are merely preliminary, as they ought to be substituted by empirical arguments. Thus, if not blocked by financial, experimental, or other constrains, conventions should be evaluated based on their comparative empirical success.

Corresponding to the two kinds of epistemic justification invoked above, my empiricist framework licenses two kinds of criticisms of industry bias. The first has been exhaustively characterized by Wilholt and targets preference-based infringement on conventional standards of inductive risk. I proposed *structural industry bias* as a name for the additional type of bias I introduced. Structural industry bias occurs if industry influence perpetuates or institutes conventional standards that are empirically ineffective. Identifying and criticizing this second kind of bias is crucial for any evaluation of the dangers of industry funded science. As Stegenga's case against the current RCT-system illustrates, such criticism is not an idle epistemological worry, but has direct relevance for the epistemic integrity of scientific research. Attention to structural industry bias, is thus of central importance for the successful regulation of industry funded research.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/Supplementary Material, further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

FUNDING

This work was supported by the German Academic Scholarship Foundation (award number: 2345990) and the German Academic Exchange Service (award number 57437986).

ACKNOWLEDGMENTS

I am indebted to Jacob Stegenga and Ahmad Elabbar for concise and insightful criticisms of the ideas that eventually culminated in this paper. It was only through discussions with Ahmad that I first became interested in the problems discussed above. I am also thankful for the comments that Jon Leefmann and three referees provided on earlier drafts of this article, as well as for Florence Kipps's stylistic advice.

REFERENCES

Barnes, D. E., and Bero, L. A. (1996). Industry-funded research and conflict of interest: an analysis of research sponsored by the tobacco industry through the center for indoor air research. *J. Health Polit. Policy Law* 21 (3), 515–542. doi:10.1215/03616878-21-3-515

Barnes, R. L., Hammond, S. K., and Glantz, S. A. (2006). The tobacco industry's role in the 16 cities study of secondhand tobacco smoke: do the data support the stated conclusions? *Environ. Health Perspect.* 114 (12), 1890–1897. doi:10.1289/ehp.9385

Betz, G. (2013). In defence of the value free ideal. *Euro Jnl Phil Sci* 3 (2), 207–220. doi:10.1007/s13194-012-0062-x

Biddle, J. B. (2016). Inductive risk, epistemic risk, and overdiagnosis of disease. *Perspect. Sci.* 24 (2), 192–205. doi:10.1162/POSC_a_00200

Biddle, J. B., and Leuschner, A. (2015). Climate skepticism and the manufacture of doubt: can dissent in science be epistemically detrimental? *Euro Jnl Phil Sci* 5 (3), 261–278. doi:10.1007/s13194-014-0101-x

Brown, M. J. (2013). Values in science beyond underdetermination and inductive risk. *Philos. Sci.* 80 (5), 829–839. doi:10.1086/673720

Chang, H. (2004). *Inventing temperature. measurement and scientific progress*. New York, NY: Oxford Univ. Press, 304.

Chang, H. (2014). *Is water H_2O? evidence, realism and pluralism*. Dordrecht, NL, Heidelberg, NY, London, United Kingdom: Springer (Boston studies in the philosophy of science), vol. 293, 316.

Chiellini, F., Ferri, M., Morelli, Andrea., Dipaola, L., and Latini, G. (2013). Perspectives on alternatives to phthalate plasticized poly(vinyl chloride) in medical devices applications. *Prog. Polym. Sci.* 38 (7), 1067–1088. doi:10.1016/j.progpolymsci.2013.03.001

Churchman, C. W. (1948). *Theory of experimental inference*. New York, NY: Macmillan.

Davidson, R. A. (1986). Source of funding and outcome of clinical trials. *J. Gen. Intern. Med.* 1 (3), 155–158. doi:10.1007/BF02602327

Decullier, E., Chan, A. W., and Chapuis, F. (2009). Inadequate dissemination of phase I trials: a retrospective cohort study. *PLOS Med.* 6 (2), e1000034. doi:10.1371/journal.pmed.1000034

Douglas, H. E. (2009). *Science, policy, and the value-free ideal*. Pittsburgh, PA: University of Pittsburgh Press, 256.

Douglas, H. (2000). Inductive risk and values in science. *Philos. Sci.* 67 (4), 559–579. doi:10.1086/392855

Eurostats (2019). *Global domestic expenditure on research and development funding 2006-2018*: R&D expenditure by source of funds. https://ec.europa.eu/eurostat/statistics-explained/index.php?title=R_%26_D_expenditure&oldid=28319#R_.26_D_expenditure_by_source_of_funds (Accesssed December 3, 2020).

Feyerabend, P. K. (2002). *Against method*. 3rd Edn. London, United Kingdom: Verso.
Frisch, M. (Forthcoming 2020). *Uncertainties, values, and climate targets*. Philosophy of Science.
Hempel, C. G. (1965). "Science and human values," in *Aspects of scientific explanation and other essays in the philosophy of science*. Editor C. G. Hempel (New York, NY: Free Press), 81–96.
Holman, B., and Bruner, J. P.. (2015). The problem of intransigently biased agents. *Philos. Sci.* 82 (5), 956–968. doi:10.1086/683344
Holman, B., and Elliott, K. C. (2018). The promise and perils of industry-funded science. *Philos. Compass* 13 (11), 11–14. doi:10.1111/phc3.12544
John, S. (2015). The example of the IPCC does not vindicate the Value Free Ideal: a reply to Gregor Betz. *Euro Jnl Phil Sci* 5 (1), 1–13. doi:10.1007/s13194-014-0095-4
Kuhn, T. S. (1977). "Objectivity, value judgement, and theory choice," in *The essential tension: selected studies in scientific tradition and change*. Editor T. S. Kuhn (Chicago, Ill: Univ. of Chicago Press), 320–339.
Lakatos, I. (1999). *The methodology of scientific research programmes*. Cambridge, United Kingdom: Cambridge University Press.
Levi, I. (1962). On the seriousness of mistakes. *Philos. Sci.* 29 (1), 47–65. doi:10.1086/287841
Lewis, D. K. (1969). *Conventions: a philosophical study*. Cambridge, Mass: Harvard University Press.
Lo, B., and Field, M. J. (2009). *Institute of medicine committee on conflict of interest in medical research, education, and practice summary*. Washington, DC: National Academies Press (US).
Longino, H. E. (1996). "Cognitive and non-cognitive values in science: rethinking the dichotomy," in *Feminism, science, and the philosophy of science*. (Editors) L. H. Nelson and J. Nelson (Dordrecht, NL: Springer), 256, 39–58.
Lundh, A., Lexchin, J., Mintzes, B., Schroll, J. B., and Bero, L. (2017) Industry sponsorship and research outcome. *Cochrane Database Syst. Rev.* 2, MR000033. doi:10.1002/14651858.MR000033.pub3
Mach, E., and McGuinness, B. (1986). *Principles of the theory of heat. Historically and critically elucidated*. Dordrecht, NL: Springer.
Mastrandrea, M. D., Mach, K. J., Plattner, G-K., Edenhofer, O., Stocker, T. F., Field, C. B., et al. (2011). The IPCC AR5 guidance note on consistent treatment of uncertainties: a common approach across the working groups. *Clim. Change* 108 (4), 675–691. doi:10.1007/s10584-011-0178-6
Moynihan, R., Bero, L., Hill, S., Johansson, M., Lexchin, J., Macdonald, H., et al. (2019). Pathways to independence: towards producing and using trustworthy evidence. *BMJ* 367, 6576. doi:10.1136/bmj.l6576
National Office of Statistics (2020). *Gross domestic expenditure on research and development: section 4*. https://www.ons.gov.uk/economy/governmentpublicsectorandtaxes/researchanddevelopmentexpenditure/bulletins/ukgrossdomesticexpenditureonresearchanddevelopment/2018 (Accesssed December 3, 2020).
O'Reilly, J., Oreskes, N., and Oppenheimer, M. (2012). The rapid disintegration of projections: the west antarctic ice sheet and the intergovernmental panel on climate change. *Soc. Stud. Sci.* 42 (5), 709–731. doi:10.1177/0306312712448130
Pritchard, D. (2005). *Epistemic luck*. Oxford, NY: Oxford University Press, 306.
Resnik, D. B. (2000). Financial interests and research bias. *Perspect.Sci.* 8 (3), 255–285. doi:10.1162/106361400750340497
Rudner, R. (1953). The scientist qua scientist makes value judgements. *Philos. Sci.* 20 (1), 1–6. doi:10.1086/287231
Sass, J. B., Castleman, B., and Wallinga, D. (2005). Vinyl chloride: a case study of data suppression and misrepresentation. *Environ. Health Perspect.* 113 (7), 809–812. doi:10.1289/ehp.7716
Schott, G., Pachl, H., Limbach, U., Gundert-Remy, U., Ludwig, W. D., and Lieb, K. (2010). The financing of drug trials by pharmaceutical companies and its consequences. Part 1: a qualitative, systematic review of the literature on possible influences on the findings, protocols, and quality of drug trials. *Dtsch Arztebl Int.* 107 (16), 279–285. doi:10.3238/arztebl.2010.0279
Solomon, M. (2020). "After disclosure," in *Uncertainty in Pharmacology: epistemology, methods and decisions. [S.l.]*. Editors L. C. Adam and B. Osimani (Dordrecht, Netherlands: Springer), 179–190.
Steel, D. (2016). Climate change and second-order uncertainty: defending a generalized, normative, and structural argument from inductive risk. *Perspect. Sci.* 24 (6), 696–721. doi:10.1162/POSC_a_00229
Steele, K. (2012). The scientist qua policy advisor makes value judgments. *Philos. Sci.* 79 (5), 893–904. doi:10.1086/667842
Stegenga, J. (2018). *Medical nihilism*. Oxford, NY: Oxford University Press, 240.
Volz, D. C., and Elliott, K. C. (2012). Mitigating conflicts of interest in chemical safety testing. *Environ. Sci. Technol.* 46 (15), 7937–7938. doi:10.1021/es3028296
Vom Saal, F. S., and Hughes, C. (2005). An extensive new literature concerning low-dose effects of bisphenol A shows the need for a new risk assessment. *Environ. Health Perspect.* 113 (8), 926–933. doi:10.1289/ehp.7713
Wald, A. (1942). *On the principles of statistical inference*. Notre Dame, Indiana: University of Notre Dame, 50.
Wilholt, T. (2009). Bias and values in scientific research. *Stud. Hist. Philoso. Sci. A* 40 (1), 92–101. doi:10.1016/j.shpsa.2008.12.005
Wilholt, T. (2016). "Collaborative research, scientific communities, and the social diffusion of trustworthiness," in *The epistemic life of groups*. Editors M. S. Brady and M. Fricker (Oxford, NY: Oxford University Press), 218–234.
Wilholt, T. (2013). Epistemic trust in science. *Br. J. Philos. Sci.* 64 (2), 233–253. doi:10.1093/bjps/axs007

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

PERSPECTIVE
published: 08 March 2021
doi: 10.3389/frma.2021.614013

Epistemic Corruption, the Pharmaceutical Industry, and the Body of Medical Science

Sergio Sismondo *

Department of Philosophy, Queen's University, Kingston, ON, Canada

When a knowledge system importantly loses integrity, ceasing to provide the kinds of trusted knowledge expected of it, we can label this *epistemic corruption*. Epistemic corruption often occurs because the system has been co-opted for interests at odds with some of the central goals thought to lie behind it. There is now abundant evidence that the involvement of pharmaceutical companies corrupts medical science. Within the medical community, this is generally assumed to be the result of conflicts of interest. However, some important ways that the industry corrupts are not captured well by standard analyses in terms of conflicts of interest. It is not just that there is a body of medical science perverted by industry largesse. Instead, much of the corruption of medical science via the pharmaceutical industry happens through grafting activities: Pharmaceutical companies do their own research and smoothly integrate it with medical science, taking advantage of the legitimacy of the latter.

Keywords: bias, medical research, pharmaceutical industry, epistemic corruption, conflict of interest

OPEN ACCESS

Edited by:
Sebastian Schuol, University of Erlangen Nuremberg, Germany

Reviewed by:
Heiner Hastedt, University of Rostock, Germany
Wendy Lipworth, The University of Sydney, Darlington, NS, Australia
Donald W. Light, Rowan University School of Osteopathic Medicine, Stratford, NJ, United States

***Correspondence:**
Sergio Sismondo
sismondo@queensu.ca

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *04 October 2020*
Accepted: *25 January 2021*
Published: *08 March 2021*

Citation:
Sismondo S (2021) Epistemic Corruption, the Pharmaceutical Industry, and the Body of Medical Science. Front. Res. Metr. Anal. 6:614013. doi: 10.3389/frma.2021.614013

INTRODUCTION: EPISTEMIC CORRUPTION

"Corrupt" and its cognates are old terms with many metaphorical uses. Bodies, fruits and meats are corrupted when they begin to rot, decompose, or otherwise spoil. What is thought pure is corrupted when mixed with something foul or lesser, as when air is made foul by pestilence or smoke, noble lineages are supposedly lessened by poor marriages, or people become less good simply because of the pressures of society. "Each of us is born with a share of purity, predestined to be corrupted by our commerce with mankind, by that sin against solitude" (Cioran 2012 [1949]).

It is only a small step from the introduction of pollution to the perversion of ends, as when a public official is corrupted by money or power for a purpose, to serve some interests rather than others. This is the most familiar kind of corruption today—so common that the metaphor has largely died—in which corrupted office holders and institutions have been captured by outside interests, or perhaps serve only their own interests. Thus there is a United Nations Convention against Corruption, which never needs to explicitly define "corruption," though it identifies it as involving a constellation of crimes that include bribery, embezzlement, influence peddling, illicit enrichment, etc., (United Nations 2004).

There can be value in analyzing knowledge systems in terms of all the above and other senses of the metaphor. When a knowledge system importantly loses integrity, ceasing to provide the kinds of trusted knowledge expected of it, or even in some cases when it ceases to establish trust, we can label this *epistemic corruption*. For example, the weaknesses of mathematical models can become entrenched, especially if they are constantly adjusted through curve-fitting, as has been claimed about several epidemiological models of the spread of Covid-19 (e.g., Jewell et al., 2020). Or,

environmental toxicology may systematically lack information about the risks of a large number of industrial and agricultural chemicals, because powerful entities can control private science (e.g., on fluorinated compounds see Richter et al., 2018) and shape public science (e.g., on glyphosate, see Thacker 2019). And, outside the sciences, although many accusations of "fake news" are wide of the mark, large swaths of both social and traditional media are genuinely untrustworthy, whether because of interests that shape the creation or the dissemination of news, or because of inherent weaknesses of systems designed to capture audiences' attention.

My focus here is on how the pharmaceutical industry corrupts medical science. Using its very substantial resources, pharmaceutical companies co-opt medical knowledge systems for their particular interests, interests that conflict with the integrity and at least some of the central goals thought to lie behind medicine. It would seem that the body of medical science is corrupted because some assumed purity—though purity is always notional—has been affected by contact with outside interests.

PHARMACEUTICAL INDUSTRY AFFECTS MEDICAL RESEARCH

For the past 25 years, researchers have been studying the effects of industry funding—most often from pharmaceutical companies—on medical science. One typical protocol compares outcomes in industry-funded and other clinical trials in some therapeutic area, or for some class of drugs or medical devices, working either from searches of the published literature or from some other sample, such as conference abstracts. Most reports of clinical trials declare sources of funding, so analysts can often cleanly divide publications and make comparisons. In addition, clinical trials within areas often have enough uniformity that at meta-analyses can sometimes be done. Since the mid-1990s, there have been hundreds of published studies of industry influence, comparing many thousands of clinical trials across all domains of medicine. The researchers designing and following these protocols often frame them as analogous to medical studies, with industry funding being the intervention, and the integrity and stability of the body of medical research being the outcome.

A 2017 Cochrane Review (Lundh et al., 2017, updated from; Lundh et al., 2012) provides a meta-analysis of such studies of industry funding, in which 75 studies, comparing more than 8,000 trials, met inclusion criteria. In all of its dimensions, the 2017 meta-analysis arrives at the same or similar results as had earlier quantitative and qualitative reviews (Bekelman et al., 2003; Lexchin et al., 2003; Schott et al., 2010). In the meta-analysis, industry funding had a risk ratio of 1.27 (95% CI: 1.17–1.51) of producing favorable efficacy results, and of 1.34 (95% CI: 1.19–1.51) of drawing favorable overall conclusions (in this study the harm results were not statistically different between industry and non-industry funding). Since there is no reason to think that non-industry funding skews results in any consistent direction, one can only conclude that industry funding biases the outcomes of clinical trials. Put simply, if a pharmaceutical company funds a trial, the chances of results and conclusions in that company's favor are increased. However, in this study, industry and non-industry research did not differ on such standard methodological quality concerns as sequence generation, allocation concealment, follow-up, or selective outcome reporting; and industry sponsored studies even had better blinding procedures.

The authors of the Cochrane Review conclude: "Our analyses suggest the existence of an industry bias that cannot be explained by standard 'Risk of bias' assessments" (Lundh et al., 2017). When pharmaceutical and other companies sponsor research there is a bias—a systematic tendency toward results serving their interests—but the bias is not seen in the formal factors routinely associated with low-quality science. The implication is that industry funding itself should be considered a standard "risk of bias" factor in clinical trials, one that is quantifiable, and even quantified, and pushes in predictable directions. Industry funding affects the results of clinical trials.

BUT FUNDING IS RARELY JUST FUNDING

The Cochrane Review I have just described shows that the pharmaceutical industry corruption of medical science doesn't happen through the mechanisms currently assessed by typical formal methodological measures. Funding itself corrupts medical science. But this does not mean that it is mysterious.

The most common way of understanding corruption through funding is in terms of conflict of interest. Perhaps funding and payments to researchers create conflicts of interest, which—for conscious or unconscious reasons—affect their actions, their judgments, and their conclusions. As a result, these conflicted researchers become more likely to report outcomes friendly to their funders. However, something else is at play here as well, and it is this that I want to illustrate below.

There is abundant evidence that conflicts of interest are important in many domains, including across medicine. For example, financial conflicts on committees producing clinical practice guidelines tend to produce assessments of evidence and recommendations that favor the companies and industries involved (Cosgrove et al., 2013; Lexchin 2020). In terms of medical practice, a recent systematic review shows that payments to physicians influence prescribing (Mitchell et al., 2020). The broad issue of conflict of interest is important enough that the United States Institute of Medicine issued a detailed report on it, overwhelmingly about how financial conflicts involving industry affect researchers' and physicians' judgment (Institute of Medicine, 2009). Despite such evidence, a focus on conflict of interest hides how pharmaceutical companies influence published results and outcomes.

Funding is rarely just funding. Most pharmaceutical company-sponsored clinical trials are designed, organized, audited, analyzed, and written up by the companies and their hired subcontractors. This is all work that happens behind the scenes, obscured by the form of academic publication. Thus much

of the corruption can happen through more substantive medical choices and through structures of influence and control, as I describe below.

Roughly 70–75% of the industry's expenditures on clinical trials go to contract research organizations (CROs), rather than to independent researchers in the form of grants (Mirowski and Van Horn 2005; Fisher 2008; Westrock 2016). CROs together have revenue estimated to be approximately US$50 billion in 2020, most of it coming from pharmaceutical industry clinical trials (Fortune Business Insights, 2019). As a result, in the comparison of "industry-sponsored" and independent research, in most cases the "sponsorship" involves direct control over the research.

Even when it appears that industry-sponsored trials are led by academic or other actors, and that their subjects are recruited via independent clinics, hospitals and academic medical centers, it is most likely that at a higher level they are run by CROs working for pharmaceutical companies, and analyzed by company statisticians and others. Manuscripts are most likely drafted by ghostwriters on structures created by publication planners, and then shepherded through to publication by those planners, with limited opportunities for their academic and other independent authors to contribute (Fugh-Berman and Dodgson, 2008; Sismondo 2009; Matheson 2016). The published articles, then, are largely creations of the companies, even if the nominal authors include independent researchers. All of this constitutes the "ghost-management" of medical research (Sismondo 2018).

The ghost-management of trials affords many opportunities to intervene on individual publications and to affect the published record, producing the effects of industry sponsorship I described above. I list some significant categories, for each of which I provide an example or evidence.

(a) Companies can design studies that are likely to produce favorable results, making careful choices of comparators, doses, experimental populations, surrogate endpoints, trial durations, and definitions. For example, in Merck's testing of its COX-2 inhibitor rofecoxib, it used most of these techniques to improve one or another of its published trials (Whitstock 2018).
(b) Given the ghost-management of industry-funded research, funding almost certainly affects the interpretation of data and the writing of articles. Internal company documents and presentations show that the companies are fully aware of the opportunities for spin (e.g., Moffatt and Elliott 2007; McHenry 2010).
(c) Sometimes the corruption goes so far as to count as scientific misconduct, such as direct manipulation of data, omission of adverse events, etc. On the basis of documents from litigation against Forest Laboratories for misleading marketing of citalopram, Jureidini et al. (2016) establish conclusively that the ghost-management of the research allowed company employees to publish efficacy and safety conclusions that were inconsistent with what the trial data could support.
(d) Industry trials with positive results are over-represented in the medical journals, and those with negative results are under-represented, resulting in significant publication biases. In antidepressant trials submitted to regulatory agencies such as the United States Food and Drug Administration (Turner et al., 2008) or the Swedish regulatory agency (Melander et al., 2003)—and thus all industry trials—positive results are much more likely to be published. The positive trials are often multiply published by lumping and splitting, than are those with negative results. This has produced an impression in the medical literature that the evidence for the effectiveness of antidepressants is much stronger than it actually is.
(e) Industry trials are more cited than are non-industry trials (Gorry 2015). This may be because when publication planners assign a manuscript to a ghostwriter, it appears that a list of references is frequently one of the key inputs, and companies have good marketing reasons to cite themselves (Sismondo 2020). However, the higher level of citation may be simply a result of the fact that pharmaceutical companies have much better resources for promoting their own trials than individual researchers have. For example, the companies employ thousands upon thousands of "key opinion leaders" to give talks to physicians, using prepared slide shows, on recent clinical research (Moynihan 2008; Sismondo 2018).

The pharmaceutical industry corrupts medical science and the medical literature through these mechanisms and many more (Sismondo 2018). In the ghost-management of research, much of the corruption does not happen via traditionally conceived conflicts of interest of independent medical researchers. Instead, it happens by more direct actions by drug companies and their agents, such as those listed in (a) to (e) above.

DISCUSSION: THE BODY OF MEDICAL SCIENCE

While it initially seems likely that medical science is corrupted by medical researchers' conflicts of interest, that picture doesn't capture at least some of what is going on. Instead, pharmaceutical companies create their own research and its own ways of disseminating that research, relying on structures and traditions of medical science to legitimate their work. While we could talk of companies as having conflicts of interest, it is more natural to talk of them as acting in their own interests.

In the pharmaceutical companies' ghost-management of research, much of the corruption of medical science happens through a process of grafting. Grafts on plants make two bodies into one, typically allowing a fruiting part of a plant of value—to the horticulturist—to thrive by drawing on nutrients provided by a different plant's rootstock. Grafting involves a carefully constructed parasitic relationship. Similarly, pharmaceutical companies add substantially to medical science, doing their own research, smoothly attaching it to medical science in a way that integrates it, and then nurturing it to make it predominate. Non-industry medical science provides legitimacy to the apparently similar additions. The effects of industry sponsorship of medical research are the results of

prominent additions to the body of medical science, not the simple introduction of an element—such as funding—that infects what it touches.

Of course, the pharmaceutical industry is a huge one, and in some areas of medicine the grafts permeate or overwhelm everything else in the area. And it is likely that the grafts affect the bodies onto which they are grafted: industry science may, for example, create costly research norms that in turn create demand for more industry funding.

Like most systems that can be corrupted, medical science has never been pure or perfect. But the pharmaceutical industry can trade on the presumed innocence of medical research's overriding goal: creating knowledge to benefit patient health. That is, some standard narratives of medical research attribute to it purity of heart, and a mere shortage of means that can be rectified by industry support.

In a very different context, Kierkegaard (1995: 76) writes: "As the world changes, the forms of corruption also gradually become more cunning, more difficult to point out." In its corruption of medical science, the pharmaceutical industry has borne this out.

DATA AVAILABILITY STATEMENT

Further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

REFERENCES

Bekelman, J. E., Li, Y., and Gross, C. P. (2003). Scope and impact of financial conflicts of interest in biomedical research: a systematic review. *JAMA* 289 (4), 454–465. doi:10.1001/jama.289.4.454

Institute of Medicine (2009). in *Conflict of interest in medical research, education, and practice*. Editors B. Lo and M. J. Field (Washington, DC: National Academies Press.). Available at: https://www.ncbi.nlm.nih.gov/books/NBK22942/pdf/Bookshelf_NBK22942.pdf (Accessed October 2, 2020).

Cioran, E. (2012). *[1949] A short history of decay*. Editor R. Howard (New York, NY: Arcade Publishing), 200.

Cosgrove, L., Bursztajn, H., Erlich, D., Wheeler, E. E., and Shaughnessy, A. F. (2013). Conflicts of interest and the quality of recommendations in clinical guidelines. *J. Eval. Clin. Pract.* 19, 674–681. doi:10.1111/jep.12016

Fisher, J. A. (2008). *Medical research for hire: the political economy of pharmaceutical clinical trials*. New Brunswick, NJ: Rutgers University Press, 272.

Fortune Business Insights (2019). Contract research organization (CRO) services market. Available at: https://www.fortunebusinessinsights.com/industry-reports/contract-research-organization-cro-services-market-100864 (Accessed October 2, 2020).

Fugh Berman, A., and Dodgson, S. J. (2008). Ethical considerations of publication planning in the pharmaceutical industry. *Open Med.* 2 (4), e121–e124.

Gorry, P. (2015). Medical literature imprinting by Pharma ghost writing: a scientometric evaluation. *Proceedings of ISSI 2015 Istanbul:* 650–651. Available at: https://www.issi-society.org/proceedings/issi_2015/0650.pdf (Accessed February 23, 2021).

Jewell, N. P., Lewnard, J. A., and Jewell, B. L. (2020). Caution warranted: using the Institute for health Metrics and evaluation model for predicting the course of the COVID-19 pandemic. *Ann. Intern. Med.* 173, 226–227. doi:10.7326/M20-1565

Jureidini, J. N., Amsterdam, J. D., and McHenry, L. B. (2016). The citalopram CIT-MD-18 pediatric depression trial: deconstruction of medical ghostwriting, data mischaracterisation and academic malfeasance. *Int. J. Risk Saf. Med.* 28 (1), 33–43. doi:10.3233/JRS-160671

Kierkegaard, S. (1995). *Works of love: Kierkegaard's writings*. Editors H. V. Hong and E. H. Hong (Princeton, NJ: Princeton University Press), Vol. 16.

Lexchin, J., Bero, L. A., Djulbegovic, B., and Clark, O. (2003). Pharmaceutical industry sponsorship and research outcome and quality: systematic review. *BMJ* 326 (7400), 1167–1170. doi:10.1136/bmj.326.7400.1167

Lexchin, J. (2020). Clinical practice guidelines and managing financial conflicts of interest. *eLS* 1 (2), 263–269. doi:10.1002/9780470015902.a0029129

Lundh, A., Lexchin, J., Mintzes, B., Schroll, J. B., and Bero, L. (2017). Industry sponsorship and research outcome. *Cochrane Database Syst. Rev.* 2, MR000033. doi:10.1002/14651858.MR000033.pub3

Lundh, A., Sismondo, S., Lexchin, J., Busuioc, O. A., and Bero, L. (2012). Industry sponsorship and research outcome. *Cochrane Database Syst. Rev.* 12, MR000033. doi:10.1002/14651858.MR000033.pub2

Matheson, A. (2016). Ghostwriting: the importance of definition and its place in contemporary drug marketing. *BMJ* 354, i4578. doi:10.1136/bmj.i4578

McHenry, L. (2010). Of sophists and spin-doctors: industry-sponsored ghostwriting and the crisis of academic medicine. *Mens Sana Monogr.* 8 (1), 129–145. doi:10.4103/0973-1229.58824

Melander, H., Ahlqvist-Rastad, J., Meijer, G., and Beermann, B. (2003). Evidenced b(i)ased medicine—selective reporting from studies sponsored by pharmaceutical industry: review of studies in new drug applications. *BMJ* 326, 1171–1173. doi:10.1136/bmj.326.7400.1171

Mirowski, P., and Van Horn, R. (2005). The contract research organization and the commercialization of scientific research. *Soc. Stud. Sci.* 35 (4), 503–548. doi:10.1177/0306312705052103

Mitchell, A. P., Trivedi, N. U., and Gennarelli, R. L. (2020). Are financial payments from the pharmaceutical industry associated with physician prescribing? a systematic review. *Ann. Intern. Med.* [Epub ahead of print]. doi:10.7326/M20-5665

Moffatt, B., and Elliott, C. (2007). Ghost marketing: pharmaceutical companies and ghostwritten journal articles. *Perspect. Biol. Med.* 50, 18–31. doi:10.1353/pbm.2007.0009

Moynihan, R. (2008). Key opinion leaders: independent experts or drug representatives in disguise?. *BMJ* 336 (7658), 1402–1403. doi:10.1136/bmj.39575.675787.651

Richter, L., Cordner, A., and Brown, P. (2018). Non-stick science: sixty years of research and (in)action on fluorinated compounds. *Soc. Stud. Sci.* 48 (5), 691–714. doi:10.1177/0306312718799960

Schott, G., Pachl, H., Limbach, U., Gundert-Remy, U., Ludwig, W. D., and Lieb, K. (2010). The financing of drug trials by pharmaceutical companies and its consequences, part 1: a qualitative, systematic review of the literature on possible influences on the findings, protocols, and quality of drug trials. *Dtsch. Arztebl. Int.* 107 (16), 279. doi:10.3238/arztebl.2010.0279

Sismondo, S. (2018). *Ghost-managed medicine: big pharma's invisible hands*. Manchester, United Kingdom: Mattering Press, 234.

Sismondo, S. (2020). "Ghost-managing and gaming pharmaceutical knowledge," in *Gaming the metrics: misconduct and manipulation in academic research*. Editors M. Biagioli and A. Lippman (Cambridge, MA: MIT Press), 123–134.

Sismondo, S. (2009). Ghosts in the machine: publication planning in the medical sciences. *Soc. Stud. Sci.* 39 (2), 171–198. doi:10.1177/0306312708101047

Thacker, P. D. (2019). An inside look at how Monsanto, a PR firm and a reporter give readers a warped view of science. Available at: https://www.huffpost.com/entry/monsanto-public-relations-reporter-warped-science-view_n_5d485c14e4b0ca604e3636b5?11&fbclid=IwAR3nyp4iCmHdnEYhBpyiLvAfqkreTkCGSlqEqJKEazLXPbWCPyFXXl2d3nU (Accessed August 13, 2019).

Turner, E. H., Matthews, A. M., Linardatos, E., Tell, R. A., and Rosenthal, R. (2008). Selective publication of antidepressant trials and its influence on apparent efficacy. *NEJM* 358, 252–260. doi:10.1056/NEJMsa065779

United Nations (2004). *United Nation convention against corruption*. New York, NY: United Nations. Available at: https://www.unodc.org/unodc/en/corruption/tools_and_publications/UN-convention-against-corruption.html (Accessed October 2, 2020).

Westrock (2016). Patient-centric clinical trial solutions: adherence, outcomes and data. Available at: https://pages.questexweb.com/WestRock-20160524-Registration.html (Accessed May 15, 2016).

Whitstock, M. (2018). Manufacturing the truth: from designing clinical trials to publishing trial data. *Indian J. Med. Ethics* 3, 152–153. doi:10.20529/IJME.2017.096

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

ORIGINAL RESEARCH
published: 05 May 2021
doi: 10.3389/frma.2021.599909

How to Assess the Epistemic Wrongness of Sponsorship Bias? The Case of Manufactured Certainty

*Jon Leefmann**

Center for Applied Philosophy of Science and Key Qualifications (ZiWiS), Friedrich-Alexander-Universität Erlangen-Nürnberg, Erlangen, Germany

Although the impact of so-called "sponsorship bias" has been the subject of increased attention in the philosophy of science, what exactly constitutes its *epistemic* wrongness is still debated. In this paper, I will argue that neither evidential accounts nor social–epistemological accounts can fully account for the epistemic wrongness of sponsorship bias, but there are good reasons to prefer social–epistemological to evidential accounts. I will defend this claim by examining how both accounts deal with a paradigm case from medical epistemology, recently discussed in a paper by Bennett Holman. I will argue that evidential accounts cannot adequately capture cases of sponsorship bias that involve the manufacturing of certainty because of their neutrality with respect to the role of non-epistemic values in scientific practice. If my argument holds, it further highlights the importance of integrating social and ethical concerns into epistemological analysis, especially in applied contexts. One can only properly grasp sponsorship bias as an epistemological problem if one resists the methodological tendency to analyze social, ethical, and epistemological issues in isolation from each other.

Keywords: sponsorship bias, manufactured certainty, epistemic wrongness, error, social epistemology, evidence, confirmation

OPEN ACCESS

Edited by:
Koen Jonkers,
European Commission, Luxembourg

Reviewed by:
Nicola Mößner,
RWTH Aachen University, Germany
Caroline S. Wagner,
The Ohio State University, United States

***Correspondence:**
Jon Leefmann
jon.leefmann@fau.de

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *28 August 2020*
Accepted: *01 March 2021*
Published: *05 May 2021*

Citation:
Leefmann J (2021) How to Assess the Epistemic Wrongness of Sponsorship Bias? The Case of Manufactured Certainty. Front. Res. Metr. Anal. 6:599909. doi: 10.3389/frma.2021.599909

SPONSORSHIP BIAS AS AN EPISTEMIC PHENOMENON

In recent years, sponsorship bias has been widely discussed in relation to bias in science (Holman and Bruner, 2015; Holman and Elliott, 2018). The term refers to the fact that research funded by industries or other commercial enterprises is more likely than publicly funded research to produce results in line with the funder's commercial interests (Lexchin et al., 2003; Sismondo, 2008; Lundh et al., 2017). Hence, it is also sometimes called preference bias (Wilholt, 2009). There is, however, disagreement about how to best explain this phenomenon. First, there is a debate about whether the phenomenon is primarily a form of bias (Wilholt, 2009; Holman and Bruner, 2017; Holman and Elliott, 2018; Robinson, 2019; Reutlinger, 2020b), *viz.*, an epistemic shortcoming, or whether it should instead be interpreted as an ethical or political problem (Melo-Martín, 2019). Second, as

we will see below, epistemic analyses of sponsorship bias differ over how to explain its epistemic wrongness.[1]

An epistemic analysis of sponsorship bias can be supported by noting the various mechanisms that enable incorrect conclusions to be drawn from scientific data. For example, the preference for one study design over another is known as design bias. Other examples concern forms of data-selection bias (favoring certain data when presenting research results), interpretation bias (favoring one interpretation and disregarding alternatives), and publication bias (only publishing results that confirm a preferred hypothesis while holding back or even suppressing results that do not).[2] These distorting mechanisms can function at the level of an individual researcher and of an entire scientific community. For example, an individual researcher can disregard certain data when drawing conclusions from an experiment, while a research group can follow rules and practices that promote flawed data analysis, false conclusions, and erroneous interpretations. These mechanisms always result in epistemic shortcomings, insofar as they cause researchers to adopt insufficiently supported or even false beliefs. Like any account of bias, an analysis of sponsorship bias must explain exactly what goes epistemically wrong in all such cases.

However, the role of preference in these biases shows that they are not merely epistemic. If sponsorship bias resulted from flawed reasoning and logical mistakes alone, it would be better described as an error, rather than a bias. But *preferring* one set of data over another, or *deciding* not to publish uncharitable research, are forms of practical reasoning. This suggests that sponsorship bias does not involve mere epistemic wrongness but, rather, such wrongness that is consciously or unconsciously motivated by practical interests or preferences. A full account of sponsorship bias must, therefore, also explain the role of practical interests in bringing about epistemic wrongness.[3]

The most obvious response to this challenge is to simply insist that the epistemic wrongness of biased research stems from the influence of political or financial interests on the research process. Science is thus imagined to be a purely epistemic endeavor[4], which is then tainted by concerns that compromise the pure pursuit of knowledge by motivating scientists to produce results that are socially acceptable, politically desirable, or supportive of social change.

Current mainstream philosophy of science would, however, not welcome this answer. The idea that science can be totally free of non-epistemic values has long been recognized as a philosophical ideal that cannot be realized in practice. To insist that only research wholly free of social, political, or practical values and interests is epistemically apt would be to repudiate the epistemic credentials of almost all actual science. In recent decades, various philosophers have argued that social, political, and practical values play a role in science, not only in relation to the choice of research agendas but also within the research process (Rudner, 1953; Longino, 1990; Douglas, 2000, 2007). The argument from inductive risk, for example, purports to show that scientists inevitably decide whether to accept or reject a hypothesis in light of evidence about the relative harmfulness of either endorsing a hypothesis that is, in fact, false, or rejecting one that is, in fact, true (Rudner, 1953; Hempel, 1965; Douglas, 2009). The harmfulness of making these mistakes cannot be evaluated without reference to practical, social, or political—i.e., non-epistemic—values.[5]

If we side with the current mainstream in the philosophy of science and accept that science is inherently value laden[6], we cannot account for the epistemic wrongness of biases (and of sponsorship bias, in particular) by simply pointing to the non-epistemic interests and preferences of those involved. Epistemically unimpeachable research would also be influenced by such values. One could, of course, point out that there is no problem with non-epistemic values as such, but only when certain such values are involved, such as purely commercial concerns to maximize financial returns on research. Even setting aside cases where the intrusion of such concerns into science does not cause epistemic problems (Carrier, 2008), this still raises the question on how to distinguish between acceptable and unacceptable non-epistemic values, and this distinction would have to be justified by reference to pragmatic or ethical rather than epistemic principles. It is logically impossible to justify the validity of *non-epistemic* values using *epistemic* criteria.[7]

[1]In focusing on sponsorship bias, I do not wish to insinuate that all interactions between public research and private sponsors are necessarily epistemically detrimental or ethically dubious. Private sponsorship can have advantages: In applied research, there are various examples of collaboration between publicly and privately funded researchers producing epistemically and socially valuable results. Moreover, private funding sometimes enables research that would not otherwise be possible due to a lack of public funding (Wilholt, 2006). Collaboration between industries and public research institutions can also sometimes accelerate and intensify research, as shown by the recent development of several vaccines against COVID-19 infection through the efforts of researchers in competing biotech companies and public universities. While these positive effects of private research funding are frequently mentioned in the literature (Adam et al., 2006; Carrier, 2008; Holman and Elliott, 2018), thorough philosophical and sociological investigation is needed to determine the exact conditions under which competition and collaboration between publicly and privately funded research have positive or negative epistemic effects.

[2]For an overview and various examples of these biases, see Wilholt (2009) and Holman and Elliott (2018).

[3]In this paper, I am only concerned with a form of bias that involves some kind of practical interest. In the literature, the term "bias" is sometimes used more broadly to also encompass cases of false reasoning and unjustified belief formation, irrespective of whether or not they originate in a conscious practical interest. In the current literature, "bias" is often conceived of as an implicit prejudice against a (social) object (Beeghly and Madva, 2020). Biased reasoning is thought to result from the influence of various social, cultural, or economic factors on human cognition. For examples of the variety of uses of the term, see Goldman (1999), Resnik (2000).

[4]I refer to science as value-free only with respect to non-epistemic, *viz.*, social, political, or moral values. I am not here interested in discussions of the role of epistemic or cognitive value judgments in theory choice. See, for instance, Kuhn (1977/2000) and Douglas (2013) on this point.

[5]The argument from inductive risk is just one example that shows that value-free science is, at best, an unreachable ideal. There are other arguments for this conclusion and in much of the sociology of science literature, the impossibility of value-free science is even taken for granted (Barnes et al., 1996). Adjudicating this complex issue is, however, far beyond the scope of this paper.

[6]Though endorsed by many, the claim that scientific practice necessarily involves non-epistemic value judgments is contested. There is, in fact, a particular debate about the argument from inductive risk (Betz, 2013, 2017; Melo-Martín, 2016).

[7]This is, however, not to say that one cannot justify the validity of epistemic and non-epistemic values by assessing their effects on epistemic practices. For instance,

Philosophers have offered various explanations of the highly plausible intuition that sponsorship bias is at least as much of an epistemic problem as an ethical one and of what exactly goes epistemically wrong in cases of sponsorship bias. Borrowing terminology from Reutlinger (2020a), one can divide these into "evidential accounts" (EAs) and "social epistemological accounts" (SEAs). In what follows, I will argue that SEAs are better suited than EAs to account for important features of sponsorship bias. I will defend this claim by discussing the two types of accounts through the lens of a paradigmatic example used in discussions of evidence hierarchies in medical epistemology: the anti-arrhythmic drug case (AAC). This example was recently offered by Bennett Holman as a case of sponsorship bias (Holman, 2019).

The paper proceeds as follows: *The Anti-Arrhythmic Drug Case* section and the *Sponsorship Bias as Manufactured Certainty* section introduce Holman's interpretation of the AAC as a paradigm case of manufactured certainty. *The Evidential Conception of Epistemic Wrongness* section and *The Social Epistemological Conception of Epistemic Wrongness* section briefly discuss, respectively, Reutlinger's evidential account of epistemic wrongness and Wilholt's social epistemological account. The *Challenging the Evidential Account* section, *The Problem of the Target Level* section, and the *Challenging the Social Epistemological Account section* analyze how these accounts deal with cases of manufactured certainty. In these sections, I will also argue that EAs fail to explain the AAC as an instance of manufactured certainty, while SEAs succeed in doing so, at least on the level of building expert consensus. I conclude that the social epistemological account should be preferred over the evidential account based on its higher explanatory potential in cases like this.

THE ANTI-ARRHYTHMIC DRUG CASE

In order to evaluate the two analyses of sponsorship bias, I shall utilize a socially contextualized version of a paradigmatic case study that is typically interpreted to show the superiority of statistical evidence over mechanistic evidence in clinical decision making (Howick, 2011). Holman takes the socio-political context of the standard version of this case study into account and argues that it presents an instance of massive sponsorship bias. He concludes that the case does not provide sufficient grounds to favor statistical over mechanistic evidence (Holman, 2019) and that the framework of social epistemology is much more useful than that of traditional epistemology for analyzing collective epistemic practices in medicine (Holman, 2019). I will illustrate these points by presenting both versions of the case, but will focus my attention on the contextually enriched version that highlights the role of the pharmaceutical industry's financial interests. Howick presents the standard version as follows:

> Myocardial infarction often damages the muscle and electrical system in the heart, leaving it susceptible to arrhythmias. A common type of arrhythmia, ventricular extra beats (VEBs), occurs when the left ventricle contracts before it has had time to fill completely. The heart then fails to pump sufficient blood. Without treatment, lung, brain, and kidney damage ensues. Worse, VEBs can also degenerate into ventricular fibrillation, or complete electrical chaos. Sudden death soon follows ventricular fibrillation in the absence of electric shock. Large-scale epidemiological studies suggested that between 25 and 50% of sudden cardiac deaths were associated with arrhythmias [...]. Based on this understanding of the underlying mechanisms, several drugs were developed and found to be successful for regulating VEBs [...]. The drugs became widely prescribed in the belief that they would reduce cardiac deaths (Howick, 2011, p. 126).

> A [...] comparative clinical study [...] the Cardiac Arrhythmia Suppression Trial (CAST), which began in 1987, [...] was designed to test whether antiarrhythmic drugs would reduce mortality in patients who had suffered from myocardial infarction (heart attack). In the study, 27 clinical centres randomized (sic!) 1,455 patients to receive encainide, flecainide, or placebo, while 272 were randomized to receive moricizine or placebo. In April 1989 the encainide, flecainide, placebo arm of the study was discontinued because of excess mortality in the experimental groups; 33 of 730 patients (4.5%) taking either encainide or flecainide had died after an average of 10 months follow-up, while only nine of 725 patients (1.2%) taking placebo had died from arrhythmia and non-fatal cardiac arrest over the same time period. The experimental drugs also accounted for higher total mortality (56 of 730, or 7.7% vs. 22 of 725 or 3.0%). Similar negative results were soon found for moricizine (Howick, 2011, p. 124).

Howick presents this case in order to argue that relying on mechanistic evidence for clinical decision making can have fatal consequences when the underlying physiological mechanisms are complex and insufficiently understood. He argues that the case shows that mechanistic evidence is not only unnecessary to establish causal relations but also that basing one's judgments on statistical evidence from randomized clinical trials does a better job in many cases (Howick, 2011). Holman argues, however, that the standard version omits the broader context concerning how and why the medical profession first decided to rely on mechanistic evidence. He contends that the decision to rely on mechanistic evidence was made despite considerable disagreement among medical experts. Holman's version of the case focuses on the definition of the clinical endpoints of the studies that were needed by the pharmaceutical companies to gain FDA approval for their drugs and on the role of the pharmaceutical industry in establishing these endpoints. His reconstruction adds three important points that cast the incident in a completely different light and explain how belief in mechanistic evidence became prominent in the cardiology community in the first place.

First, Holman notes that even highly accredited experts who promoted the hypothesis that VEBs precipitate sudden cardiac death, such as Bernard Lown, warned that VEBs needed to be

one can validate (non-)epistemic values that shape scientific practices by their long-term empirical success, i.e., the involvement of (non-)epistemic values in a scientific practice is justified insofar as they contribute to the overall success of the practice to produce empirical knowledge. Ohnesorge (2020) has recently made a similar point. I thank an anonymous reviewer for reminding me of this.

suppressed "in only a minority of patients, who usually have ischemic heart disease and a life-threatening or symptomatically disabling arrhythmia" (Lown, 1979, p. 321). This shows that there was actually a scope for interpretation about which therapeutic interventions would be justified if, as hypothesized, suppressing VEBs could help to prevent cardiac arrest. At least from Lown's widely respected perspective, the truth of hypothesis would not have licensed the widespread prescription of the anti-arrhythmic drugs.

Second, Holman explains that the FDA and the pharmaceutical companies together organized a conference to determine what kind of evidence concerning the drugs' efficacy would be required for its approval for therapeutic purposes. The aim was to achieve expert consensus as to whether the clinical trials preceding approval should use death as the endpoint of the study or whether a surrogate endpoint such as the suppression of VEBs would suffice. The expert panel led by cardiologist Joel Morganroth consisted of various academic researchers, industry representatives, and members of the FDA cardio-renal division. Morganroth received support from various pharmaceutical companies to determine the agenda of conference and to frame the subsequent discussions. Holman reports that the speakers at the conference were primarily proponents of industry-friendly positions and favored VEB suppression as an adequate (and cost-efficient) endpoint for the studies. He reports, furthermore, that Morganroth actively used his position to prevent critical discussions of the VEB suppression hypothesis when these were demanded several times by critical researchers during the conference. Even though it was obvious that there was considerable disagreement among the experts in attendance about the therapeutic role of VEB suppression, Morganroth was able to build a strong coalition in favor of the surrogate endpoint. The FDA ultimately accepted this conclusion, even though several FDA members explicitly acknowledged that VEB suppression was not enough to guarantee the therapeutic effectiveness of the drugs. The conference not only reached a decision about the endpoint of the clinical studies but also gave the impression that the relevant experts all endorsed the VEB suppression hypothesis (Holman, 2019).

Third, after approval of the endpoints for clinical trials, several pharmaceutical companies launched a marketing campaign for their upcoming drugs. This campaign included efforts to increase the number of industry-friendly scientific publications on this topic by publishing the same study multiple times in several high-ranking medical journals and, in some cases, hindering the publication of contrary evidence. This campaign was complemented by increased funding for researchers, such as Morganroth, who promoted the VEB suppression hypothesis. Several pharmaceutical companies also distributed copies and reprints of favorable studies to doctors to raise awareness of their upcoming products and hired industry-friendly researchers to conduct cardiology seminars for doctors who might later prescribe the drugs. They also engaged selected cardiologists in so-called seeding trials, allowing them to acquire experience of the drugs before they went to market and to compare them to competing treatments (Holman, 2019).

I will assume, for the purposes of this paper, that Holman's enriched version of the anti-arrhythmic drug case is correct. Holman's version not only undermines Howick's interpretation of the case as revealing the insufficiency of mechanistic evidence but also presents it as a case of massive sponsorship bias.[8] I will argue that it also poses new challenges for the two kinds of accounts of the epistemic wrongness of sponsorship bias. First, it challenges evidential accounts because it shows that decisions about study endpoints and about the kind of evidence necessary to support a hypothesis cannot be explained by reference to confirmation theory. False claims about evidential confirmation relationships can only constitute epistemic wrongs relative to some predefined standard. Second, it challenges social epistemological accounts because it shows that compliance with the methodological standards of a scientific community can have epistemically detrimental results. I will argue, however, that social epistemological accounts can respond to this challenge, while evidential accounts cannot.

My argument will proceed as follows: I will first show that Holman's enriched version of the case represents an instance of sponsorship bias. This will involve identifying the instances of the research that contributed to the anti-arrhythmic drug disaster were actually affected by sponsorship bias. Second, I will explain the challenge to EAs in more detail and show why they cannot fully account for the features that make the example a case of sponsorship bias. Finally, I will explain how this case poses a challenge to SEAs because it shows that infringement of methodological standards is irrelevant to the ascription of epistemic wrongness.

SPONSORSHIP BIAS AS MANUFACTURED CERTAINTY

Holman's enriched version of the case constitutes a *prima facie* drastic case of sponsorship bias. However, because many of the practices described in the case might equally shape research that produces valid results, it is necessary to ask whether AAC is a representative case. As I will show, AAC instantiates a range of strategies that is widely used by the pharmaceutical industry. These strategies promote epistemic errors by leading to the adoption of inappropriate research designs. The enriched version of AAC also permits an interpretation that contains two important criteria for sponsorship bias, namely, the occurrence of an epistemic wrong and the generation of this wrong by some kind of practical interest.

A plausible interpretation of AAC would be that the epistemic wrong consists in a research design that is adequate for determining whether the drugs suppress VEBs but that is inadequate to determining whether the drugs have any therapeutic effect. Hence, claiming that the drugs had a

[8]Holman is not concerned with sponsorship bias in the cited paper, but rather with the preconditions of a practically relevant medical epistemology. Accordingly, he uses both versions of the case to illustrate how the traditional, individualist epistemology that underlies Howick's criticism of mechanistic evidence fails to account for the financial, social, and political interests involved in the determination of epistemic standards in medicine.

therapeutic effect—a claim that was made by researchers in several publications and disseminated by the marketing campaign—was epistemically unjustified, as this had not been shown by the studies that used VEB suppression as an endpoint. This epistemic error was only identified in the subsequent comparative clinical study.[9]

This epistemic wrong was clearly facilitated by practical interests. This is revealed by the influence of the pharmaceutical industry in shaping the make-up and conclusions of the expert panel and the subsequent marketing campaign, which helped create the impression that VEB suppression was accepted by the relevant experts as a guarantee of therapeutic success. The acceptance of the VEB suppression hypothesis, which led to the anti-arrhythmic drug disaster, thus stands as a clear case of manufactured certainty, that is, the impression of certainty over issues that are actually contested.

It is remarkable, furthermore, that the AAC involved several well-known strategies that powerful industries typically use to promote their products. Most of these strategies were pioneered by the tobacco industry from the 1950s onward and are often referred to as the tobacco strategy (Oreskes and Conway, 2010), though they have since been copied by several other industries. The tobacco strategy seeks to hinder the production of scientific knowledge contrary to the interest of the industry. The strategy has five elements: an emphasis on scientific uncertainty, the support of friendly research, the recruitment of distinguished scientists, the creation of an echo chamber effect, and attacks on unfavorable scientific research (Fernandez Pinto, 2017). The pharmaceutical industry did not utilize all of these strategies in AAC, and those it did take up were pursued in a comparatively less aggressive way than by other industries.[10] In AAC, the industry concentrated on recruiting distinguished researchers (so-called key opinion leaders, such as Morganroth) who promoted their position, gaining the support of friendly research, and on creating an echo chamber effect through their marketing efforts, to get their message across to the medical community. On the other hand, attacks on unfavorable research, if they occurred at all, seem to have been rather indirect, such as refusing to fund critical research. Unlike other uses of the tobacco strategy, the pharmaceutical industry did not wish to manufacture doubt or uncertainty in this case. As the description of the expert panel shows, the pharmaceutical companies rather aimed at *promoting certainty* over an issue (the VEB suppression hypothesis) that was actually uncertain and heavily contested within the research community. In sum, these efforts served to distort the academic discourse on the therapeutic efficacy of the anti-arrhythmic drugs, such that the industry-friendly position gained higher visibility than dissenting views in scientific publications and the medical community.

These observations clearly confirm that AAC can be read as a case of sponsorship bias. Let us now consider how this case challenges evidential and social epistemological accounts of this bias.

I will first briefly survey the distinctive features of these two groups of accounts by examining paradigmatic formulations of each: Reutlinger's evidential account of epistemic wrongness (Reutlinger, 2020b) and Wilholt's social epistemological account (Wilholt, 2009, 2013).

THE EVIDENTIAL CONCEPTION OF EPISTEMIC WRONGNESS

Reutlinger defends an evidential account of epistemic wrongness, according to which "research affected by sponsorship bias is epistemically wrong if and only if the researchers in question make false claims about the (degree of) evidential support of some hypothesis H by data E" (Reutlinger, 2020b).

This statement primarily concerns the nature of epistemic wrongness in the empirical sciences. A scientific claim is wrong insofar as it is not sufficiently supported by evidence. This account of epistemic wrongness is introduced as the defining epistemic property of sponsorship bias, so Reutlinger's formulation seems to imply that there could, in principle, be cases of sponsorship bias in which researchers only make claims that are sufficiently supported by the evidence and that would not therefore constitute cases of epistemic wrongness. This implication seems conceptually disturbing—can research be affected by bias but nonetheless be epistemically flawless?—but I will not concern myself with this problem here. Rather, I will take for granted that biased research by definition contains an element of epistemic wrongness and that this holds *ipso facto* for research affected by sponsorship bias.

Reutlinger defends this evidential account by applying insights from confirmation theory to paradigmatic cases of sponsorship bias, such as the Bisphenol A case (vom Saal and Hughes, 2005; Wilholt, 2009; Carrier, 2013; Biddle and Leuschner, 2015), the Celebrex Case (Brown, 2008), and the tobacco strategy (Oreskes and Conway, 2010; Proctor, 2012). According to Reutlinger, the epistemic error in these different cases can be explained by reference to epistemic principles derived from Bayesian confirmation theory (Earman, 1992; Sprenger and Hartmann, 2019) and frequentist hypothesis testing (Mayo, 2011a,b). These theories of evidential confirmation explain what it means for a set of data E to provide evidential support for a hypothesis H and thereby formulate accounts of what it means to be epistemically justified in believing H in light of the available evidence. It is important for evidential accounts of sponsorship bias to invoke such principles of epistemic justification because such accounts explain the epistemic wrongness of a belief in terms of a lack of epistemic justification for holding the belief as true.

The empirical sciences typically conceive of epistemic justification in terms of evidential confirmation.[11] Bayesian confirmation theory and frequentist hypothesis testing are

[9] At least this seems a plausible reading. One might, however, object that researchers did not actually claim that VEB suppression was sufficient evidence for therapeutic effectiveness, but that this claim was merely an implication of the regulations issued by the FDA to approve the respective drugs. We will return to this issue in *The Problem of the Target Level* section.

[10] For a comparison see, for example, the analysis by Oreskes and Conway (2010).

[11] There are also views in the philosophy of science that deny the possibility of evidential confirmation altogether (Popper, 1959/2008). However, falsificationism gains much of its plausibility from its argument against an absolute understanding of confirmation, as opposed to a probabilistic understanding. Current theories of evidential confirmation, however, are invariably probabilistic.

currently the most widely accepted theories of evidential confirmation (Reutlinger, 2020b). Both are probabilistic theories. According to the Bayesian confirmation theory, evidence E supports a hypothesis H, if and only if the probability that H is true given E and relevant background knowledge K is higher than the probability that H is true given only the relevant background knowledge K, or more formally: P(H|E, K) > P(H|K). Applications of this Bayesian principle, however, require consideration of a further principle, that of complete local evidence. This latter principle states that one ought to always consider all *available* data produced in an experiment or series of experiments whenever one wishes to establish the degree of confirmation of a hypothesis. The principle of complete local evidence ensures that Bayesian assessments of subjective probabilities take into consideration potentially defeating evidence and so guards against confirming hypotheses based on selective data.

Reutlinger claims that one or both of these basic epistemic principles are typically violated in paradigmatic cases of sponsorship bias. For example, the famous Bisphenol A case (vom Saal and Hughes, 2005) can be interpreted as a case of biased research because the researchers made false claims about the evidential support for their hypothesis that low doses of Bisphenol A do not increase cancer rates in laboratory rats of the CD(SD) strain. The researchers violated the principle of complete local evidence because there was evidence available at the time that CD(SD) rats are insensitive to estrogens and that Bisphenol A functions as an endocrine disruptor and hence strongly influences the effects of estrogens. This defeating evidence was not taken into consideration. Consequently, the researchers also violated the epistemic principle. By claiming that low doses of Bisphenol A do not increase cancer rates in laboratory rats of the CD(SD) strain, researchers suppressed relevant background knowledge K (i.e., CD(SD) rats are insensitive to the effects of low doses of Bisphenol A) but nevertheless claimed that the results of their experiments supported their hypothesis, or more formally: P(H|E) > P(H|E, K). This is the exact opposite of what Bayesian confirmation theory demands.

Reutlinger suggests that the other two cases can be interpreted similarly. In the Celebrex case (Brown, 2008), researchers violated the principle of complete local evidence because they based their claim that the anti-arthritis drug Celebrex caused fewer side effects than its competitors on evidence from only the first 6 months of their study. Had they considered all available evidences from their own research, the study would not have supported this claim. Focusing on partial evidence instead of complete local evidence ignores available and potentially defeating evidence.

In the context of the tobacco strategy, Reutlinger introduces the case of a researcher who claimed in court that smoking cannot be said to cause lung cancer because being a cause in a scientific sense requires constituting a *necessary and sufficient condition* for an effect.[12] This however, is clearly not the case, as there are people who smoke and never get lung cancer, as well as people who get lung cancer despite never having smoked. In terms of Bayesian confirmation theory, the researcher did not violate either of the two principles in making this claim, but instead confused the very idea of evidential confirmation from which these principles derive. Evidential confirmation operates in probabilistic terms, that is, a hypothesis is more or less likely to be true depending on the degree of confirmation derived from the available evidence. That empirical evidence alone can never establish the necessary and sufficient conditions of an effect has been recognized at least since David Hume's discussion of causation (Hume, 1748/2009).

Reutlinger's evidential account thus explains paradigmatic cases of sponsorship bias as cases in which scientists make false claims based on a misconception of evidential support relationships. On this account, the epistemic wrongness of sponsorship bias is, therefore, primarily a feature of the scientist's assertions and not of their epistemic practices. The researchers in the above cases made false claims insofar as they were unjustified in making these claims given the evidence that was actually available to them. The problem in the Bisphenol A case, for example, was not that the researchers used the insensitive CD(SD) rat strain but that they could have known (and, indeed, probably knew) that using this strain in an experiment could not provide evidence that could actually confirm their hypothesis and yet nevertheless claimed that it did. This shows that evidential accounts tend to construe biased research as analogous to erroneous research. Errors occur due to deviations from valid and generally accepted epistemic principles and researchers can be blamed for committing such an error if they knew or should have known the relevant principles.[13] Such errors must, however, be distinguished from false beliefs that do not originate from such epistemic deviations and have no implications for blameworthiness. EAs show that bias and error are similar insofar as bias not only indicates a false belief but a false belief that should (and often could) have been avoided. Biased research and erroneous research are thus epistemically wrong for the same kinds of normative reasons.[14]

[12]The original description of the case can be found in Proctor (2012).

[13]Blaming someone for an error only seems justified if one supposes that the person should have known the norms that she violated when making the error. What someone in a certain situation could or should have known, however, depends on social norms about what we can reasonably expect each other to know.

[14]I believe that this coupling of bias and error represents a major problem with Reutlinger's account. There are important differences between our ordinary concepts of bias and error that cannot be accounted for in purely epistemic terms. Bias implies that violations of valid epistemic principles are brought about in a way that involves *specific* and wrongful epistemic practices. For example, one can make an error due to negligence, inattentiveness, or bad luck, but one's reasoning is not rendered biased by such cognitive failures alone. The term bias refers to *structural conditions* (cognitive or social) that systematically influence the epistemic practices that justify one's beliefs. An error is just the *result* of processes that are influenced by these cognitive or social conditions. Reutlinger's account seems to blur the distinction between bias and error because his focus on epistemic wrongness leaves out the conditions of error formation that are decisive for understanding bias. Insofar as these structural factors are decisive, his evidential account of epistemic wrongness does not sufficiently discriminate between error and bias as two subspecies of flawed research and, hence, does not have the resources to fully explain the phenomenon of sponsorship bias.

Reutlinger might respond that his primary goal was only to deliver an account of epistemic wrongness, which might later be supplemented with more concrete descriptions of the mechanisms that make the epistemic wrong more likely to occur in cases of sponsorship bias. However, while describing such mechanisms would surely be helpful, it remains unclear how such a description relates to the

THE SOCIAL EPISTEMOLOGICAL CONCEPTION OF EPISTEMIC WRONGNESS

Social–epistemological accounts (SEAs) of the epistemic wrongness of sponsorship bias approach the phenomenon from a different angle. Like EAs, they share the intuition that there is something genuinely epistemically wrong in cases of sponsorship bias, but they explain the relevant epistemic shortcomings in terms of epistemic social practices rather than evidential support. This has the advantage of better accounting for the social mechanisms that lead to epistemic wrongs in a specific research setting.

I will here treat Wilholt's widely discussed SEA as paradigmatic of this type of account (Wilholt, 2009, 2013). Wilholt argues that the epistemic wrong in cases like those discussed above consists in "the infringement of an explicit or implicit conventional standard of the respective research community in order to increase the likelihood of arriving at a preferred result" (Wilholt, 2009, p.99). He argues in a more recent work that such conventional methodological standards are epistemically relevant on a collective level because they enable mutual trust between the members of a research community and so help to coordinate the joint activity of scientific knowledge production (Wilholt, 2013, 2016). An important motivation for SEAs, and for Wilholt's account in particular, is the critique of the value-free ideal of science. If one takes seriously the insight, mentioned above, that all empirical research involves making judgments based on non-epistemic values and that complete freedom from such values cannot be the hallmark of unbiased research, it seems impossible that accepting the truth of a hypothesis could be epistemically justified solely on evidential grounds. As the argument from inductive risk shows, value judgments about the consequences of falsely accepting a hypothesis are necessarily invoked when determining the degree of evidential confirmation necessary to endorse a hypothesis. If the stakes are high, and the consequences of false acceptance are sufficiently bad, a higher degree of confirmation will be necessary than in cases where less is at stake. Wilholt argues that it is impossible to objectively determine the degree of confirmation necessary for accepting a hypothesis and that any measures utilized by a specific research community must therefore be merely conventional (Wilholt, 2009). For example, the level of statistical significance that determines the degree of confirmation needed to accept a hypothesis is a methodological convention of a research community. This level can, in principle, vary between scientific disciplines and contexts of investigation. However, even though these standards are merely conventional, they nevertheless serve an important epistemic function. Without such common methodological standards, a research community could not coordinate their research activities in a proper manner. Methodological conventions are needed to establish mutual trust in the results of research between the members of a scientific community. To see this, consider for example, a research community that employs various levels of statistical significance (say 0.05, 0.07, and 0.09), allowing hypotheses to be accepted or rejected depending on the chosen level of significance. This ambiguity would lead to confusion about what statistical significance means and which studies should be accepted as making valid claims. It would thus undermine the reliability of research results, and therefore also the coordination of collective processes of knowledge production.

We can now attend to an important difference between Reutlinger's version of EA and Wilholt's version of SEA. Wilholt's account accepts the value-ladenness of scientific inquiry and so centers on the issue of what degree of evidential confirmation C is needed to accept a hypothesis H in a given context. In contrast, Reutlinger's account focuses on the question of whether the evidence available suffices for the researcher to accept hypothesis H. For Wilholt, therefore, it is not enough to show for a given hypothesis H that $P(H|E, K) > P(H|K)$. It is more important, on this SEA account, to show that the probability of H given a set of data E and relevant background knowledge K is sufficiently high to accept H, that is, to show that it exceeds a certain threshold of evidential confirmation.[15] More formally:

$$P(H|E, K) > C > P(H|K)$$

The crucial question for Wilholt's account, therefore, is how to determine the exact threshold level of confirmation C such that P(H|E, K) justifies believing H given the available evidence and background information. This threshold can only be established conventionally.[16] It is therefore impossible to evaluate the epistemic merits of accepting or rejecting a hypothesis solely by assessing all of the local evidence using Bayesian confirmation theory.

Reutlinger (2020b) highlights an obvious problem with the role of such conventional thresholds in hypothesis confirmation. Epistemic wrongness can be conventionally defined in terms of undermining collective epistemic practices that establish a specific threshold C, but it remains unclear why infringing such a convention would be *epistemically unjustified,* for it might be that the chosen level for C is epistemically inadequate. Consider, for example, the Bisphenol A case in which, according to Wilholt's analysis, the epistemic shortcoming consisted in the researcher's infringement of the methodological convention not to use CD(SD) rats in experiments to determine the carcinogenic effects of Bisphenol A. Now, one might say that the reason for this convention was that evidence gathered using CD(SD) rats does not raise P(H|E,K) above C. However, if not using the rats was merely a convention grounded in practical rather than epistemic

occurrence of error. What needs to be shown is how, for example, conflicting interests or financial incentives make the occurrence of the described error more likely than in cases in which these influences are absent.

[15]In this respect, Wilholt's account is reminiscent of Lockean approaches to the rationality of belief revision [Foley (1992)]. According to the so-called Lockean thesis, an epistemic agent who assigns credence to propositions in proportion to the available evidence is required to believe all and only those propositions to which she assigns sufficiently high credence, *viz.*, credence above some threshold level t (Shear and Fitelson, 2019). I am grateful to an anonymous reviewer for making me aware of this point.

[16]It should be obvious that this account requires, at the minimum, that $C > 0.5$. However, how much larger than 0.5 the threshold must be cannot be determined by the available evidence.

considerations, it seems difficult to argue that the researchers made an *epistemic* mistake by using the rats.

If methodological standards are merely conventional, there is no *epistemic* reason to believe that one standard is more apt than another. In Wilholt's framework, methodological conventions are chosen because of their functionality for coordinating collective practices, not because they provide epistemic justification in terms of evidential confirmation (Wilholt, 2013). How important is this critique of Wilholt's SEA? In offering a social–epistemological account, Wilholt is not committed to individualistic conceptions of knowledge and justification. If one conceives of scientific knowledge as something produced by a collective, epistemic justification cannot reside in the reasons and evidence of any individual researcher. It must instead reside in the way a scientific community organizes the social practice of confirming and refuting hypotheses. This, however, raises the question on how to determine whether these social practices are epistemically adequate and successful in producing reliable results.

In defending his social epistemological account, Wilholt emphasizes the role of the division of cognitive labor in science (Wilholt, 2013, 2016). If science can only be epistemically successful as a collective endeavor, the criterion for assessing the aptness of conventional methodological standards must be the capacity of these standards to enable collaboration between scientists and mutual epistemic trust in their ability and willingness to report reliable results. Trust and reliability can thus themselves be considered epistemic criteria insofar as they are important to the sharing of research results and hence to the effectiveness of the division of cognitive labor and the collective search for truth. While this argument does not directly explain why it is *epistemically unjustified* for an individual researcher to infringe conventional methodological standards, it shows that such standards have a crucial epistemic function and that failure to abide by them can undermine the collaborative production of scientific knowledge. In SEAs, questions about epistemic justification must be answered with reference to the degree to which the relevant social practices are functional for bringing about beliefs that are appropriately sensitive to the relevant evidence.

We can now consider how these two paradigmatic accounts of epistemic wrongness would treat the anti-arrhythmic drug case, as described by Holman. The *Challenging the Evidential Account* section and *The Problem of the Target Level* section will discuss problems with EAs. The *Challenging the Social Epistemological Account* section formulates a challenge to SEAs and discusses a possible response.

CHALLENGING THE EVIDENTIAL ACCOUNT

In this section, I will argue that EAs are ill suited to account for the influence of the pharmaceutical industry in AAC because of their focus on evidential confirmation. EAs ask whether individual researchers were justified in making a claim based on the evidence available to them. What does this mean for AAC? Were the researchers involved in the pharmaceutical industry's studies justified in believing that anti-arrhythmic drugs not only suppressed VEBs but were also therapeutically efficient? I think that they were, given the officially held and widely disseminated background belief that the VEB hypothesis was true and the preliminary evidence from *in vitro* and animal studies, which supported the existence of a causal mechanism linking anti-arrhythmic drugs to VEB suppression. The mechanistic evidence E combined with the background belief B (that VEB suppression prevents heart failure and death) to provide stronger support for the hypothesis H that the anti-arrhythmic drugs were therapeutically efficient than was given by the background belief B alone. So $P(H|E, B) > P(H|B)$ holds.[17] It also seems hard to argue that the researchers violated the principle of complete local evidence. Of course, there probably were studies available to them that provided counter-evidence to the VEB suppression hypothesis. However, as these studies were far outnumbered by publications suggesting the opposite, it seems that individual researchers cannot be accused of endorsing a hypothesis contrary to considerable defeating evidence. Even if the counter-evidence was fairly considered by the researchers, they were—according to the EA—epistemically justified in drawing the conclusion that anti-arrhythmic drugs are therapeutically effective.

A natural response to this argument is to argue that every epistemic agent—and scientists in particular—has a duty to question all the background assumptions of their claims. In AAC, this would have involved questioning the plausibility of the endpoint, the reliability of the expert panel that issued it as a standard, and the mainstream opinion in the cardiology community that the VEB suppression hypothesis was true. Had researchers taken into consideration evidence about the conditions under which the decision for the endpoint was taken, they would not have been justified in accepting the background belief B that the VEB suppression hypothesis was true and that VEB suppression represented a suitable endpoint for determining the therapeutic effects of the drugs.

However, this argument is more epistemically demanding than EAs, or at least than Reutlinger's, which I here treat as paradigmatic of EAs. The principle of complete local evidence only requires assessment of the *available local* evidence (Reutlinger, 2020b). The evidence required by this argument would neither be local nor, to a large extent, available because the debates in the expert panel were not transparent to the ordinary scientist, let alone to medical practitioners or patients. Moreover, the objection requires that individual researchers be more independent from the knowledge of other scientists than is plausible and have implausibly extensive abilities to double-check every premise of their argument. Scientific research in complex areas such as medicine involves a division of cognitive labor, which, as many have recognized, requires that researchers can mutually rely on each other for the truth of their reported research results, at least to some extent. This is not to say, of course, that researchers need not or should not check whether they can reproduce each other's results. However, this is often

[17] I use the variable B instead of K because obviously the background belief is in fact false and, hence, does not amount to background knowledge.

unnecessary (e.g., when a third party has already done it) or irrelevant (as when one's conclusions do not conflict with background knowledge). While a healthy skepticism is surely helpful to the scientific endeavor, scientists must make choices about when it is appropriate to adopt a skeptical stance. Limited expertise and lack of time are simple pragmatic reasons for limiting skepticism.

One problem with EAs, therefore, seems to be that they do not take into account the social context that helps determine confirmation relationships between hypothesis H, evidence E, and alleged background knowledge K. EAs do not require researchers to check for suspiciously skewed distributions of studies providing either confirming or defeating evidence or to question the genesis of the background knowledge underlying (mainstream) work in their field. EA also lacks the means to inquire into these social conditions because it does not involve epistemic principles that work at the collective level. It does not include any rules about how the pursuit of scientific consensus should be organized so that epistemic goals can be met, and it says nothing about the distribution of true and false beliefs within a community. EA accounts are thus of limited use as tools to analyze cases of sponsorship bias. As this analysis of AAC shows, researchers can make false claims and contribute to biased research without being wrong about evidential confirmation or misunderstanding confirmation relationships altogether.

THE PROBLEM OF THE TARGET LEVEL

So far, my argument against EA has focused on the claims of the researchers involved in the trials that used VEB suppression as an endpoint. This focus may, however, make the argument against EA too easy because one could object that the researchers involved in these studies are not the relevant target of its analysis. Perhaps the epistemically problematic claim in AAC was the expert panel's claim that VEB suppression is a valid means of predicting the long-term survival of patients. This is the claim that should be regarded as unjustified by the standards of EA because the evidence concerning the connection between VEB suppression and heart failure was, in fact, inconclusive and therefore unsuitable to confirm or falsify the hypothesis.[18] It was this unjustified claim that led to the methodologically flawless but erroneous research by the individual scientists.

This initially appears to be a more serious objection to my argument. I will show, however, that this response relies on a misunderstanding of the applicability of confirmation theory to this panel's decision. If one takes these constraints on its application into account, one sees that the determination of the study endpoint by the expert panel must be analyzed in ethical as well as epistemic terms, which goes beyond the scope of EA.

[18]This argument echoes Howick's argument for preferring statistical over mechanistic evidence in therapeutic decision making. Pathophysiological mechanisms are often unknown or too complex to allow for definite predictions (Howick, 2011). Consequently, it is too risky to rely on them when there is a lot at stake. As Howick interprets the case, the failure of the experts was exactly this—they relied on "low quality" mechanistic evidence about the connection between VEB suppression and death, when statistical evidence would have been more appropriate.

The objection that, for EA, the expert panel's claim is the relevant target for understanding the epistemic wrongness in AAC implies that the hypothesis "VEB suppression is a reliable indicator for therapeutic effectiveness" (H_1) was not supported by the evidence. We do have good reasons to believe that this was the case. First, as Howick (2011) reports, when the expert panel met, studies about the supposed causal mechanism linking VEB suppression and patient survival were ambiguous. In the absence of conclusive evidence, the experts were unjustified in endorsing H_1; they should have suspended their judgment because $P(H_1|E, K)$ was not, in fact, (significantly) larger than $P(H_1|K)$. On this view, the expert panel failed because it did not base its endorsement of H_1 on conclusive evidence. Second, the panel did not properly acknowledge views opposing the VEB suppression hypothesis and thus did not consider potentially defeating evidence. If the panel had complied with the principle of complete local evidence, $P(H_1|E, K)$ would probably have actually been smaller than $P(H_1|K)$, such that the rational response would have been to hold that H_1 was false.

A proponent of EA can therefore claim that the epistemic wrongness of AAC consisted in the expert panel making a claim that was not supported by the available local evidence. However, this line of reasoning presupposes that it is possible to distinguish between two different wrongs involved in this case: the epistemic wrong involved in the panel's erroneous claim and the ethical wrong of the panel's dubious evaluation of the inductive risks associated with H_1. This presupposition is false. I will argue that it is not possible to clearly distinguish between these two wrongs in AAC and thus that the above explanation of the EA account cannot withstand critical scrutiny. More precisely, my argument is that one can commit the epistemic wrong without committing the ethical wrong, which EA rightly acknowledges, but that one cannot commit the ethical wrong without also committing the epistemic wrong, which EA does not sufficiently acknowledge.

I shall first explain why AAC involved an ethical as well as an epistemic wrong. It is ethically wrong to prefer the hypothesis "VEB suppression is a reliable indicator for the therapeutic effectiveness of anti-arrhythmic drugs" (H_1) over the competing hypothesis "an increased patient survival rate is a reliable indicator for the therapeutic effectiveness of anti-arrhythmic drugs" (H_2). Such a preference is unethical because the primary aim of producing the drug should be to heal or at least to improve the health of patients after heart attacks. Preferring H_1 over H_2 would not be the optimal choice by this metric even if H_1 was true. Even if the VEB suppression hypothesis was true and would guarantee patient survival, one could still not rule out possible further downstream effects on patient health. To optimally determine the potential risk of a drug, it would, in any case, have been better to choose an endpoint as far downstream as possible, which would be the death of the patient. Therefore, on the assumption that the experts on the panel were committed to improving patient health, their decision to choose VEB suppression as a general standard was not only an epistemic but also an ethical wrong.

One might resist the claim that the panel's decision constituted an ethical wrong by pointing out that death would not have been the optimal endpoint from the perspective of all involved.

Such a choice would, for example, have significantly prolonged the study and thus delayed the drugs' availability. This would have delayed the treatment of patients struggling with heart disease, leading some to die prematurely. Choosing death as an endpoint would also not have helped settle academic disputes about the physiological mechanism underlying the effects of anti-arrhythmia drugs. As everyone eventually dies, death is the most unspecific endpoint possible if one is interested in the causal physiological mechanism of the drug. The expert panel thus faced a difficult trade-off between these different interests, and in choosing VEB suppression as a suitable endpoint, they granted lower priority to the interests of future patients than they should have. Setting endpoints for clinical trials is always a trade-off between different values and interests, but very strong arguments are needed to justify giving a relatively lower priority to the *prima facie* duty of benefitting the long-term health and survival of study participants and prospective patients. The panel did not seem to offer or consider any such arguments. It is plausible, therefore, that, even though the panel had to weigh competing interests, it committed an ethical wrong by, at the least, failing to provide an ethical justification for its decision to favor VEB suppression as a clinical endpoint.

Having established that the expert panel committed an ethical and an epistemic wrong in AAC, I shall now argue that EA does not appropriately account for the dependence relation between these wrongs. This leads EA to blur the distinction between error and bias and to unduly ignore the influence of relevant non-epistemic factors on epistemic processes. I will now show that the epistemic wrong of accepting H_1 despite inconclusive evidence depends on the ethical wrong of preferring H_1 over H_2.

It is certainly logically possible to commit the epistemic wrong without committing the ethical wrong. An expert panel might wrongly conclude that the available local evidence favors H_1 and yet judge that establishing H_1 as an endpoint in a clinical trial is ethically unjustified. One can consistently endorse the truth of H_1 and deny that H_1 is an ethically justifiable endpoint. However, it is impossible to commit the ethical wrong without also committing the epistemic wrong. An expert panel cannot consistently hold that it would be ethically acceptable to define H_1 as the study endpoint while also holding that H_1 is wrong. Anyone committing the ethical wrong necessarily also commits the epistemic wrong. One simply cannot consistently opt for a study endpoint that one believes has nothing to do with the causal effects of the drug. EA cannot properly account for this relationship between the ethical wrong and the epistemic wrong.

EA describes AAC's epistemic problem solely with respect to the fallacious endorsement of H_1, and thus abstracts away from the social conditions that bring this mistake about, including the ethical wrong. EA does not take into account that the practical interests that led to the ethical wrong also implied the epistemic wrong. EA can explain *why* the expert panel's conclusion was wrong, but it cannot account for *how* the social circumstances contributed to the panel reaching this wrong conclusion. EA thus construes AAC as a case of collective cognitive error rather than of genuine bias. By disconnecting the cognitive aspect of bias from the non-epistemic, social aspects that cause the error, EA fails to distinguish between error as a mere epistemic failure and bias as an epistemic failure caused by non-epistemic motives.

This becomes more obvious when we consider the epistemic failure in relation to the expert panel's task of evaluating the competing hypotheses H_1 and H_2 in order to determine the appropriate endpoint for the study. As the case is described, the expert panel was not epistemically justified in accepting either H_1 or H_2. In the case of H_2, this is because there was no known biochemical mechanism leading from the use of the drug to the survival of the patient. Patient survival thus could not have indicated any therapeutic effect, let alone a specific causal effect of the anti-arrhythmic drugs. So, with respect to H_2, the expert panel should have suspended judgment. As there was insufficient evidence for accepting either H_1 or H_2, the expert panel had no epistemic reason to prefer either hypothesis. Given that the panel's task was to decide which of the two hypotheses was better supported by the evidence, by the standards of EA, it should not have endorsed either of them. It should instead have concluded that the evidence was inconclusive and that more research was needed.

If this analysis is correct, proponents of EA will struggle to explain how the epistemic error could have occurred without accepting that non-epistemic reasons were decisive, such as the pressure to reach a decision. The epistemic wrong certainly consisted in falsely asserting that H_1 was true, but given that both options available to the panel were epistemically problematic, the only possible explanation for their decision is their preference for H_1.

Seen this way, the expert panel's task was not to determine which of the two competing hypotheses was better supported by the evidence, but what kind of standard for epistemic justification was acceptable in this case. This is an evaluative question that cannot be answered by evidential considerations alone.

It is significant that EA is neutral on the question of whether science should be conceived of as value-free. Reutlinger regards this neutrality as an advantage (Reutlinger, 2020b). The above discussion, however, shows that EA is of limited use in cases like AAC because the expert panel was making a decision about the proper standards for epistemic justification. Such decisions involve an assessment of the ethical consequences of choosing one standard over the other, and hence involve value judgments. So, if proponents of EA wish to insist that the relevant instance of epistemic wrongness is to be located on the level of the expert panel, they cannot maintain that an account of epistemic wrongness can properly ignore the role of values in science and focus only on narrower evidential concerns. In sum, EA lacks the resources to explain AAC as a case of epistemic wrongness.

CHALLENGING THE SOCIAL EPISTEMOLOGICAL ACCOUNT

In order to reach a fully considered decision between the two proposed analyses of sponsorship bias, it is necessary to also consider how the social epistemological account treats AAC. I will argue that AAC also poses a challenge to SEA, but I will also argue that SEA has better resources than EA to respond to this challenge.

In AAC, the expert panel established a corrupted methodological standard. Therefore, it seems that one cannot

explain the epistemic wrongness of the case in terms of individual researchers infringing that standard. We still might want to say that evidence produced by the pharmaceutical industry was biased, as it was based on the corrupted methodological standard. However, we cannot make this claim on the grounds required by SEA, which invoke the epistemic practices of the whole research community. The pharmaceutical industry's research into the effectiveness of the drug was conducted on the premise that the VEB hypothesis was true and thus was perfectly in line with the conventional standard of the research community. Therefore, this research cannot be criticized for infringing a conventional methodological standard. Rather, the work of critical researchers who challenged the VEB suppression hypothesis would have to be accused of this infringement.

However, a proponent of SEA might mount a similar response to the proponent of EA and argue that the methodological standard used by the researchers is not the relevant target of epistemic critique. This standard was the result of an infringement of more general standards of scientific discourse by the expert panel and the FDA. One could argue, for example, that the expert panel in AAC infringed the rule that in, an open scientific discourse, all positions should be heard and all relevant evidence considered. Epistemic wrongness might thus still be explained as an infringement of a conventional rule. Just as an EA proponent might want to claim that the panel's decision was not properly based on complete local evidence, a proponent of SEA might want to argue that rules of building a valid scientific consensus were infringed by (some of) the experts on the panel.

From a social epistemological perspective, there are good reasons to conclude that the expert panel's decision-making process infringed the standards of an epistemically fruitful scientific discourse. Epistemologists such as Longino (1990) and Kitcher (2001) have long argued that a plurality of perspectives and a critical and open discourse are preconditions for successful scientific inquiry. From the perspective of theoretical frameworks that emphasize the collective nature of scientific knowledge, it can plausibly be argued that the rules governing these collective practices should establish these conditions in order to enable reliable knowledge production. However, it is highly doubtful that these rules should themselves be regarded as merely conventional. Such rules are valid not simply because they are conventional but because they are grounded in the epistemological principle that a proposition is more likely to be true if it can be independently confirmed from multiple perspectives. Whether a proposition can be confirmed in this way, however, is not simply a question of actually reaching an agreement, but of what the different parties deliberating about the issue actually have reason to believe. The development of collective knowledge through discourse therefore has a rational basis. From this perspective, proponents of SEA do seem to have the resources to explain what went epistemically wrong in EA's analysis of the expert panel.[19]

One might wonder whether one could make the same point from the perspective of Wilholt's specific SEA, which I introduced as a paradigmatic of the approach. Wilholt's account seems to differ from those of Longino and Kitcher because it conceives of methodological standards as somehow creating the conditions under which scientific inquiry can flourish, rather than as grounded in a foundational epistemic principle such as the diversity of perspectives. Conventional standards are epistemically relevant for Wilholt because they enable scientific inquiry as a collective endeavor. It should, however, also be possible to conceive of the failure of the expert panel as an infringement of (higher order) conventional standards from the perspective of Wilholt's account. The scientific community must be able to rely on expert panels to determine methodological standards in a way that ensures that research aligns with contextually relevant non-epistemic values. In AAC, these values would include, most relevantly, the value of promoting public health rather than private profit. The expert panel should have chosen a stricter standard than VEB suppression in order to be worthy of the trust of the broader scientific community. This analysis assumes that methodological standards should be representative of the shared values of the members of the scientific community. The irony is that a conventional standard can only enable the epistemic trust that Wilholt's account demands if it is representative of the shared values of the research community. From the perspective of a social epistemological account like Wilholt's, the expert panel in AAC can be seen to have disregarded the relevant values of the scientific community. It thereby not only implemented a dysfunctional standard that did not enable epistemic trust, but also infringed the (implicit) norm of finding a standard that was representative of the values of the research community, and not the pharmaceutical industry.

If we accept this analysis, then we can see that social epistemological accounts provide a more plausible analysis than evidential accounts of the kind of manufactured certainty seen in AAC. I conclude that, insofar as AAC represents a case of sponsorship bias, SEA has more explanatory power. This suggests that it is more fruitful to assess the epistemic wrongness of sponsorship bias from a social epistemological rather than an individualist perspective. Focusing only on relationships of evidential support not only neglects the causal influence on research practices of the preferences of various stakeholders and how they shape the evaluation of evidential support relationships but also fails to account for the role of values and decision making in scientific research. As AAC shows, the latter is crucial, at least for some paradigmatic cases of sponsorship bias.

CONCLUSION

This paper compared two recent accounts of the epistemic wrongness of sponsorship bias (SB): the evidential account (EA) and the social epistemological account (SEA). The advantages and disadvantages of these accounts were illuminated by applying them to a paradigmatic case of sponsorship

[19]SEA might also identify other epistemic errors in AAC. For instance, one might also argue from a social epistemological perspective that the marketing strategies applied by the pharmaceutical industry infringed standards of transparency or that the one-sided funding of friendly research violated the requirement to give equal consideration to different perspectives.

bias. This case can be interpreted as one of manufactured certainty, in which the financial interests of stakeholders contributed to the establishing of epistemically inadequate methodological standards.

Evidential accounts give a convincing account of what goes epistemically wrong in many cases of sponsorship bias and identify the fundamental epistemic flaw as involving the making of assertions that are not backed up by the available local evidence or that misunderstand evidential support relations. However, evidential accounts struggle to explain how these epistemic flaws are produced by the concrete epistemic practices of knowledge-producing community. As a result, they struggle to properly distinguish between bias and error, and also to account for cases such as AAC, which involve infringements of the normative structure of scientific research. Social epistemological accounts, on the other hand, can quite easily explain how practices lead to instances of bias because they explain the epistemic wrongness of bias in terms of breaking the conventions of scientific practice. However, as a result of their emphasis on practices and conventions, SEAs in turn face the problem of providing an epistemological basis for evaluating the infringement of merely conventional standards. I have argued that this problem can be resolved by supplementing the conventional view of epistemic wrongness with a robust social epistemology that, like Wilholt's view, explains the epistemic significance of conventions through their relevance to collective processes of knowledge generation. More importantly, however, SEAs, unlike EAs, also have the conceptual resources to explain cases of sponsorship bias such as AAC because their focus on collective practices facilitates analysis of decision-making processes that are responsive to values as well as to evidence. These cases suggest that an alleged advantage of EAs, that they can remain neutral regarding the value-ladenness of science, is actually a disadvantage. The inability of EA to properly distinguish bias and error is an expression of exactly this disadvantage. Approaches like SEA, that link epistemological concerns with concerns about the role of social and ethical values in science, are thus more useful than EA for research into sponsorship bias.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

JL declares that the complete manuscript is product his own research and writing.

FUNDING

This work was funded by Deutsche Forschungsgemeinschaft (DFG, German Research Foundation: Grant-no. 396775817 / LE 4086/1-1).

ACKNOWLEDGMENTS

The author whishes to thank Louise Chapman for support with the language editing of the final manuscript.

REFERENCES

Adam, M., Carrier, M., and Wilholt, T. (2006). How to serve the customer and still be truthful: methodological characteristics of applied research. *Sci. Public Policy* 33, 435–444. doi: 10.3152/147154306781778849

Barnes, B., Bloor, D., and Henry, J. (1996). *Scientific Knowledge: A Sociological Analysis*. Chicago, IL: University of Chicago Press.

Beeghly, E., and Madva, A., (eds.). (2020). *An Introduction to Implicit Bias: Knowledge, Justice, and the Social Mind*. New York, NY: Routledge.

Betz, G. (2013). In defence of the value free ideal. *Eur. J. Philos. Sci.* 3, 207–220. doi: 10.1007/s13194-012-0062-x

Betz, G. (2017). "Why the argument from inductive risk doesn't justify incorportaing non-epistemic values in scientific reasoning," in *Current Controversies in Values and Science*, eds K. C. Elliott and D. Steel (New York, NY, London: Routledge), 94–110.

Biddle, J. B., and Leuschner, A. (2015). Climate skepticism and the manufacture of doubt: can dissent in science be epistemically detrimental? *Eur. J. Philos. Sci.* 5, 261–278. doi: 10.1007/s13194-014-0101-x

Brown, J. R. (2008). "The community of science®," in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, eds D. Howard, J. A. Kourany, and M. Carrier (Pittsburgh, PA: University of Pittsburgh Press), 189–216.

Carrier, M. (2008). "Science in the grip of the economy: on the epistemic impact of the commercialization of research," in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, eds D. Howard, J. A. Kourany, and M. Carrier (Pittsburgh, PA: University of Pittsburgh Press), 217–234.

Carrier, M. (2013). Values and objectivity in science: value-ladenness, pluralism, and the epistemic attitude. *Sci. Educ.* 22, 2547–2568. doi: 10.1007/s11191-012-9481-5

Douglas, H. E. (2000). Inductive risk and values in science. *Philos. Sci.* 67, 559–579. doi: 10.1086/392855

Douglas, H. E. (2007). "Rejecting the ideal of value-free science," in *Value-Free Science? Ideals and illusions*, eds A. Wylie, H. Kincaid, and J. Dupré (Oxford, New York, NY: Oxford University Press), 120–139.

Douglas, H. E. (2009). *Science, Policy, and the Value-Free Ideal*. Pittsburgh, PA: University of Pittsburgh Press.

Douglas, H. E. (2013). The value of cognitive values. *Philos. Sci.* 80, 796–806. doi: 10.1086/673716

Earman, J. (1992). *Bayes or Bust? A Critical Examination of Bayesian Confirmation Theory*. Cambridge, MA: MIT Press.

Fernandez Pinto, M. (2017). To know or better not to: agnothology and the social construction of ignorance in commercially driven research. *Sci. Technol. Stud.* 30, 53–72. doi: 10.23987/sts.61030

Foley, R. (1992). The epistemology of belief and the epistemology of degrees of belief. *Am. Philos. Q.* 29, 111–124.

Goldman, A. I. (1999). *Knowledge in a Social World*. Oxford, New York, NY: Clarendon Press.

Hempel, C. G. (1965). "Science and human values," in *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, ed R.E. Spiller (New York, NY, London: Free Press; Collier Macmillan), 81–96.

Holman, B. (2019). Philosophers on drugs. *Synthese* 196, 4363–4390. doi: 10.1007/s11229-017-1642-2

Holman, B., and Bruner, J. (2015). The problem of intransigently biased agents. *Philos. Sci.* 82, 956–968. doi: 10.1086/683344

Holman, B., and Bruner, J. (2017). Experimentation by Industrial Selection. *Philos. Sci.* 84, 1008–1019. doi: 10.1086/694037

Holman, B., and Elliott, K. C. (2018). The promise and perils of industry-funded science. *Philos. Compass* 13:e12544. doi: 10.1111/phc3.12544

Howick, J. (2011). *The Philosophy of Evidence-Based Medicine*. Chichester: Wiley-Blackwell BMJ Books.

Hume, D. (1748/2009). *An Enquiry Concerning Human Understanding: A Critical Edition*. Oxford: Clarendon Press.

Kitcher, P. (2001). *Science, Truth, and Democracy*. New York, NY: Oxford University Press.

Kuhn, T. S. (1977/2000). "Objectivity, value judgement, and theory choice," in *The Essential Tension: Selected Studies in Scientific Tradition and Change*, ed T. Kuhn (Chicago, IL: University of Chicago Press), 320–339.

Lexchin, J., Bero, L. A., Djulbegovic, B., and Clark, O. (2003). Pharmaceutical industry sponsorship and research outcome and quality: systematic review. *BMJ* 326, 1167–1170. doi: 10.1136/bmj.326.7400.1167

Longino, H. E. (1990). *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton, NJ: Princeton University Press.

Lown, B. (1979). Sudden cardiac death: the major challenge confronting contemporary cardiology. *Am. J. Cardiol.* 43, 313–328. doi: 10.1016/S0002-9149(79)80021-1

Lundh, A., Lexchin, J., Mintzes, B., Schroll, J. B., and Bero, L. (2017). Industry sponsorship and research outcome. *Cochrane Database Syst. Rev.* 2:MR000033. doi: 10.1002/14651858.MR000033.pub3

Mayo, D. G. (2011a). *Error and the Growth of Experimental Knowledge*. Chicago, IL: Univ. of Chicago Press.

Mayo, D. G. (2011b). "Learning from error, severe testing, and the growth of theoretical knowledge," in *Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science*, eds A. Spanos and D. G. Mayo (Cambridge: Cambridge University Press), 28–57.

Melo-Martín, I. de (2019). The commercialization of the biomedical sciences: (mis)understanding bias. *Hist. Philos. Life Sci.* 41:34. doi: 10.1007/s40656-019-0274-x

Melo-Martín, I. de, and Intemann, K. (2016). The risk of using inductive risk to challenge the value-free ideal. *Philos. Sci.* 83, 500–520. doi: 10.1086/687259

Ohnesorge, M. (2020). The limits of conventional justification: inductive risk and industry bias beyond conventionalism. *Front. Res. Metr. Anal.* 5:599506. doi: 10.3389/frma.2020.599506

Oreskes, N., and Conway, E. M. (2010). *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues From Tobacco Smoke to Global Warming*. New York, NY: Bloomsbury Press.

Popper, K. R. (1959/2008). *The Logic of Scientific Discovery*. London: Routledge.

Proctor, R. N. (2012). *Golden Holocaust: Origins of the Cigarette Catastrophe and the Case for Abolition*. Berkeley, CA: University of California Press.

Resnik, D. B. (2000). Financial interests and research bias. *Perspect. Sci.* 8, 255–285. doi: 10.1162/106361400750340497

Reutlinger, A. (2020a). "Strategischer Wissenschaftsskeptizismus," in *Wissenschaftsreflexion: Interdisziplinäre Perspektiven auf Theorie, Praxis und Ethik der Wissenschaften*, eds M. Jungert, A. Frewer, and E. Mayr (Paderborn: Mentis), 351–370.

Reutlinger, A. (2020b). What is epistemically wrong with research affected by sponsorship bias? the evidential account. *Eur. J. Philos. Sci.* 10:g5949. doi: 10.1007/s13194-020-00280-2

Robinson, M. D. (2019). Financializing epistemic norms in contemporary biomedical innovation. *Synthese* 196, 4391–4407. doi: 10.1007/s11229-018-1704-0

Rudner, R. (1953). The scientist qua scientist makes value judgments. *Philos. Sci.* 20, 1–6. doi: 10.1086/287231

Shear, T., and Fitelson, B. (2019). Two approaches to belief revision. *Erkenntnis* 84, 487–518. doi: 10.1007/s10670-017-9968-1

Sismondo, S. (2008). Pharmaceutical company funding and its consequences: a qualitative systematic review. *Contemp. Clin. Trials* 29, 109–113. doi: 10.1016/j.cct.2007.08.001

Sprenger, J., and Hartmann, S. (2019). *Bayesian Philosophy of Science*. Oxford: Oxford University Press.

vom Saal, F. S., and Hughes, C. (2005). An extensive new literature concerning low-dose effects of bisphenol A shows the need for a new risk assessment. *Environ. Health Perspect.* 113, 926–933. doi: 10.1289/ehp.7713

Wilholt, T. (2006). Design rules: industrial research and epistemic merit*. *Philos. Sci.* 73, 66–89. doi: 10.1086/510175

Wilholt, T. (2009). Bias and values in scientific research. *Stud. Hist. Philos. Sci. Part A* 40, 92–101. doi: 10.1016/j.shpsa.2008.12.005

Wilholt, T. (2013). Epistemic trust in science. *Br. J. Philos. Sci.* 64, 233–253. doi: 10.1093/bjps/axs007

Wilholt, T. (2016). "Collaborative research, scientific communities, and the social diffusion of trustworthiness," in *The Epistemic Life of Groups: Essays in the Epistemology of Collectives*, eds M. Brady and M. Fricker (Oxford: Oxford University Press), 218–235.

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

HYPOTHESIS AND THEORY
published: 28 May 2021
doi: 10.3389/frma.2021.600706

What, Me Worry? Research Policy and the Open Embrace of Industry-Academic Relations

Bennett Holman[1,2]*

[1]Yonsei University, Seoul, South Korea, [2]University of Johannesburg, Johannesburg, South Africa

The field of research policy has conducted extensive research on partnerships between industry and academics and concluded that such collaborations are generally beneficial. Such a view stands in stark contrast to the literature in the philosophy of science which almost wholly finds such collaborations corrosive to scientific inquiry. After reviewing the respective literatures, I propose explanations for these polarized views which support the claim that both disciplines have only a partial vantage point on the effects of industry-funded science. In closing, I outline how the research agendas of each discipline might remediate their respective shortcomings.

Keywords: industry-funded science, academic engagement, philosophy of science, policy research, commodification of knowledge, feminist epistemology, regulatory science

OPEN ACCESS

Edited by:
Christoph J Merdes, University of Erlangen Nuremberg, Germany

Reviewed by:
Sharon Crasnow, Norco College, United States
Alexander Christian, Heinrich Heine University of Düsseldorf, Germany

***Correspondence:**
Bennett Holman
bholman@yonsei.ac.kr

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *30 August 2020*
Accepted: *06 May 2021*
Published: *28 May 2021*

Citation:
Holman B (2021) What, Me Worry? Research Policy and the Open Embrace of Industry-Academic Relations. Front. Res. Metr. Anal. 6:600706. doi: 10.3389/frma.2021.600706

INTRODUCTION

It should not be surprising to find that when different academic disciplines study the same topic matter that different aspects of a phenomenon come to the fore, especially when that phenomenon is a complex human institution. Nevertheless, for the scholar immersed in her own way of conceptualizing a phenomenon, it is disorienting to encounter another framework. It is like stepping into a similar but parallel universe in which familiar objects are cast in a different light and aspects of reality which had faded into the background and which had been taken for granted, now come into sharp relief against unexpected absences.

Such is the case in the study of industry-funded science as seen from the vantage points of philosophy of science and from science policy studies. While both disciplines have an extensive literature on the influence of industry-funding on science, they have remained, so far as I can discern, almost completely distinct. To wit, review articles of academic-industry relations summarizing research in science policy (Perkmann et al., 2021) and philosophy of science (Holman and Elliott, 2018) do not share a single common source despite both including over 100 citations. Of course, some of the sources in the former were published after 2018 and could not have been cited in the latter, but this does not explain the absence of the research cited in the philosophy of science review from informing the science policy literature. In short, there really are two largely independent bodies of research.

Accordingly, the primary function of this paper is to begin put these two literatures into contact with one another, to identify areas of overlap, and to suggest how each could draw most fruitfully from the other. I first review the literatures in philosophy of science (*The Perils of Industry Funding in Science*) and research policy (*Moderating Industry Collaborations and Maximizing Scientific Output*). In *Two Worlds*, I confront the drastically different attitudes that each discipline takes towards the influence of industry. I argue that notwithstanding the wealth of scholarship which philosophers of science could profitably draw from, that the science policy literature lacks the fundamental

conceptual resources to gauge the epistemic impact of industry on the scientific process and thus their rosy view of industry-funded science stems from a blind spot rather than a superior vantage point. I conclude by identifying areas of overlap and ways in which research in each discipline may benefit from incorporating the research done in the other.

THE PERILS OF INDUSTRY FUNDING IN SCIENCE

Philosophers of science are standardly interested in the fundamental questions that underpin scientific inquiry. This includes both the central concepts deployed within science (e.g., causation, explanation, etc.,) as well as a concern for the method(s) and overall reliability of a science generally. Though it became better integrated with the history of science over the course of the 20th century, work in philosophy of science generally remained removed from science in practice. In the early 20th century the social epistemology of science has emerged within philosophy of science as an attempt to situate traditional philosophical concerns within a contextualized and grounded account of inquiry (Goldman, 1999; Solomon, 2001; Longino, 2002). Attention to industry funding and science has only begun to attract sustained attention within the past decade.[1]

With some exceptions (e.g., Adam et al., 2006), the primary focus has been on how industry-funded science distorts or corrupts the scientific endeavor. In stark contrast, the science policy literature generally regards the influence of the private sector with something between neutrality and unabashed enthusiasm. In this section and the next, I briefly survey the respective literatures as means of illustrating the difference in foci and to substantiate the claim that there is a stark difference between the way that industry is regarded.

Holman and Elliott (2018) organize the philosophy of science literature schematically according to the ways in which industry can distort various stages of inquiry.[2] At the most fundamental level, industry can shape the concepts scientists work with in ways that predispose inquiry to reach commercially favored outcomes. One prominent manifestation of this in the medical field is disease mongering—or the pathologization of normal human suffering in order to increase the potential commercial applications of drugs (Brown, 2002; Moynihan and Cassels 2005; González-Moreno,et al., 2015). Another means of shaping the communal body of knowledge is to channel research by selectively funding projects with commercially advantageous outcomes and away from establishing facts that would be economically damaging. In so doing, industry is not simply funding one line of research over another, they are actively preventing the scientific community—and thus the general public—from coming to know something which would be in their objective interest to learn. Such active maintenance of ignorance has now formed its own research domain under the label of agnotology (Proctor, 2011; Fernandez Pinto, 2015; 2017).

Even when threatening questions must be asked (for example when they are required to satisfy regulatory approval), industry often works assiduously to make sure that the methods, experimental design, and statistical analysis used to answer those questions yield commercially favorable outcomes (Steele, 2018; Stegenga, 2018). Similarly, a wide latitude exists on how results are discussed, which opens up the door for a considerable degree of rhetorical spin (Biddle, 2007; Matheson, 2008). If all else fails, undesirable results can simply be withheld from publication (McGarity and Wagner, 2008; Jukola, 2015a).

Finally, philosophers of science have contended that to understand the influence of industry on science, that the focus must ultimately move beyond the individual to include the larger social structure within which science operates (Biddle, 2007; Wilholt, 2009; Intemann and de Melo-Martín, 2014; Holman, 2015, 2019). Following the lead of the tobacco Industry, numerous sectors (e.g., petroleum, pharmaceuticals, lead, etc.) have used high level strategies to manipulate scientific knowledge (McGarity and Wagner, 2008; Michaels 2008; Oreskes and Conway, 2010; White and Bero, 2010). Understanding the larger social context is necessary both because some effects only occur at the social level (Holman and Bruner, 2017) and because solutions to active manipulation must consider a full range of how industry would attempt to circumvent reform in order to increase the likelihood that it will be effective (Holman and Geislar, 2018).

MODERATING INDUSTRY COLLABORATIONS AND MAXIMIZING SCIENTIFIC OUTPUT

The discipline of policy research is populated primarily by scholars in either economics departments or business schools.[3] They take their audience to be two-fold. First, it aims to be of practical importance for university managers and government policy makers. Indeed, the size of this audience is growing because of increased pressure from national governments to demonstrate that recipients of research funding make a demonstrable public impact (Sá et al., 2013). In addition, numerous nonprofit and

[1]In this section, I am only referring to philosophers of science. There have been other scholars including medical journal editors (e.g., Angell, 2004), medical researchers (e.g, Glantz et al., 1998), and environmental policy scholars (e.g., Krimsky, 2004), that have also written on industry funding and science and tend to be critical of it. These contributions could be seen as policy research, though it does not fit the narrow definition used in this paper (see note 3). To some extent, the literature within the philosophy of science is integrated into this larger body of work, but policy research is not. Tracing the histories of this diverse array of scholarship is left as a project for another day.

[2]In using "distort" rather than a more neutral term such as "shape", I am intentionally taking over the value-laden tone of this literature.

[3]I am here using "policy research" in a narrow sense to refer to the discipline as it generally conceives of itself and particularly as the discipline is captured in the summary papers of Perkmann et al. (2013, 2021). There is a broader sense in which many scholars conduct work that is relevant to science policy, including for example, many of the scholars cited in *The Perils of Industry Funding in Science*. My discussion of policy research in this paper is directed at this narrow sense.

government agencies (e.g., the Gates Foundation, The Wellcome Trust, the President's Council of Advisors on Science and Technology, the Food and Drug Administration) are encouraging collaborations between industry and academia (Drazen, 2015). Indeed, over the past decade the entire pharmaceutical industry has restructured a significant portion of their research and development into university-industry partnerships (Robinson, 2019; 2020). On this front research policy is supposed to inform and facilitate successful engagement.

Beyond this, the university is a readily available social system which has a long history of scholarly study (e.g., Merton, 1973). As Perkmann et al. (2021) note, exploring the interface of industry and university research has provided an opportunity for scholars to study norms around information sharing and the violation thereof (Haas and Park, 2010): how do hybrid organizations manage the demands? Are there disparate practices and self-identities of two conflicting social institutions (Sauerman and Stephan, 2013; Perkmann, et al., 2019)? And which factors affect researchers' uptake of new practices involving technology transfers (Bercovitz and Feldman, 2007). On this front, the interface of science and industry is of interest to, and can potentially draw from, a wide range of social scientific frameworks.

Collectively, the reviews by Perkmann et al. (2013, 2021) cover thirty years of research on what they call "academic engagement." Strictly speaking, academic engagement is narrower than industry-funded science. It is meant to encapsulate instances of university researchers interacting with industry (e.g., collaborative research, contract research, consulting). For the moment, it is important to note that academic engagement does not include science conducted exclusively in-house in private corporations, the work of industry-funded think tanks, nor does it include "commercialization" which is designated as the creation of intellectual property or founding a for-profit business from one's academic work. I will return to these distinctions in *Two Worlds* and discuss the extent to which this shapes the respective literatures.

While it is important to foreground that Perkmann et al. (2013, 2021) are considering a narrower range of phenomena, there is a considerable degree of conceptual overlap in the areas of study. Both reviews are primarily focused on what effects industry involvement has on scientific inquiry. Perkmann et al.'s review is organized by describing what factors make a researcher more likely to participate in academic engagement and then shifts to outlining the consequences for academic research and the commercial consequences of academic engagement. Those uninterested in the determinants of engagement may wish to skip to *The Consequences of Engaging with Industry* where I discuss the research on its consequences.

The Determinants of Engaging With Industry

At least in the United Kingdom, men are more likely to participate in academic engagement than women (Abreu and Grinevich, 2013; 2017); however, there were some specific activities (public engagement and informal advice) in which women were more likely than men to engage in (Lawson, et al., 2016). Moreover, when universities had systematic policies to promote women's careers, these differences were significantly reduced (Tartari and Salter, 2015). There are mixed effects on whether older academics are more likely engaged with industry irrespective of whether it is measured by biological age (Tartari and Breschi, 2012; Abreu and Grinevich, 2013; Lawson, et al., 2019; Iorio, et al., 2017) or in years since PhD (Schuelke-Leech, 2013; Acshhoff and Grimpe, 2014; Huyghe and Knockaert, 2015). However, there is a clear positive relation with professional rank obtained (i.e., from research assistant to full professor (Tartari and Breschi, 2012; Abreu and Grinevich, 2013; Lawson, et al., 2019)).

Prior experiences also have an impact on likelihood of engaging with industry. Tartai et al. (2012) find academics that have previously worked outside of academia perceive fewer barriers to academic engagement. Indeed, such researchers are more likely to engage with industry (Abreu and Grinevich, 2013) even if that experience is in the non-profit sector (Gulbrandsen and Thune, 2017). Once an academic has participated in some form of academic engagement, most will do so again (Lawson, et al., 2016).

Other research has focused on the academic profile of those inclined towards collaborating with industry. Such researchers tend to be more prolific publishers (Aschhoff and Grimpe, 2014; D'Este et al., 2019; Ding and Choi, 2011), but are not more likely to publish work of superior quality (Ding and Choi, 2011; Tartari et al., 2014; Zi and Blind 2015). Unsurprisingly, researchers who engage with industry are more likely to publish in applied scientific journals (Tartari and Breschi, 2012; Zi and Blind, 2015).

A researcher's context also had a significant effect. Academics in departments where their colleagues were engaged with industry were more likely to do so themselves (Aschhoff and Grimpe 2014; Tartari et al., 2014). University policies also have an effect. When universities have stricter policies about disclosure of conflicts of interest, researchers are less likely to engage with industry (Halilem et al., 2017). If a university takes a higher percentage for royalties for work done at the institution, researchers tend to shift towards engagement (e.g., consulting) and away from developing their own intellectual property (Halilem et al., 2017).

Finally, in terms of consciously held, individual motivations, policy research breaks up the conceptual terrain into intellectual challenge ("puzzles"), professional recognition ("ribbons"), and personal financial gain ("gold" (Stephan and Levin, 1992; Lam, 2011)). When asked why they engaged with industry, ribbons and puzzles emerged as the primary motivations (Lam, 2011). This finding was refined in German academics, standardization efforts were primarily motivated by a desire to solve puzzles, while patenting was driven by gold (Blind et al., 2018). Yet this framework does not capture the full range of motivations to engage with industry. Italian and Spanish researchers cite obtaining research funding—rather than personal gain—as their primary motivation, though they express concern that such interactions may limit their academic freedom and tarnish their reputation (Tartari and Breschi, 2012; Ramos-Vielba et al., 2016). Iorio et al. (2017) unpack the desire to obtain research funding, finding that it is driven by a desire to benefit society rather than generating new knowledge. This

finding departs from research from the United Kingdom and Denmark which finds that increasing knowledge is a major driver of engagement (Hughes et al., 2016; Kongsted et al., 2017).

The Consequences of Engaging With Industry

In policy research, the consequences of engagement are primarily framed in terms of quality and quantity of subsequent research as measured by the number of publications the author appears on and the ranking of the journal where the articles are published (e.g., as measured by impact factor). By these measures, academic engagement leads researchers to produce both a higher quantity of research (Hottenrott and Lawson, 2017; Garcia et al., 2020) and a higher caliber of research (Hottenrott and Lawson, 2017). Especially large productivity gains are observed when researchers are selective about who they partner with (Callaert et al., 2015). However, other research has suggested factors that modify the effect of engagement on productivity. In particular, Banal-Estañol et al. (2015) found that engagement tended to increase productivity, however, once academics began engaging with industry in more than 30–40% of their projects, productivity decreased because "research ideas may then be of lower value, industry may impose non-disclosure clauses or because extensive collaboration could reduce the time to do research and cause attention problems" (p. 1173). Moreover, it appears that not all forms of engagement (e.g., consulting) increase productivity (Rentocchini et al., 2014).

Beyond publication, receiving funding from industry has been shown to increase secrecy. Researchers who received industry funding were twice as likely to deny requests to share data or other research methods and materials, as well as to delay publication of their findings (Czarnitzki et al., 2015a; Czarnitzki et al., 2015b). Nevertheless, academic engagement has been found to increase researchers' reputation amongst their peers (Hughes, et al., 2016). Perhaps because industry engagement serves as a ribbon, Fini, et al. (2018) find that moderate engagement with industry increases both a researcher's reputation and ability to obtain public grant funding. However, at high levels of engagement they find that a researcher's reputation amongst their peers decreases as they begin to suffer an identity penalty (viz. they start to be viewed as an industry researcher as opposed to an academic researcher who occasionally partners with industry).

In terms of their commercial output, engagement with industry increases a researcher's patent output (Beaudry and Kananian, 2013; Lawson, 2013; Libaers, 2017; cf.; Bikard et al., 2019), though such researchers are also more likely to circumvent the universities technology transfer office (Perkmann et al., 2015; Goel and Göktepe-Hultén, 2018). As with previous findings, the result is curvelineal (e.g., at very high levels engagement decreases patent output). Finally, serving as a company's scientific adviser has been shown to decrease the likelihood of starting one's own company (Fritsch and Krabel, 2012). On these grounds, Perkmann et al. (2021) assure policy makers that "academic engagement is complementary with research, practiced by scientifically productive individuals … and likely to have positive effects on research productivity and other research related performance measures" (p. 4).

TWO WORLDS

I find it difficult to keep in mind that these two literatures are about the same substantive topic (I hope readers now feel this way too). Having summarized both bodies of research, I wish to: (1) suggest a reason why the research policy literature is predominantly positive on academic-industry partnerships (*Upon the Altar of Productivity*); (2) explore why philosophy of science is predominately negative (*The View From Somewhere*); and (3) identify some areas where these literatures might begin to inform one another (Discussion: Synergy or Schism). I wish to be clear that I do not pretend that the explanations I offer are "the" explanations, indeed, I won't even offer the same type of explanation in both cases. Rather, my only contention is that the explanations shed some light on the phenomenon, why each discipline generates a one-sided account of industry academic partnerships, and how their respective research programs might move forward.

Upon the Altar of Productivity

Perhaps the most significant difference between the two accounts is that policy research is a social science. To be clear, both literatures are clearly empirical in some broad sense as the philosophical literature is heavily based on particular episodes of scientific inquiry. Nevertheless, policy research, at least insofar as it is captured by Perkmann et al. (2013, 2021), is fundamentally rooted in quantitative research methods in ways that predispose it to take the functioning of science at face value. While it is clear that there are also likely to be structural reasons why economists and business professors are less critical of academics collaborating with profit-seeking entities than philosophers, I want to focus on the difficulty of detecting the deleterious effect of industry, given the outcome variables policy researchers are inclined to collect.

Consider, for example, a recent high-profile case of academic engagement in the study of remdesivir for the treatment of COVID-19 (Beigel et al., 2020). The trial was primarily funded by the American government, but employees of Gilead Sciences (the manufacturers remdesivir) "participated in discussions about protocol development and in weekly protocol team calls" (p. 11). Moreover, numerous authors on the publication had some form of previous engagement with Gilead (e.g., consulting). The article was published in the *New England Journal of Medicine* one the most elite medical journals in the world and in the first three months since its publication it has garnered over 500 citations. Practically speaking, it instantly changed medical practice worldwide. From a research policy perspective this appears to be a clear triumph.

Yet surely, in some very important sense, this research can be considered a success only if remdesivir is in fact an effective treatment for COVID-19 (or at least if the trial was "fair test" (Evans, et al., 2011)). But this depends on a number of substantive and methodological questions. For example, the trial was stopped early because the results were significant on the primary end

point (time to recovery), was it appropriate to stop the trial at the point when significance had been reached? Did the statistical analyses used properly take into account the fact that interim data was being analyzed with the possibility of terminating the trial? Was the published primary outcome measure appropriate? Was the decision to change the primary outcome measure during the trial appropriate? And so on.

In short, answering questions about the integrity of any particular piece of research is going to require a considerable amount of time. Current studies on research output take as data hundreds or thousands of researchers each with tens or hundreds of articles. There is no clear practicable way to exercise anything close to the level of scrutiny that would seem to be required to independently assess each article of each author. Even if there were, doing so would require a considerable amount of expert knowledge, which even if one had in some particular domain, would be required in every academic domain and sub-domain under study.

Using quantified outcome variables makes it possible for a reasonably small group of researchers to assess wide swathes of academia without needing to understand the content of the subject field that they are studying. Moreover, their approach to doing so mirrors a logic that is seemingly practical and familiar. For example, when a department seeks to hire a new position, it is often the case that they are hiring someone to fill a gap in the intellectual breadth of the department. That is, the very reason that they are hiring is because the current faculty lack someone with the very expertise that would be needed to independently assess the academic qualifications of the candidates under consideration. In such circumstances, a natural shorthand for assessing candidates is to assume that a publication is a genuine indication that the candidate has contributed to that area of knowledge. Similarly, since most fields have journals with a hierarchical system of prestige, one assumes that an article published in an elite journal is of higher quality than one published in a smaller specialty journals. In short, if you are willing to assume that a discipline is well-functioning, publication record is an accurate, though impoverished, proxy for merit and it is difficult to see how one could reasonably discard this as a simplifying assumption and continue to carry out traditional research policy projects.

Such problems are amplified when it comes to assessing if industry engagement biases the direction of the research agenda. As Inmaculada de Melo-Martin (2019) has pointed out, "it is not clear that there is any such thing as the *epistemically correct* research agenda or the *epistemically appropriate* direction for a research agenda to take" (p. 8, emphasis in original). Indeed, it has long been argued that choices about what research to undertake are underdetermined (e.g., Kuhn, 1962; Lakatos, 1970). Nevertheless, even if there is no uniquely correct course of research, this does not necessarily imply that anything goes. There may well be some research agendas which are objectively deleterious (e.g., the tobacco industry's funding of research that questioned the link between smoking and cancer). Yet the ability to assess each individual's choice in research agenda, because there are even more degrees of freedom than in research design, would be correspondingly more knowledge intensive than scrutinizing the quality of research.

In sum, given the goal to assess a large heterogeneous collection of academic disciplines by researchers who will generally lack the subject area expertise necessary to make independent judgments, the discipline of policy research has coalesced to using easily accessible, quantitatively tractable outcome measures. While these metrics may be crude, in a well-functioning discipline, such metrics might be a reasonable proxy for scientific contribution. Moreover, given the aims of the discipline to serve university managers, these measures of productivity might be the relevant variable to study irrespective of their validity. To the extent that managers aim to burnish the image of their institution and external bodies (e.g., QS world rankings) use these measures to evaluate universities, tracking these metrics may well be instrumentally rational. Nevertheless, the principle of charity would dictate that managers use these metrics not just to manage the university's reputation, but because they trust that the measures accurately reflect genuine scientific contribution. Similarly, while it may be the case that given their disciplinary aims, research policy scholars are simply not interested in detecting distortions in scientific research caused by industry-funding, a more charitable explanation is that their standard outcome variables preclude such questions from being meaningfully raised.

The View From Somewhere

Among academic disciplines that make the scientific process a focus of study, philosophy of science has been a relative late comer in its attention towards industry involvement. I hope to show that a brief genealogy of this development fruitfully contextualizes the philosophical literature. I propose that three intellectual antecedents of this emerging body of work can be found, which account for why philosophy of science has largely focused on the perils of industry funding. The first is through feminist epistemology, the second is a focus on areas of science that intersect with regulatory issues, and the third is through concern with the changing nature of the university as an institution.[4]

At the outset it is worth noting that there is nothing inherent in the philosophical approach that should restrict it to abstracting away from the context in which science is conducted. As Heather Douglas (2014) has argued, philosophy's focus on "the logic of science" is the outcome of a struggle between John Dewey and Bertrand Russell. Russell worried that a focus on utility and application would lead science away from the pure pursuit of knowledge and towards a complicity in the type of destruction that scientists had facilitated over the course of WWI (e.g., gas warfare). Faced with a similar concern, Dewey attributed such evils to a lack of knowledge of what was needed to serve the public good. Indeed, he viewed the very idea of pure science as part of a

[4]With regard to the first two streams. I have separated them out, but philosophers whose primary interest is within one, frequently find themselves writing about the other. Accordingly, they might also be seen as two tributaries of the same stream.

mythology that facilitated scientists' ignorance of their responsibility to consider the social consequences of their work.

In part because drawing a sharp distinction between science and values forestalled Marxist critiques of scientific inquiry, Russell's focus on a pure logic of science dominated American and British philosophy of science and ultimately set the agenda for the next 50 years. As a result, mid-century philosophers of science focused on disembodied questions such as the logic of causation, what constitutes an explanation, and the nature of scientific mechanisms (for example consider anthologies (Curd and Cover, 1998; Boyd et al., 1991, etc..). The philosophical debates on these topics grew removed from actual practice.

For example, consider Bas Van Fraassen (1980) fable of the "Tower and the Shadow" in the context of debates surrounding Hempel's DN model of explanation (Hempel and Oppenheim, 1948). In essence, the DN model considers a scientific explanation as a derivation of an observation from a set of initial conditions and the laws of the relevant science. A common type of objection is that it seems that while the height of a tower could be predicted from the laws of optics and the length of a shadow, the length of a shadow does not explain the height of the tower (Bromberger, 1960). Van Fraassen responds to this objection in his pragmatic account of explanation, arguing that whether it is an explanation depends on contextual factors. In the Tower and the Shadow parable, Van Frassen considers a case where a tower was built to cast a shadow in a particular place at a particular time. Van Frassen (1980) claims that the length of the shadow plus the laws of optics would be satisfactory explanation of the height of the tower in this case. Clearly, such an argument is not grounded on an in-depth study of explanation in the scientific literature.

Even amongst philosophers most immersed in the practice of science, the economics of its practice was nowhere to be found. For example, Karl Popper advocated for an understanding of the historical canon that situated philosophers in their larger societal and historical contexts. Specifically, he argued that it is necessary to study the history of science and mathematics because traditional philosophical problems arise out of "urgent and concrete problems, problems which they found could not be dismissed" (Popper, 1963, p. 73). According to Popper's account, Plato's philosophy stems from wrestling with the irrationality of the square root of two from within a Pythagorean framework (that asserted essence of reality is numerical) and Kant's *Critique of Pure Reason* is an attempt to understand how it was possible for Newton to have discovered the truths of physics. What they show, Popper argues, is that "*genuine philosophical problems are always rooted in urgent problems outside of philosophy* . . . What matters is not methods or techniques, but a sensitivity to problems and a consuming passion for them; or as the Greeks said, the gift of wonder" (Popper, 1963, p. 72, italics in original). Yet when it came to his conceptualization of science, Popper (1970) regarded it as "subjectless" and nothing but a "system of theories." Even Kuhn (1962, p. x), whose philosophy of science was richly informed by the history of science, noted the partiality of the view he offered in *The Structure of Scientific Revolution*: "More important, except in occasional brief asides, I have said nothing about the role of technological advance or of external social, economic, and intellectual conditions in the development of the sciences." It is only recently that the influence of industry on science has found a comfortable home within philosophical discourse.

The Feminist Critique

Feminist epistemology of science is the first stream which informs the philosophical discussion. While there are numerous aspects to this body of work, one central theme is that sexist values are subtly—or not so subtly—influencing scientific research. For example, consider the study of primate sexual behavior in langurs by Sarah Blaffer Hrdy. In langur troops, there are periodic bouts of infanticide and Hrdy's earlier work established that they were evolutionarily rational. Such work extended the Bateman-Trivers paradigm which argued that the sex which physically cared for the offspring most would be a site of resource competition for the other sex. Accordingly, Hrdy (1974) showed that when a new male arrives from outside the troop, killing the troop's infants is evolutionarily rational because it brings their mothers into estrus sooner and increases the male's reproductive fitness.

However, inspired by the contemporary feminist movement, Hrdy began to see this account as only half the story. The behavior is evolutionarily rational for the new invading male, but it is clearly not rational for the troop's females to have their infants killed every 2–3 years, so why did female langurs seem to put up with such behavior? This question led to many others and forced her to reconceptualize old observations. For example, it suggested an explanation for why pregnant langurs would solicit sexual pairings with males outside their troop, an otherwise costly behavior with no obvious reproductive benefit (Hrdy, 1977). It also suggested other meta-scientific questions, such as why females were seen as coy (sexually discriminating) despite the fact that they were actually engaging in a significant amount of sexually promiscuous non-monogamous behavior (Hrdy, 1986/2006).

The answer to the former question is of primary interest to evolutionary biology, the latter question is of primary interest to feminist epistemology. Hrdy's work was seen by feminists (and by evolutionary biologists as well) as epistemically superior to the work that preceded it. Yet divisions emerge among feminist epistemologists as to what accounts for the superiority.

Feminist empiricists (e.g., Longino, 1990, 2002; Solomon, 2001) have argued that for an account for the superiority of such knowledge one has to analyze the social structure of science. For example, on Longino's account objective knowledge arises from a properly structured society of diverse inquirers. When a scientific group is homogenous, their values—and the way that those values influence inquiry—go unexamined. In the case of Hrdy, we can see how the Victorian ideal of a sexually chaste female choosing amongst her suitors is replicated in the Bateman-Trivers paradigm. When considering her own early work Hrdy describes how the existing biases within the field shaped her understanding of mating behaviors and produced a dearth of scholarship on female mating strategies: "because theoretically the phenomenon [female promiscuity] should not have existed and therefore there was little theoretical infrastructure for studying it, certainly not the sort of study that could lead to a

PhD (or a job)" (p. 135). On Longino's account, what occurs with the introduction of Hrdy (and other feminists) into the field is that Victorian values are questioned and the ways in which they bias research are exposed and corrected. Objectivity emerges socially out of a clash of subjectivities.

An alternative account of the superiority of Hrdy's work arises from the work of standpoint theorists (e.g., Collins, 1990; Harding, 1991). On such accounts it is crucially important that Hrdy is a feminist in a patriarchal society. According to standpoint theories, social location systematically influences knowledge production and knowledge systems tend to embed the interests of dominant groups. Because their interests diverge, subjugated groups often have their own standpoint from which to understand the relevant phenomenon. On some accounts (e.g., Hartsock, 1983), power differentials produce subjugated groups with a privileged epistemic position. This occurs because subjugated groups must often be conversant in both their understanding of a phenomenon and the understanding of the dominate groups. Conversely, the dominant group can safely remain ignorant of the subjugated group's understanding. On other accounts (e.g., Wylie, 2003; Harding, 2004), an epistemically superior standpoint is differentiated from a person's individual experiences. A standpoint is not simply the experiences of someone from a marginalized group, but is rather a group achievement that arises from the attainment of a "critical consciousness"—an awareness of how power structures have influenced the dominant ideology.

If we return to Hrdy, we might note that the Bateman-Trivers paradigm assumes that nearly all females mate and so do not have a significant fitness differential for evolution to act upon. In adopting the perspective of the female as a site of evolutionary action, Hrdy was breaking significant ground and the fact that she did so was not incidental to her social location and interaction with feminist thought:

> In my own case, changes in the way I looked at female langurs were linked to a dawning awareness of male–female power relationships in my own life, though "dawning" perhaps overstates the case ... Each step in understanding what, for example, might be meant by a term like androcentric was embarked upon very slowly and dimly, sometimes resentfully, as some savage on the fringe of civilization might awkwardly rediscover the wheel ... Nevertheless, the notion of "solidarity" with other women and, indeed, the possibility that female primates generally might confront shared problems was beginning to stir and to raise explicit questions about male–female relations in the animals I studied. (Hrdy 1986/2006, p. 151)

A standpoint theorist would be inclined to point out that there were women studying primatology prior to Hrdy. What made Hrdy different was her exposure to the developing feminist consciousness.

This is only a brief sketch of a rich branch of feminist scholarship and there have been significant developments as these positions (for updated surveys see Intemann, 2010; Grasswick, 2018), but it suffices for an understanding of why this stream of thought leads to focusing on the negative influences of industry involvement in science. To begin with, industry involvement with science almost necessarily infuses commercial values into scientific inquiry. Given a view of objectivity that requires scientific inquiry to be disinterested, industry involvement is inherently a source of bias. Thus, as Intemann and de Melo-Martin (2014) have argued, for philosophers who are coming to these issues afresh, feminist epistemology "seems particularly well situated to provide resources to help address such concerns because this literature has both 1) theorized about how to minimize biases in science, e.g., sexist or androcentric biases, and 2) generated accounts of objectivity that do not require individual scientists to be value-neutral or disinterested" (p. 135).

However, coming to look at industry-funded science with the tools of feminist epistemology almost necessarily results in focusing on the perils of industry-funded science. Though it is oversimplifying to a degree, a dominant form of a research project in feminist epistemology is to begin with a piece of accepted science, to next demonstrate how such research was distorted by the infusion of sexist values, and to finally use this distortion to probe the functioning of science. This pattern is repeated when Longino's framework is applied to commercial applications. For example, Justin Biddle (2007) takes the Vioxx debacle and argues that it was caused by institutional failures at Merck. Internally, numerous researchers at Merck raised red flags regarding the safety data years prior to its removal, yet Merck publicly maintained that Vioxx was safe. Given Merck's vast ability to shape the scientific literature, Biddle argues that it is implausible to think that adding a final stage of critical discussion, will render objective knowledge.

In all cases that I am aware of this general pattern is repeated: specifically, the philosopher begins with a case where industry influence is seen to be problematic and then applies feminist theories of objectivity to assess whether they are adequate to account for the epistemic failing. Other examples of this pattern include: manufacturing uncertainty regarding the safety risks of commercial products (Borgerson, 2011); the failure to develop a HPV vaccine that can be successfully used in developing nations (de Melo-Martin and Intemann, 2011), the distortion of the science on the health effects of second-hand smoke (Fernandez Pinto, 2014), distortion of the reliability of anthropogenic climate change (Fernandez Pinto, 2014; Rolin, 2017); a distortion in the agenda of medical research away from illnesses of the poor (Intemann and de Melo-Martin, 2014); the downplaying of the risks that SSRIs induce suicide (Jukola, 2015a); the manipulation of the FDA in the approval of flibanserin for "hypoactive sexual desire disorder" in women (Holman and Geislar, 2018; Bueter and Jukola, 2020).

To be clear, I am neither disputing any of the particular conclusions of the research cited above nor taking issue with the line of research more generally. The point is simply this: logically speaking the framework supplied by feminist epistemology could be used in the analysis of the successful generation of knowledge with industry-academic collaboration, but it never is. A reason for this one-sided focus on the perils of

industry funding is that the framework of feminist epistemology is dispositionally critical of existing power structures. This is particularly true of standpoint epistemologies which "require adopting a normative commitment to examining scientific phenomena in ways that challenge, rather than reinforce, systems of oppression" (Intemann and de Melo-Martin, 2014, p. 144). Such a project necessarily focusses on where power corrupts rather than where it refines.

The Weaponization of Science

A second intellectual stream by which philosophers of science have come to study the role of industry-funding is by studying what might be called regulatory science. Starting with medical research and food safety in the early 20th century and subsequently with environmental and safety regulations in the mid-twentieth century, national governments attempted to use science to inform public policy and to control and regulate corporate actors.

Though she is less frequently cited amongst philosophers, Kristen Shrader-Frechette is an early and influential example of such a philosopher. Shrader-Frachette's works include critiques of scientific technology assessment Shrader-Frechette (1980), environmental regulation Shrader-Frechette (1982), risk assessment Shrader-Frechette (1988), and nuclear waste Shrader-Frechette (1993) to name just few representative examples of an voluminous body of work that is primary (and atypically) aimed outside of the philosophical discourse.[5] More proximally influential has been the work of Naomi Oreskes and Eric Conway's historical work on how a small group of industry-sponsored scientists were able to derail or delay significant legislation of smoking, second-hand smoke, acid rain, ozone depletion, and global warming. A similar vein of research emerges from the study of the Tobacco Industry and agnotology—the intentional production of ignorance (Proctor 1995; 2008; 2011; Fernandez Pinto 2015; 2017). Finally, medical research and particularly the manipulation of scientific research by the pharmaceutical industry has been a similar entry point for a number of philosophers now working on the topic (Brown, 2002, 2004, 2008; Biddle, 2007; Jukola, 2015a, 2015b, 2017; Holman, 2015, 2019; Holman and Bruner, 2015; Stegenga, 2018).

What is of crucial importance is that what has drawn philosopher's attention has been only the portion of industry-funded science that interface with regulatory spheres where science is designed to constrain otherwise profitable activity. Indeed, often times profit-seeking entities are asked to conduct scientific studies on their own products. This creates an incentive to satisfy the letter of the law with regards to producing scientific evidence, while conducting studies which produce results which are systematically skewed in favor of their commercial interests. Simultaneously, regulators (and other reformers) are incentivized to revise governmental structure and scientific methodology in ways that prevent or remediate distortions introduced by the regulated entity. Because these pressures remain in place on both parties and because profit incentives and truth frequently diverge, science becomes the battleground for an asymmetric arms race between the two parties (Holman, 2015; Holman and Geislar, 2018).

Yet while the regulatory sphere creates incentives for industry to distort science, there are several other domains where, at least on a prima facie basis, it seems that market incentives and reliable science align. For example, the oil and gas industry has funded a significant amount of scientific research that it uses for exploration. Similarly, the pharmaceutical industry conducts or funds a large amount of R&D work in the process of drug development that is not immediately concerned with regulatory approval or promotion. Yet with a few exceptions (e.g., Adam, 2005), this has garnered almost no attention from philosophers of science. Accordingly, this selective focus on some industry science but not others has left philosophers with a distorted view of the whole.

The Epochal Break

A final, though less central vein has brought a third group of philosophers to study the role of industry on science. Though it is less central, it is worth mentioning here because it is a direct response to the very administrators who are the consumers of research policy scholarship. As Willem Halffman and Hans Radder (2015) write in *The Academic Manifesto: From an Occupied University*:

> The university has been occupied . . . [by] a mercenary army of professional administrators, armed with spreadsheets, output indicators, and audit procedures . . . the scientific publication system is now all but broken: it is caving in under an endless stream of worthless publications, edited papers posing as republications 'for a different audience', strategic citations, and opportunistic or commercial journals: an exponentially growing stream of output, hardly ever read. You do not further your career in this publication factory (Halffman and Leydesdorff, 2010; Abma, 2013) by *reading* all these papers, but rather by *writing* as many as possible, or at least by adding your name to them—and finding this absolutely normal (p. 165ff).

The primary concern is that the very nature of the university is changing in a self-reinforcing cycle with the nature of science from a mission of public service to a mission for private enrichment. Such concerns are variously expressed as both concerns for the university itself and for character of science conducted within it (Krimsky, 2004; Carrier, Howard, and Kourany, 2008 [esp. part 3]; Carrier and Nordmann, 2011; Nordmann et al., 2011; Radder, 2010; 2019). On this view the norms of the business world are inherently corrosive to the mission of academic scholarship. Again, it is no surprise that

[5]It should be noted that a group of practically oriented philosophers grew up around Notre Dame including Don Howard and Janet Kourany. This group collaborated with Martin Carrier at Bielefeld. Within the field, a number of the scholars now working on the topic studied emerged from Notre Dame (Kevin Elliott, Justin Biddle, Manuela Fernandez Pinto, and Chris ChoGlueck) or Bielefeld (Torsten Wilholt).

scholars brought to the topic from this approach tend to focus on the drawbacks of industry-funded science generally and industry-academic partnerships in particular.

DISCUSSION: SYNERGY OR SCHISM

In the course of this paper I have summarized two very different bodies of literature on the effects of industry-funding on scientific research. I have argued that each of the bodies of research remains partial, and as such has no claim as a definitive assessment of the full scope of the phenomenon. In closing, I wish to sketch out how the two bodies of work might be fruitfully integrated.

First, while it may first appear that the two literatures are bereft of overlap, there are some potential points of contact. The first is the effect of patents on scholarship and the changes brought about to encourage technology transfers from the university to the private sector. The research policy literature find that researchers who are involved in patenting publish both more and a higher quality of research (Agrawal and Henderson 2002; Azoulay et al., 2007; Breschi et al., 2007; Fabrizo and Di Minin, 2008). Moreover, researchers report a perception that patenting increases their professional reputation (Owen-Smith and Powell, 2001; Moutinho et al., 2007; van Rijnsoever et al., 2008). In contrast, philosophers have argued that patenting can produce "thickets" which prohibit the rest of the community from engaging in work in the area (Sterckx 2010; Biddle, 2014). These are not inherently contradictory, it is conceivable, for example, that patents allow a small handful of researchers to corner some area of study allowing them to produce significant research to the detriment of the remainder of the scientific community. Exploring this and contrasting views on legislation like the Bayh-Dole act seems like a particularly fruitful area for further work (representative philosophical work includes Irzık, 2007; Brown, 2008; Biddle, 2011; Sterckx 2011; Biddle, 2015; for representative work from research policy scholars see Thursby and Thursby, 2002; Sampat et al., 2003; Mowery and Sampat, 2004; Powers and McDougall, 2005).

Beyond areas where the literatures could be immediately integrated, each body of scholarship poses a direct challenge to the other. Accordingly, it should be productive for each literature to apply its methods to cases that run against the general dispositions of the discipline. Specifically, with regard to policy research, we might ask how its standard outcome measures function in an area of science that we currently have good reason to think was epistemically defective.

For example, we have in-depth accounts of numerous drug disasters, such as the development and promotion of anti-arrhythmic drugs (Moore, 1995). In broad strokes, we know that a group of researchers collaborating with industry was able to produce research that anti-arrhythmic medications were safe in the face of a group of more senior researchers who raised serious concerns about drug safety (Holman, 2019). This occurred in large part because industry actively sought out scholars whose research methods would best portray their products and funded their research (Holman and Bruner, 2017) while actively blacklisting researchers who raised concerns (Moore, 1995).

Importantly, in this case, not only were the drugs dangerous, researchers were in a position to know they were dangerous and had it not been for the influence of industry, it is very likely that such drugs would never have been used. Though histories of this period provide the broad strokes of how research unfolded, an elaborated account could be paired with how policy research metrics faired. For example, it might be that the epistemically inferior studies were published in lower-tier journals and that the high prevalence of usage was due to promotion that occurred outside of medical journals. Alternatively, it could be that industry funding increased both the quantity of publications as well as the venues that those studies were published. The former scenario would be one in which the metrics used by policy researchers correctly gauge high-quality science, while the latter would show a worrisome detachment of productivity metrics from genuine scientific contributions. If we find case after case where researchers who engage with industry succeed on productivity metrics but produce epistemically defective research, then we begin to undermine the claim that such measures track the production of scientific knowledge.

Similarly, revisiting the different streams that brought industry-funding to the attention of philosophers of science shows ways in which some of their efforts might be profitably diverted. Feminist philosophers of science have focused on where the power of industry has distorted scientific inquiry or undermined its integrity, but where financial interests and truth align shouldn't we expect science to be advanced? Surely, the discovery and swift production of COVID-19 vaccines provide an example of the tremendous benefit of industry science generally and in some cases the collaboration of industry-university collaborations (e.g., the partnership between Oxford and AstraZeneca).

By the same token philosophers of science could make detailed case studies of industry-funded science that is not specifically targeted towards regulatory ends. As indicated above, regulatory science provides the vast majority of the cases discussed by philosophers of science (Holman and Elliott, 2018), but barely rates a mention in the survey of industry-academic engagement (Perkmann et al., 2021). This strongly suggests that philosophers of science have an overly narrow and unrepresentative sample of the industry-funded science. Moreover, it is likely to be the case that inquiry outside areas of regulatory impact function neither like areas with regulatory impact nor like university-based science. What little work has been done here suggests that science is conducted in ways that are different than university science, but which nevertheless produce genuine scientific advances (Adam et al., 2006).

Finally, for those concerned with the "epochal break" and the changing nature of the university, there would seem to be a new group of scholars receptive to such engagement. Science policy scholars have recently identified "socially engaged" universities as a promising area for future research (Fecher and Friesike, 2014; Grau et al., 2017), but the literatures have evolved in isolation from the discussion in philosophy of science. As elsewhere, science policy scholars have identified areas where philosophers have been overly

gloomy. For example, Perkmann et al. (2019) have investigated purportedly successful cases where the ideals and norms of academic research were shielded from the negative effects of industry engagement. These domains seem like a natural place to begin to integrate and address the concerns expressed by those who advocate for public-interest science (Krimsky, 2004; Carrier et al., 2008 [esp. part 3]; Carrier and Nordman, 2011; Nordmann et al., 2011; Radder, 2010; 2019).

In short, both disciplines have blind spots. In my view, part of the explanation for why research policy scholars are entirely unconcerned about the influence of industry on scientific research is that they lack the tools to detect any problems. The philosophy of science literature suggests there are significant problems to detect if a means of measurement could be devised. Yet the general negativity with which philosophers view the influence of industry stands in stark contrast with views expressed by many who are so engaged. It is possible that such researchers are deluded; however, I have also offered a reason why the research agendas that have brought philosophers of science to study these issues predispose them to focus on the perils of industry-funded science. I expect that the same close attention to the promise of industry-funded science could open up a significant amount of rich intellectual ground and go some way to explain why research policy scholars have such an open embrace of industry-academic relations.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/Supplementary Material, further inquiries can be directed to the corresponding authors.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

REFERENCES

Abma, R. (2013). De publicatiefabriek. Over de betekenis van de affaire-Stapel. *Tijdschrift voor Psychiatrie* 55 (12), 1019–1020.

Abreu, M., and Grinevich, V. (2017). Gender Patterns in Academic Entrepreneurship. *J. Technol. Transf.* 42, 763–794. doi:10.1007/s10961-016-9543-y

Abreu, M., and Grinevich, V. (2013). The Nature of Academic Entrepreneurship in the UK: Widening the Focus on Entrepreneurial Activities. *Res. Policy* 42, 408–422. doi:10.1016/j.respol.2012.10.005

Adam, M., Carrier, M., and Wilholt, T. (2006). How to Serve the Customer and Still Be Truthful: Methodological Characteristics of Applied Research. *Sci. Public Policy* 33, 435–444.

Adam, M. (2005). Integrating Research and Development: The Emergence of Rational Drug Design in the Pharmaceutical Industry. *Stud. Hist. Philos. Sci. C: Stud. Hist. Philos. Biol. Biomed. Sci.* 36 (3), 513–537. doi:10.1016/j.shpsc.2005.07.003

Agrawal, A., and Henderson, R. (2002). Putting Patents in Context: Exploring Knowledge Transfer from MIT. *Manag. Sci.* 48 (1), 44–60. doi:10.1287/mnsc.48.1.44.14279

Angell, M. (2004). *The Truth about Drug Companies*. New York: Random House Press.

A. Nordmann, H. Radder, and G. Schiemann Editors (2011). *Science Transformed?: Debating Claims of an Epochal Break*. Pittsburg: University of Pittsburgh Press.

Aschhoff, B., and Grimpe, C. (2014). Contemporaneous Peer Effects, Career Age and the Industry Involvement of Academics in Biotechnology. *Res. Policy* 43 (2), 367–381. doi:10.1016/j.respol.2013.11.002

Azoulay, P., Ding, W., and Stuart, T. (2007). The Determinants of Faculty Patenting Behavior: Demographics or Opportunities?. *J. Econ. Behav. Organ.* 63 (4), 599–623. doi:10.1016/j.jebo.2006.05.015

Banal-Estañol, A., Jofre-Bonet, M., and Lawson, C. (2015). The Double-Edged Sword of Industry Collaboration: Evidence from Engineering Academics in the UK. *Res. Policy* 44 (6), 1160–1175. doi:10.1016/j.respol.2015.02.006

Beaudry, C., and Kananian, R. (2013). Follow the (Industry) Money - the Impact of Science Networks and Industry-To-University Contracts on Academic Patenting in Nanotechnology and Biotechnology. *Industry and Innovation* 20 (3), 241–260. doi:10.1080/13662716.2013.791125

Beigel, J. H., Tomashek, K. M., Dodd, L. E., Mehta, A. K., Zingman, B. S., Kalil, A. C., et al. (2020). Remdesivir for the Treatment of Covid-19-Preliminary Report. *N. Engl. J. Med.* 383, 1813–1826. doi:10.1056/NEJMoa2007764

Bercovitz, J., and Feldman, M. (2008). Academic Entrepreneurs: Organizational Change at the Individual Level. *Organ. Sci.* 19 (1), 69–89. doi:10.1287/orsc.1070.0295

Biddle, J. (2007). Lessons from the Vioxx Debacle: What the Privatization of Science Can Teach Us about Social Epistemology. *Social Epistemology* 21, 21–39. doi:10.1080/02691720601125472

Biddle, J. (2011). "Bringing the Marketplace into Science: On the Neoliberal Defense of the Commercialization of Scientific Research," In *Science in the Context of Application*. Editors M. Carrier and A. Nordmann (Dordrecht: Springer), 245–269. doi:10.1007/978-90-481-9051-5_15

Biddle, J. B. (2014). Can Patents Prohibit Research? on the Social Epistemology of Patenting and Licensing in Science. *Stud. Hist. Philos. Sci. A.* 45, 14–23. doi:10.1016/j.shpsa.2013.12.001

Biddle, J. (2015). "Intellectual Property in the Biomedical Sciences," in *Routledge Companion to Bioethics*. Editors R. Kukla, J. Arras, and E. Fenton (London: Routledge), 149–161.

Bikard, M., Vakili, K., and Teodoridis, F. (2019). When Collaboration Bridges Institutions: The Impact of university-industry Collaboration on Academic Productivity. *Organ. Sci.* 30 (2), 426–445. doi:10.1287/orsc.2018.1235

Blind, K., Pohlisch, J., and Zi, A. (2018). Publishing, Patenting, and Standardization: Motives and Barriers of Scientists. *Research Policy* 47, 1185–1197.

Borgerson, K. (2011). Amending and Defending Critical Contextual Empiricism. *Eur. J. Philos. Sci.* 1 (3), 435–449. doi:10.1007/s13194-011-0035-5

Breschi, S., Lissoni, F., and Montobbio, F. (2007). The Scientific Productivity of Academic Inventors: New Evidence from Italian Data. *Econ. Innov. Tech.* 16 (2), 101–118. doi:10.1080/10438590600982830

Bromberger, S. (1960). The Concept Of Explanation *(Doctoral Dissertation*. Harvard University Library. Microreproduction Service.

Brown, J. R. (2002). Funding, Objectivity and the Socialization of Medical Research. *Sci. Eng. Ethics.* 8 (3), 295–308. doi:10.1007/s11948-002-0048-8

Brown, J. R. (2004). Money, Method and Medical Research. *Episteme*. 1 (1), 49–59. doi:10.3366/epi.2004.1.1.49

Brown, J. R. (2008). Politics, Method, and Medical Research. *Philos. Sci.* 75 (5), 756–766. doi:10.1086/594521

Bueter, A., and Jukola, S. (2020). "Sex, Drugs, and How to Deal with Criticism: The Case of Flibanserin," In *Uncertainty in Pharmacology*. Editors B. Osimani and A. La Caze (Cham: Springer), 451–470. doi:10.1007/978-3-030-29179-2_20

Callaert, J., Landoni, P., Van Looy, B., and Verganti, R. (2015). Scientific Yield from Collaboration with Industry: The Relevance of Researchers' Strategic Approaches. *Res. Policy* 44 (4), 990–998. doi:10.1016/j.respol.2015.02.003

Carrier, M., and Nordmann, A. (2011). "Science in the Context of Application: Methodological Change, Conceptual Transformation, Cultural Reorientation,"

in *Science in the Context of Application*. Editors M. Carrier and A. Nordmann (Dordrecht: Springer), 1–7. doi:10.1007/978-90-481-9051-5_1

Collins, P. H. (1990). *Black Feminist Thought: Knowledge, Consciousness, and the Politics of Empowerment*. New York: Routledge.

Curd, M., and Cover, J. A. (1998). *Philosophy of Science: The Central Issues*. New York: W. W. Norton.

Czarnitzki, D., Grimpe, C., and Pellens, M. (2015a). Access to Research Inputs: Open Science versus the Entrepreneurial University. *J. Technol. Transf.* 40 (6), 1050–1063. doi:10.1007/s10961-015-9392-0

Czarnitzki, D., Grimpe, C., and Toole, A. A. (2015b). Delay and Secrecy: Does Industry Sponsorship Jeopardize Disclosure of Academic Research?. *Ind. Corporate Change* 24 (1), 251–279. doi:10.1093/icc/dtu011

de Melo-Martín, I. (2019). The Commercialization of the Biomedical Sciences: (Mis) Understanding Bias. *Hist. Philos. Life Sci.* 41 (3), 34. doi:10.1007/s40656-019-0274-x

de Melo-Martín, I., and Intemann, K. (2011). Feminist Resources for Biomedical Research: Lessons from the HPV Vaccines. *Hypatia* 26 (1), 79–101. doi:10.1111/j.1527-2001.2010.01144.x

D'Este, P., Llopis, O., Rentocchini, F., and Yegros, A. (2019). The Relationship between Interdisciplinarity and Distinct Modes of University-industry Interaction. *Res. Policy* 48 (9), 103799. doi:10.1016/j.respol.2019.05.008

Ding, W., and Choi, E. (2011). Divergent Paths to Commercial Science: A Comparison of Scientists' Founding and Advising Activities. *Res. Policy* 40 (1), 69–80. doi:10.1016/j.respol.2010.09.011

Douglas, H. (2014). Pure Science and the Problem of Progress. *Stud. Hist. Philos. Sci. Part A* 46, 55–63. doi:10.1016/j.shpsa.2014.02.001

Drazen, J. M. (2015). Revisiting the Commercial-Industry Interface. *N. Engl. J. Med.* 372, 1853–1854. doi:10.1056/NEJMe1503623

Evans, I., Thornton, H., Chalmers, I., and Glasziou, P. (2011). *Testing Treatments: Better Research for Better Healthcare*. London: Pinter & Martin Publishers.

Fabrizio, K. R., and Di Minin, A. (2008). Commercializing the Laboratory: Faculty Patenting and the Open Science Environment. *Res. Policy* 37 (5), 914–931. doi:10.1016/j.respol.2008.01.010

Fecher, B., and Friesike, S. (2014). "Open Science: One Term, Five Schools of Thought," In *Opening Science*. Editors S. Bartling and S. Friesike (Cham: Springer), 17–47. doi:10.1007/978-3-319-00026-8_2

Fernandez Pinto, M. (2014). Philosophy of Science for Globalized Privatization: Uncovering Some Imitations of Critical Contextual Empiricism. *Stud. Hist. Philos. Sci. Part A* 47, 10–17. doi:10.1016/j.shpsa.2014.03.006

Fernandez Pinto, M. (2015). Tensions in Agnotology: Normativity in the Studies of Commercially-Driven Ignorance. *Soc Stud. Sci.* 45, 294–315. doi:10.1177/0306312714565491

Fernandez Pinto, M. (2017). To Know or Better Not to: The Social Construction of Ignorance in Commercial Research. *Sci. Technol. Stud.* 30, 53–72. doi:10.23987/sts.61030

Fini, R., Jourdan, J., and Perkmann, M. (2018). Social Valuation across Multiple Audiences: The Interplay of Ability and Identity Judgments. *Acad. Manag. J.* 61 (6), 2230–2264. doi:10.5465/amj.2016.0661

Fritsch, M., and Krabel, S. (2012). Ready to Leave the Ivory tower?: Academic Scientists' Appeal to Work in the Private Sector. *J. Technol. Transf.* 37 (3), 271–296. doi:10.1007/s10961-010-9174-7

Garcia, R., Araújo, V., Mascarini, S., Santos, E. G., and Costa, A. R. (2020). How Long-Term University-industry Collaboration Shapes the Academic Productivity of Research Groups. *Innovation* 22 (1), 56–70. doi:10.1080/14479338.2019.1632711

Goel, R. K., and Göktepe-Hultén, D. (2013). Industrial Interactions and Academic Patenting: Evidence from German Scientists. *Econ. Innovation New Techn.* 22 (6), 551–565. doi:10.1080/10438599.2013.776861

Goldman, A. (1999). *Knowledge in a Social World*. New York: Oxford University Press. doi:10.1093/0198238207.001.0001

González-Moreno, M., Saborido, C., and Teira, D. (2015). Disease-mongering through Clinical Trials. *Stud. Hist. Philos. Biol. Biomed. Sci.* 51, 11–18. doi:10.1016/j.shpsc.2015.02.007

Grasswick, H. (2018). Feminist Social Epistemology. Stanford Encyclopedia of Philosophy. Available at: https://plato.stanford.edu/entries/feminist-social-epistemology/ (Accessed May 14, 2021).

Grau, F. X., Goddard, J., Hall, B. L., Hazelkorn, E., and Tandon, R. (2017). *Higher Education in the World 6. Towards a Socially Responsible university: Balancing the Global with the Local*. Girona: Global University Network for Innovation.

Gulbrandsen, M., and Thune, T. (2017). The Effects of Non-academic Work Experience on External Interaction and Research Performance. *J. Technol. Transf.* 42 (4), 795–813. doi:10.1007/s10961-017-9556-1

Haas, M. R., and Park, S. (2010). To Share or Not to Share? Professional Norms, Reference Groups, and Information Withholding Among Life Scientists. *Organ. Sci.* 21 (4), 873–891. doi:10.1287/orsc.1090.0500

Halffman, W., and Leydesdorff, L. (2010). Is Inequality Among Universities Increasing? Gini Coefficients and the Elusive Rise of Elite Universities. *Minerva* 48 (1), 55–72. doi:10.1007/s11024-010-9141-3

Halffman, W., and Radder, H. (2015). The Academic Manifesto: From an Occupied to a Public University. *Minerva* 53 (2), 165–187. doi:10.1007/s11024-015-9270-9

Halilem, N., Amara, N., Olmos-Peñuela, J., and Mohiuddin, M. (2017). "To Own, or Not to Own?" A Multilevel Analysis of Intellectual Property Right Policies' on Academic Entrepreneurship. *Res. Policy* 46 (8), 1479–1489. doi:10.1016/j.respol.2017.07.002

Harding, S. (1991). *Whose Science? Whose Knowledge? Thinking from Women's Lives*. Ithaca: Cornell University Press.

Harding, S. G. (2004). A Socially Relevant Philosophy of Science? Resources from Standpoint Theory's Controversiality. *Hypatia* 19 (1), 25–47. doi:10.1353/hyp.2004.0008

Hartsock, N. (1983). "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism," In *Discovering Reality: Feminist Perspectives on Epistemology, Metaphysics, Methodology, and the Philosophy of Science*. Editors S. Harding and M. Hintikka (Dordrecht: D. Reidel), 283–310.

Hempel, C. G., and Oppenheim, P. (1948). Studies in the Logic of Explanation. *Philos. Sci.* 15 (2), 135–175. doi:10.1086/286983

Holman, B. (2015). *The Fundamental Antagonism: Science and Commerce in Medical Epistemology*. Irvine: University of California. doi:10.22269/151130

Holman, B. (2019). Philosophers on Drugs. *Synthese* 196 (11), 4363–4390. doi:10.1007/s11229-017-1642-2

Holman, B., and Bruner, J. P. (2017). Experimentation by Industrial Selection. *Philos. Sci.* 84, 1008–1019. doi:10.1086/694037

Holman, B., and Bruner, J. P. (2015). The Problem of Intransigently Biased Agents. *Philos. Sci.* 82, 956–968. doi:10.1086/683344

Holman, B., and Elliott, K. (2018). The Promise and Perils of Industry-Funded Science. *Philos. Compass* 13, e12544. doi:10.1111/phc3.1254

Holman, B., and Geislar, S. (2018). Sex Drugs and Corporate Ventriloquism: How to Evaluate Science Policies Intended to Manage Industry-Funded Bias. *Philos. Sci.* 85, 869–881. doi:10.1086/699713

Hottenrott, H., and Lawson, C. (2017). Fishing for Complementarities: Research grants and Research Productivity. *Int. J. Ind. Organ.* 51, 1–38. doi:10.1016/j.ijindorg.2016.12.004

Hrdy, S. B. (1974). Male–male Competition and Infanticide Among the Langurs (Presbytis Entellus) of Abu, Rajasthan. *Folia Primatol. (Basel).* 22, 19–58. doi:10.1159/000155616

Hrdy, S. B. (1977). *The Langurs of Abu: Female and Male Strategies of Reproduction*. Cambridge: Harvard Press, University.

Hrdy, S. B. (1986/2006). "Empathy, Polyandry, and the Myth of the Coy Female," In *Conceptual Issue in Evolutionary Biology*. Editor E. Sober (Boston: MIT Press), 131–159.

Hughes, A., Lawson, C., Kitson, M., Salter, A., Bullock, A., and Hughes, R. (2016). *The Changing State of Knowledge Exchange: UK Academic Interactions with External Organisations 2005-2015*. London: NCUB London. doi:10.1007/978-3-319-31029-9

Huyghe, A., and Knockaert, M. (2015). The Influence of Organizational Culture and Climate on Entrepreneurial Intentions Among Research Scientists. *J. Technol. Transf.* 40 (1), 138–160. doi:10.1007/s10961-014-9333-3

Intemann, K. (2010). 25 Years of Feminist Empiricism and Standpoint Theory: Where Are We Now? *Hypatia* 25 (4), 778–796. doi:10.1111/j.1527-2001.2010.01138.x

Intemann, K., and de Melo-Martín, I. (2014). Addressing Problems in Profit-Driven Research: How Can Feminist Conceptions of Objectivity Help? *Eur. J. Philos. Sci.* 4, 135–151. doi:10.1007/s13194-013-0079-9

Iorio, R., Labory, S., and Rentocchini, F. (2017). The Importance of Pro-social Behaviour for the Breadth and Depth of Knowledge Transfer Activities: An

Analysis of Italian Academic Scientists. *Res. Policy* 46 (2), 497–509. doi:10.1016/j.respol.2016.12.003

Irzık, G. (2007). "Commercialization of Science in a Neoliberal World," In *Reading Karl Polanyi for the Twenty-First century*. Editors A. Bugra and K. Agartan (New York: Palgrave Macmillan), 135–153.

Jukola, S. (2015a). "Longino's Theory of Objectivity and Commercialized Research," In *Empirical Philosophy of Science. Introducing Qualitative Methods into Philosophy of Science*. Editors S. Wagenknecht, N. Nersessian, and H. Andersen (Cham: Springer), 127–143. doi:10.1007/978-3-319-18600-9_7

Jukola, S. (2015b). Meta-analysis, Ideals of Objectivity, and the Reliability of Medical Knowledge. *Sci. Tech. Studies.* 28, 101–121. doi:10.23987/sts.55344

Jukola, S. (2017). On Ideals of Objectivity, Judgments, and Bias in Medical Research - A Comment on Stegenga. *Stud. Hist. Philos. Sci. Part C: Stud. Hist. Philos. Biol. Biomed. Sci.* 62, 35–41. doi:10.1016/j.shpsc.2017.02.001

Kongsted, H. C., Tartari, V., Cannito, D., Norn, M. T., and Wohlert, J. (2017). *University Researchers' Engagement with Industry, the Public Sector and Society: Results from a 2017 Survey of University Researchers in Denmark*. Copenhagen: Copenhagen Business School (CBS) and the Think Tank DE.

Krimsky, S. (2004). *Science in the Private Interest: Has the Lure of Profits Corrupted Biomedical Research?* Lanham: Rowman and Littlefield.

Kuhn, T. (1962). *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Lakatos, I. (1970). "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*. Editors I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press), 91–196. doi:10.1017/cbo9781139171434.009

Lam, A. (2011). What Motivates Academic Scientists to Engage in Research Commercialization: 'Gold', 'Ribbon' or 'Puzzle'? *Res. Policy* 40, 1354–1368. doi:10.1016/j.respol.2011.09.002

Lawson, C. (2013). Academic Patenting: The Importance of Industry Support. *J. Technol. Transf.* 38 (4), 509–535. doi:10.1007/s10961-012-9266-7

Lawson, C., Hughes, A., Salter, A., Kitson, M., Bullock, A., and Hughes, R. (2016). *Knowledge Exchange in UK Universities: Results from a Panel of Academics 2005-2015*. London: NCUB.

Lawson, C., Salter, A., Hughes, A., and Kitson, M. (2019). Citizens of Somewhere: Examining the Geography of Foreign and Native-Born Academics' Engagement with External Actors. *Res. Policy* 48 (3), 759–774. doi:10.1016/j.respol.2018.11.008

Libaers, D. (2017). Time Allocations across Collaborations of Academic Scientists and Their Impact on Efforts to Commercialize Novel Technologies: Is More Always Better?. *R&D Manag.* 47 (2), 180–197. doi:10.1111/radm.12164

Longino, H. E. (1990). *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*. Princeton: Princeton University Press. doi:10.1515/9780691209753

Longino, H. E. (2002). *The Fate of Knowledge*. Princeton: Princeton University Press. doi:10.1515/9780691187013

Matheson, A. (2008). Corporate Science and the Husbandry of Scientific and Medical Knowledge by the Pharmaceutical Industry. *BioSocieties* 3, 355–382. doi:10.1017/s1745855208006297

Carrier, M, Howard, D, and Kourany, J (2008). *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*. (Pittsburg: University of Pittsburgh Press).

McGarity, T., and Wagner, W. (2008). *Bending Science: How Special Interests Corrupt Public Health Research*. Cambridge: Harvard University Press.

Merton, R. K. (1973). *The Sociology of Science: Theoretical and Empirical Investigations*. Chicago: University of Chicago Press.

Michaels, D. (2008). *Doubt Is Their Product: How Industry's Assault on Science Threatens Your Health*. New York: Oxford University Press.

Moore, T. (1995). *Deadly Medicines: Why Tens of Thousands of Heart Patients Died in America's Worst Drug Disaster*. New York: Simon & Schuster.

Moutinho, P. S. F., Fontes, M., and Godinho, M. M. (2007). Do Individual Factors Matter? A Survey of Scientists' Patenting in Portuguese Public Research Organisations. *Scientometrics* 70 (2), 355–377. doi:10.1007/s11192-007-0207-4

Mowery, D. C., and Sampat, B. N. (2004). The Bayh-Dole Act of 1980 and University-industry Technology Transfer: A Model for Other OECD Governments?. *The J. Technology Transfer* 30 (1), 115–127. doi:10.1007/s10961-004-4361-z

Moynihan, R., and Cassels, A. (2005). *Selling Sickness: How the World's Biggest Pharmaceutical Companies Are Turning Us All into Patients*. New York: Perseus Books.

Oreskes, N., and Conway, E. (2010). *Merchants of Doubt*. New York: Bloomsbury Press.

Owen-Smith, J., and Powell, W. W. (2001). To Patent or Not: Faculty Decisions and Institutional success at Technology Transfer. *J. Tech. Transfer* 26 (1-2), 99–114. doi:10.1023/a:1007892413701

Perkmann, M., Fini, R., Ross, J.-M., Salter, A., Silvestri, C., and Tartari, V. (2015). Accounting for Universities' Impact: Using Augmented Data to Measure Academic Engagement and Commercialization by Academic Scientists. *Res. Eval.* 24 (4), 380–391. doi:10.1093/reseval/rvv020

Perkmann, M., McKelvey, M., and Phillips, N. (2019). Protecting Scientists from Gordon Gekko: How Organizations Use Hybrid Spaces to Engage with Multiple Institutional Logics. *Organ. Sci.* 30 (2), 298–318. doi:10.1287/orsc.2018.1228

Perkmann, M., Salandra, R., Tartari, V., McKelvey, M., and Hughes, A. (2021). Academic Engagement: A Review of the Literature 2011-2019. *Res. Policy* 50 (1), 104114. doi:10.1016/j.respol.2020.104114

Perkmann, M., Tartari, V., McKelvey, M., Autio, E., Broström, A., D'Este, P., et al. (2013). Academic Engagement and Commercialisation: A Review of the Literature on University-industry Relations. *Res. Policy* 42, 423–442. doi:10.1016/j.respol.2012.09.007

Popper, K. (1963). *Conjectures and Refutations*. London: Routledge and Keagan Paul Ltd.

Popper, K. (1970). "Normal Science and its Dangers," In *Criticism and the Growth Knowledge*. Editors I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press), 51–58.

Powers, J. B., and McDougall, P. P. (2005). University Start-Up Formation and Technology Licensing with Firms that Go Public: a Resource-Based View of Academic Entrepreneurship. *J. Business Venturing* 20 (3), 291–311. doi:10.1016/j.jbusvent.2003.12.008

Proctor, R. (1995). *Cancer Wars: How Politics Shapes what We Know and Don't Know about Cancer*. New York: Basic Books.

Proctor, R. (2011). *Golden Holocaust: Origins of the Cigarette Catastrophe and the Case for Abolition*. Berkeley: University of California Press.

Proctor, R. N. (2008). "Agnotology: A Missing Term to Describe the Cultural Production of Ignorance (And its Study)," In *Agnotology: The Making and Unmaking of Ignorance*. Editors R. Proctor and L. Schiebinger (Stanford: Stanford University Press), 1–33.

Radder, H. (2010). *The Commodification of Academic Research Science and the Modern University*. Pittsburgh: University of Pittsburgh Press.

Radder, H. (2019). From Commodification to the Common Good: Reconstructing Science, Technology, and Society. Pittsburgh: University of Pittsburgh Press. doi:10.2307/j.ctvnb7qrh

Ramos-Vielba, I., Sánchez-Barrioluengo, M., and Woolley, R. (2016). Scientific Research Groups' Cooperation with Firms and Government Agencies: Motivations and Barriers. *J. Technol. Transf.* 41 (3), 558–585. doi:10.1007/s10961-015-9429-4

R. Boyd, P. Gasper, and J. D. Trout Editors (1991). *The Philosophy of Science*. Cambridge: MIT Press.

Rentocchini, F., D'Este, P., Manjarrés-Henríquez, L., and Grimaldi, R. (2014). The Relationship between Academic Consulting and Research Performance: Evidence from Five Spanish Universities. *Int. J. Ind. Organ.* 32, 70–83. doi:10.1016/j.ijindorg.2013.11.001

Robinson, M. D. (2019). Financializing Epistemic Norms in Contemporary Biomedical Innovation. *Synthese* 196 (11), 4391–4407. doi:10.1287/30601916-8c5a-4077-9e01-1e020c273d0f

Robinson, M. (2020). Why Translational Medicine Is, in Fact, "New," Why This Matters, and the Limits of Apredominantly Epistemic Historiography. *Hist. Philos. Life Sci.* 42 (3), 1–22. doi:10.1007/s40656-020-00324-z

Rolin, K. (2017). Scientific Dissent and a Fair Distribution of Epistemic Responsibility. *Public Aff. Q.* 31 (3), 209–230. doi:10.1080/02691728.2017.1346722

Sá, C. M., Kretz, A., and Sigurdson, K. (2013). Accountability, Performance Assessment, and Evaluation: Policy Pressures and Responses from Research Councils. *Res. Eval.* 22 (2), 105–117. doi:10.1093/reseval/rvs041

S. A. Glantz, L. A. Bero, J. Slade, D. E. Barnes, and P. Hanauer Editors (1998). *The Cigarette Papers*. Berkeley: University of California Press.

Sampat, B. N., Mowery, D. C., and Ziedonis, A. A. (2003). Changes in University Patent Quality after the Bayh-Dole Act: A Re-examination. *Int. J. Ind. Organ.* 21 (9), 1371–1390. doi:10.1016/s0167-7187(03)00087-0

Sauermann, H., and Stephan, P. (2013). Conflicting Logics? A Multidimensional View of Industrial and Academic Science. *Organ. Sci.* 24 (3), 889–909. doi:10.1287/orsc.1120.0769

Schuelke-Leech, B.-A. (2013). Resources and Research: An Empirical Study of the Influence of Departmental Research Resources on Individual STEM Researchers Involvement with Industry. *Res. Policy.* 42 (9), 1667–1678. doi:10.1016/j.respol.2013.06.010

Shrader-Frechette, K. (1988). Producer Risk, Consumer Risk, and Assessing Technological Impacts. *Impact Assess.* 6 (3-4), 155–164. doi:10.1080/07349165.1988.9725656

Shrader-Frechette, K. S. (1993). *Burying Uncertainty: Risk and the Case against Geological Disposal of Nuclear Waste*. Berkeley: University of California Press. doi:10.1525/9780520913967

Shrader-Frechette, K. S. (1982). Environmental Impact Assessment and the Fallacy of Unfinished Business. *Environ. Ethics* 4 (1), 37–47. doi:10.5840/enviroethics19824138

Shrader-Frechette, K. S. (1980). Technology Assessment as Applied Philosophy of Science. *Sci. Technol. Hum. Values* 5 (4), 33–50. doi:10.1177/016224398000500404

Solomon, M. (2001). *Social Empiricism*. Cambridge: MIT Press. doi:10.7551/mitpress/6296.001.0001

Steele, D. (2018). If the Facts Were Not Untruths, Their Implications Were: Sponsorship Bias and the Art of Misleading Communication. *Kennedy Inst. Ethics J.* 28 (2), 119–144. doi:10.1353/ken.2018.0011

Stegenga, J. (2018). *Medical Nihilism*. Oxford: Oxford University Press. doi:10.1093/oso/9780198747048.001.0001

Stephan, P. E., and Levin, S. G. (1992). *Striking the Mother Lode in Science: The Importance of Age, Place, and Time*. Oxford: Oxford University Press.

Sterckx, S. (2010). "Knowledge Transfer from Academia to Industry through Patenting and Licensing: Rhetoric and Reality," In *The Commodification of Academic Research: Science and the Modern University*. Editor H. Radder (Pittsburgh: Pittsburgh University Press), 44–64.

Sterckx, S. (2011). Patenting and Licensing of university Research: Promoting Innovation or Undermining Academic Values?. *Sci. Eng. Ethics* 17 (1), 45–64. doi:10.1007/s11948-009-9168-8

Tartari, V., and Breschi, S. (2012). Set Them Free: Scientists' Evaluations of the Benefits and Costs of University-industry Research Collaboration. *Ind. Corporate Change* 21 (5), 1117–1147. doi:10.1093/icc/dts004

Tartari, V., Perkmann, M., and Salter, A. (2014). In Good Company: The Influence of Peers on Industry Engagement by Academic Scientists. *Res. Policy.* 43 (7), 1189–1203. doi:10.1016/j.respol.2014.02.003

Tartari, V., Salter, A., and D'Este, P. (2012). Crossing the Rubicon: Exploring the Factors that Shape Academics' Perceptions of the Barriers to Working with Industry. *Cambridge J. Econ.* 36 (3), 655–677. doi:10.1093/cje/bes007

Tartari, V., and Salter, A. (2015). The Engagement Gap: Exploring Gender Differences in University-industry Collaboration activities. *Res. Policy.* 44, 1176–1191. doi:10.1016/j.respol.2015.01.014

Thursby, J. G., and Thursby, M. C. (2002). Who Is Selling the Ivory tower? Sources of Growth in University Licensing. *Manag. Sci.* 48 (1), 90–104. doi:10.1287/mnsc.48.1.90.14271

Van Fraassen, B. C. (1980). *The Scientific Image*. Oxford: Oxford University Press. doi:10.1093/0198244274.001.0001

van Rijnsoever, F. J., Hessels, L. K., and Vandeberg, R. L. J. (2008). A Resource-Based View on the Interactions of University Researchers. *Res. Policy* 37 (8), 1255–1266. doi:10.1016/j.respol.2008.04.020

White, J., and Bero, L. (2010). Corporate Manipulation of Research: Strategies Are Similar Across Five Industries. *Stanford L. Policy Rev.* 21, 105.

Wilholt, T. (2009). Bias and Values in Scientific Research. *Stud. Hist. Philos. Sci. Part A* 40, 92–101. doi:10.1016/j.shpsa.2008.12.005

Wylie, A. (2003). "Why Standpoint Matters," In *Science and Other Cultures:Issues in Philosophies of Science and Technology*. Editors R. Figueroa and S. Harding (New York: Routledge), 26–48.

Zi, A., and Blind, K. (2015). Researchers' Participation in Standardisation: A Case Study from a Public Research Institute in Germany. *J. Technol. Transf.* 40 (2), 346–360. doi:10.1007/s10961-014-9370-y

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

ORIGINAL RESEARCH
published: 05 July 2021
doi: 10.3389/frma.2021.615357

Novel Reflections on the Autonomy and Responsibility of Science

Fabian Hempel[1,2]*

[1]Institute of Sociology and Economics, Department of Social Sciences and Public Affairs, Bundeswehr University Munich, Munich, Germany, [2]Working Group on Sociological Theory, Social Differentiation and Governance, Department of Theoretical and Normative Foundations, SOCIUM Research Center on Inequality and Social Policy, University of Bremen, Bremen, Germany

This paper explores how cultural understandings of the autonomy and responsibility of science in modern society are manifested in two contemporary science novels about research misconduct in biomedical research. In doing so, it looks at several facets of the societal impact of and on public and private biomedical research, especially with respect to changing authority relations and their epistemic and institutional consequences. The analysis focuses on the multi-layered ways in which social and epistemic interests are treated in Allegra Goodman's *Intuition* and Jennifer Rohn's *The Honest Look*. Goodman's novel demonstrates how, intensified by the economization of science, internal cultural and institutional aspects of the scientific field enable social configurations that, among others, encourage scientific malpractice and lead to the delay of research projects epistemically and socially worth pursuing. In contrast, Rohn's novel exemplifies the corrosion of the ideal scientific ethos by profit-driven practices in private-sector biomedical sciences. The concluding discussion juxtaposes these findings with pertinent contemporary phenomena in modern science systems to provide a more substantial understanding of the interpenetration between science and other social spheres.

Keywords: autonomy of science, responsibility of science, science in fiction, science in society, modern society, sociology of science, sociology of literature, modernity

OPEN ACCESS

Edited by:
Julia Böttcher, University of Erlangen Nuremberg, Germany

Reviewed by:
Karin Hoepker, Friedrich Alexander University Erlangen-Nuremberg, Germany
Sebastian Büttner, Freie Universität Berlin, Germany

***Correspondence:**
Fabian Hempel
Fabian.Hempel@uni-bremen.de

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *08 October 2020*
Accepted: *24 June 2021*
Published: *05 July 2021*

Citation:
Hempel F (2021) Novel Reflections on the Autonomy and Responsibility of Science. Front. Res. Metr. Anal. 6:615357. doi: 10.3389/frma.2021.615357

INTRODUCTION

The social institutions of science are inherently linked to the concept of modernity, as it is the advanced functional differentiation of modern societies that has enabled the development of science into a distinct social subsystem. While all types of social communities can and do spawn institutions that generate theoretical and practical knowledge, modern science and the other institutions of modernity demonstrate a particular sort of co-production (Jasanoff, 2004: 2–3). The social sciences view science and modernity as culturally and structurally embedded in each other, and society at large has considered them the paragons of ambivalent progress–at least from the European Enlightenment to the present era of climate change and pandemics (Wagner, 2016: 23; Renn, 2020: 12). At the same time, they have also been faulted with generating many contemporary environmental hazards and social risks (Beck, 1992: 163; Collins and Pinch, 1993: 1–3).

The organization and practice of scientific research is thus a key element of contemporary knowledge production that both structures and is structured by modern society. Nevertheless, the idea that the institutional and epistemic autonomy of science is a functional imperative has become a foundational notion, and it is supported by the scientific community's own ideal of science as an effectively democratic, self-correcting system in which the validity and worth of epistemic assertions are based solely on clear intellectual criteria (Merton, 1973; United States Congress, 1981). This ideal

of science is often accompanied by the pragmatic and more realistic acknowledgment that science indeed has a social purpose beyond that of certifying knowledge and requires resources from the society at large if it is to operate in a socially beneficial and responsible manner. Scholars have traced the increasing prevalence of partisan societal, political, and economic considerations in the contemporary governance, organization, and practice of science. These include the focus and sources of research funding and economic outcomes of scientific research (Resnik, 2009; Hackett, 2014). These phenomena would seem to contradict the foundational notion of science as an autonomous system with its own field-specific values, organizational modes, and reward structures.

In this paper, I consider the implications of these developments and the cogency of the ideal model of science open to inquiry. Employing the realist science novels *Intuition* (Goodman, 2010) and *The Honest Look* (Rohn, 2010b) as tools for sociological inquiry, I explore cultural understandings of the autonomy and social responsibility of science, with a focus on biomedical research and the epistemic and institutional consequences of changing authority relationships. It is the methodological starting point of this paper that literary imaginations are the product of cultural perceptions and that science novels may convey conceptual insights by centering their stories on social actors and constellation, and on interactions both within and beyond the institutions of science (Engelhardt and Hoydis, 2019; Gaines et al., 2021).

The next sections provide a theoretical background and an overview of discourse on scientific autonomy and responsibility; establish a methodological foundation for employing fiction as an epistemic tool for the social studies of science; perform in-depth readings of the two novels; and discuss those readings in light of scholarly observations of contemporary science systems.

CONCEPTUAL BACKGROUND

Several strands of social and political theory provide the sociological underpinnings for my analysis of cultural understandings of scientific autonomy and responsibility: the duality of science and society in the evolution of modernity, and theories of social autonomy and responsibility.

Science and Modernity

Scholarly use of the term modernity varies widely, but at its conceptual core is the ongoing interplay between new and traditional cultural and institutional patterns (Münch, 1986: 11–34; Bhambra, 2015: 692). An idealistic understanding associates modernity with a commitment to the inalienable freedom of human beings and trust in their capacity to reason. According to Peter Wagner, "this basic commitment translates into principles of individual and collective self-determination and in the expectation of ever-increasing mastery of nature and ever more reasonable interaction between human beings" (Wagner, 2012: 4). Modern society's prevalent structures and processes arise from these principles and expectations. This involves an ongoing functional differentiation into a set of largely self-contained social subsystems with institutionalized means of social production, social roles, and cultural value spheres (Schimank, 2015a: 392; Schimank, 2015b: 415). The formation of modern science systems is a paradigmatic example of functional differentiation with its inclination to produce autonomous actors and the individual and collective mastery of a set of tangible and intangible assets (Wagner, 2008: 98).

Under the influence of functional differentiation, science has evolved into a highly productive social institution with the prerogative to produce, advance, and certify knowledge (Merton, 1973: 270). It is modern society's principal means of acquiring and applying new knowledge that is of epistemic, and, if applicable and socially desirable, of practical interest. In hindsight, scientific practices and scientific knowledge have substantially increased modern society's capacity for action (Adolf and Stehr, 2016: 42) and fostered its belief in the intentional controllability of progress and (Schimank, 2014: 118–123) and in its ability to remove all those aspects of the social and natural world that are constitutively unavailable (Rosa, 2020: 86–101). Science plays a societal role that cannot be filled by any other social subsystem (Parsons and Platt, 1973: 53–57), but it is also structurally dependent on resources produced by other societal subsystems (Luhmann, 2013: 113–114).

A key feature of modern societal subsystems is the cultural valuation and institutional enactment of individual and collective autonomy, whereby social actors allegedly operate by self-referential, field-specific value systems that are not derived from or determined by external sources of authority (Luhmann, 2013: 108–110). Different normative and instrumental notions of the autonomy and social responsibility of scientific research have been part of its institutional and governance discourses at least since World War I (Kaldewey, 2013: 17–23; Stilgoe and Guston, 2017: 853–857). The American Association for the Advancement of Science's Statement on Scientific Freedom and Responsibility is a prototypical example of the scientific community's perception that both scientific autonomy and social responsibility are integral to the production and application of knowledge: "Scientific freedom is the freedom to engage in scientific inquiry, pursue and apply knowledge, and communicate openly (. . .) Scientific responsibility is the duty to conduct and apply science with integrity, in the interest of humanity, in a spirit of stewardship for the environment, and with respect for human rights" (as cited in Jarvis, 2017: 462). Concepts of social autonomy and responsibility are thus key to understanding the ways that the science system and the wider society shape each other.

Autonomy and Responsibility of Science

Immanuel Kant's conception of autonomy and heteronomy provides an ideal starting point for understanding both terms as analytical micro-, meso-, macro-level variables in science and society. Following Kant [Kant, 2011 (1785): 109–111], heteronomous acts are those determined by the will of entities other than their perpetrators. And purely autonomous acts are those determined solely by the will of the actor. Pure autonomy requires actors without external links. Empirically, no social action is purely autonomous. Social action is always

determined by a combination of both internal and external factors. But Kant's juxtaposition can serve as a reference frame for social actions, with the conditions for autonomy being satisfied whenever social actors employ self-reflexive procedures in the process of determining whether and how to act (Dworkin, 1988: 20).

Isaiah Berlin's differentiation between inner and exterior freedom connects the notion of the autonomy of action with the concept of social responsibility (Berlin, 2005: 20). Berlin imagines inner freedom as a retreat to an internal citadel–an area free of outside forces that allows for autonomous action without interference by others–whereas exterior freedom pertains to the capacity of actors to control actions whose consequences extend to other actors. This actor-focused perspective leads to a sociological understanding of autonomy and responsibility within the framework of action theory, which aims to explain how and why different types of social protagonists act in the ways they do (Kalter, 2015: 75). In action theory, the underlying mechanisms of action are analyzed on the micro level, with a focus on specific interactional contexts and actor constellations. These situational analyses lay the foundation for meso- or macro-scale explanations.

Action theory is based on the notion that sociality is continually produced and reproduced by the interplay between social actions and social structures (Schimank, 2015b: 415–416; Schimank, 2016: 16–27). Social actors never fully determine the genesis and outcome of their own actions due to their inclusion into constraining or enabling social structures and relationships. Autonomy is defined as a social actor's degree of control over setting and approaching objectives (Gläser and Schimank, 2014: 47; Gläser et al., 2020: 5). Purposive social actions–acts that are in some part oriented toward others and involve a choice and the evaluation of multiple alternatives–are social precisely because they might have intended or unintended effects on other social actors (Merton, 1936: 895; Offe, 1989: 758). A social actor shares responsibility for such purposive actions, thus linking the social action to the autonomy and responsibility of the actor.

We can analyze the generally accepted institutional goals of science in terms of this understanding of autonomy and social responsibility. The autonomy of science from other social subsystems might then be measured in terms of the relative impacts of scientific and non-scientific actors on both the epistemic and institutional means to produce and certify knowledge. Though it is analytically difficult to separate purely epistemic from institutional actions, it is useful to differentiate between epistemic and institutional autonomy, if only heuristically, e.g. by compartmentalizing scientific capital into the purely epistemic and purely social (Bourdieu, 1991: 7).

The epistemic and social consequences of modern science systems are closely connected to systems for technological innovation and production (Genus and Stirling, 2018: 63). It is difficult to control the intents and impacts of a scientific action, and "during its early stages, when it can be controlled, not enough can be known about its harmful social consequences to warrant controlling its development; but by the time these consequences are apparent, control has become costly and slow" (Collingridge, 1980: 19). Notwithstanding, if these consequences–whether intended or unintended, beneficial, or harmful–are the outcome of a linked chain of action, all actors involved in the planning and implementation of those actions must bear some degree of responsibility. Heather E. Douglas (Douglas, 2003: 63–66) distinguishes scientists' general moral responsibilities as members of society from those that are specific to their roles in defining and performing scientific research and related actions. This distinction applies to both individual and collective actors. Assigning responsibility for the intended and unintended consequences of social action highlights the structural tension between scientific autonomy and responsibility.

In scholarly and policy discourse, arguments about the various social forces that support or constrain autonomy and responsibility in the organization and practice of scientific research focus on three basic topics (Wilholt and Glimell, 2011: 352–357; Wilholt, 2012: 11–12):

- Freedom of research: The autonomy of science is a prerequisite for free inquiry, which is the most effective way of organizing and conducting research that is primarily motivated by purely epistemic interests (Polanyi, 1945: 142). The productivity of scientific research is essential to the function of other social spheres, as scientific knowledge is required for informed decision-making, particularly in the government policy and economic spheres (Stehr, 2015: 108).
- Accountability: Because of its potential impacts and systemic openness, science is inseparable from society and requires societal accountability. The interests and actions of individual and collective actors within science are not the result of purely epistemic considerations but of interactions with other actors both within and beyond science: they therefore require outside oversight.
- Targeted research: Modern society requires science to contribute to its context- and time-specific social needs. Basic research that derives from purely epistemic considerations cannot be expected to generate enough socially applicable knowledge to meet these demands, so science must be strategically organized and governed (Gibbons, 1999: C84).

These arguments about the independence and accountability of scientific inquiry pertain to all actors in the science system, including individual scientists, research groups, private and public research institutes, universities, and funding organizations. At their heart, lies the classical scientific ethos-norms of conduct such as universalism, disinterestedness, communalism, and organized skepticism that "are in varying degrees internalized by the scientist, thus fashioning his scientific conscience" (Merton 1973: 269) and the notion that the balance between autonomy and responsibility impacts the production and, ultimately, the advancement of knowledge (Kaldewey, 2013: 410). For this reason, and because of their rhetorical efficacy, "scientific autonomy" and "scientific responsibility" have joined the everyday parlance of both scientists and policymakers (Panofsky, 2010: 140).

METHODOLOGICAL BACKGROUND

Contemporary literature's increasing engagement with modern science (Gaines, 2001; Rohn, 2010a) and distinct observational qualities make it a fruitful resource for social studies of science. Cultural studies scholars, sociologists, and literary theorists have delved into the nexus between understandings of social interaction and the creation and consumption of literature. Rita Felski notes that "reading involves a logic of recognition; that esthetic experience has analogies with enchantment in a supposedly disenchanted age; that literature creates distinctive configurations of social knowledge; that we may value the experience of being shocked by what we read" (2008: 14). Erkki Sevänen maintains that fiction can process and represent societal developments; he regards modern literature "as communicative acts between authors and society" (2018: 53). Similarly, Albrecht Koschorke (Koschorke, 2018: 51) considers narration a communicative game with hypothetical problem-solving possibilities that processes and resolves topics that matter to the narrators and their intended audience. A sociology of knowledge approach to literature acknowledges that social actors such as authors, general readers, and scholars have internalized tacit and explicit knowledge of the social worlds they are embedded in and of these worlds' social and cultural conventions, attitudes, and rules (Sevänen, 2018: 52). Literary and cultural studies scholars have long plumbed literature and other cultural artifacts for their latent social analysis and knowledge in understanding postcolonial societies (Ashcroft, 2017), economic history (Roxburgh, 2015), or contemporary financial capitalism (Vogl, 2014).

The disciplinary division of labor between literature and the social sciences, even while addressing similar topics of inquiry, has been an issue since the mid-nineteenth century when scholars "contested with one another the claim to offer the key orientation for modern civilization" (Lepenies, 1992: 1). In Europe in particular sociology established itself as a distinct epistemic culture that was situated between the natural sciences and the humanities. Large swathes of the social sciences in France, England, and Germany underwent a process of conceptual and methodological scientification that separated them from their literary counterparts. In North America, the assumption that the analysis of sociological topics could be advanced by exploring fictional literature was more established, especially in the Chicago School of Sociology (Coser, 1972: 2–3). For instance, Florian Znaniecki (Znaniecki, 1934: 193–197) advanced a methodology in which observations of the social by other social actors–what Niklas Luhmann calls first-order observations (2013: 224–225)-constitute a form of sociological utilizable data. Znaniecki assigned literature to this class of data on human experience (Znaniecki, 1952: 134), because it is composed by authors who process the cultural tendencies and social interactions of the societies they live and work in. This line of reasoning anticipated the general approach employed by the sociology of literature and literary and cultural studies to substantiate the value of fictional literature for scholarly social analysis (Becker, 2007: 5; Matthies, 2016: 17; Farzin, 2019: 140; Váňa, 2020: 184–186).

Fictional literature allows the reader to experience particular social worlds from the inside (Felski, 2008: 92). Fiction does not necessarily provide documentary representations of the social world, but rather "what-if" constructions of the interaction between social actors who deliberately process, embed, and configure a variety of themes, events, and relationships. Fictional texts offer a means "with which to probe into reality, testing certain features of the world as described in the text" (Longo, 2015: 140) precisely because they can highlight the cultural desiderata of particular social milieus and discursively shared cultural conceptions. These observations form the basis for my analysis of science novels, which adopts Helmut Kuzmics' and Gerald Mozetič's three premises for the use of literary sources in sociological analysis (Kuzmics and Mozetič, 2003: 26–35): First, fiction can illustrate sociologically relevant themes and phenomena. Second, literature has the potential to be a descriptive source of cultural and social representations. Third, fictional texts can bear latent and/or manifest explanatory potential of social phenomena.

The novels I examine fall within a category of fiction–variously known as science novels, lab lit, and science in fiction–that features researchers as main characters and explores scientific problems, research practices, and their respective organizational and cultural contexts (Haynes, 2016a: 128–130; Pilkington, 2019: 1–2; Gaines et al., 2021). These works of fiction enable examination of the purposive actions of scientists and "the way in which that fictionalized process is affected by the author's reconstruction of the dominant discourse of the day, both within and beyond the scientific community" (Schaffeld, 2016: 121). Following on Znaniecki and Luhmann, I consider my analysis of these novels a second-order observation of science in society (Gaines et al., 2013: 9). While sociologists can and do produce first-order observations of the culture, organization, and practice of science, such observations are limited by the methodological constraints of conventional research. Novelists, on the other hand, are more autonomous in their scope to observe, participate in, respond to, and imagine "what-ifs," creating a singular configuration of social circumstances for the sociologist reader to process alongside first-order empirical accounts and sociological survey data.

ANALYSIS OF TWO SCIENCE NOVELS

Summarizing the above considerations, the conceptual and methodological premises of my analysis are threefold: First, the autonomy and responsibility of social actors are distinct institutional and cultural features of modern society. Second, the autonomy and social responsibility of science are central cultural frames in modern science systems, though their institutional manifestations and effects may vary considerably. Third, science in fiction is the literary product of culturally situated social practices that observe, process, and display cultural understandings of science in society. I employ a form of qualitative content analysis with a textual and thematic focus that is guided by two basic research questions (Braun and Clarke, 2006; Schreier, 2017): What understandings of the autonomy and

societal responsibility of science are displayed in the novels? How do they correspond with actual cultural and scholarly conceptions of scientific autonomy and responsibility? Progressing from a descriptive summary to a thematic interpretation of the text, I do not aim for a generic account of autonomy and responsibility, but rather to "elicit meaning, gain understanding, and develop empirical knowledge" (Bowen, 2009: 27) of a set of individual and novel cultural understandings.

Allegra Goodman's *Intuition* (2010) and Jennifer Rohn's *The Honest Look* (2010b) are set in biomedical research laboratories in the United States. and Europe respectively. Literary scholars and critics have called both novels "lab lit," a term Jennifer Rohn herself coined to describe decidedly "realistic novels that contain scientists as central characters plying their trade" (Rohn, 2010a: 552; Pilkington, 2019: 301). Lab lit typically details everyday laboratory life, engages "with the process of 'doing science'," and indicates "realistically how actual scientists think and behave in the intense atmosphere of a research laboratory" (Haynes 2016a: 36). Both *Intuition* and *The Honest Look* derive from the authors' immersion in contemporary science systems and local research sites. In order to observe the inner working of research laboratories for her novel, Goodman did a considerable amount of field observation at the Whitehead Institute for Biomedical Research in Cambridge, Massachusetts (Longhito, 2007: 2,272). Jennifer Rohn, besides being a novelist and science journalist, works as a cell biologist at University College London and leads her own research lab (Rohn, 2008: ix). Moreover, scientists who have reviewed *Intuition* (Thomas, 2006: 1,235) and *The Honest Look* (Herndon, 2010: 1,039) describe their stories as plausible, credible, and thematically relevant to current developments in modern science systems. Both novels depict prototypical scenarios of the organization and practice of public and private biomedical research and are thus well-situated to explore the autonomy and social responsibility of science.

An Intuitive Look at Public Biomedical Research

The story of Goodman's *Intuition* revolves around the fictional Philpott Institute, a publicly funded biomedical research laboratory in Cambridge, MA. The Philpott is not affiliated with the local universities, Harvard and the Massachusetts Institute of Technology, but it is trapped in competition with them for research funding and peer recognition. The laboratory staff comprises the Philpott directors, Sandy Glass and Marion Mendelssohn, several postdoctoral researchers–including Cliff Banneker and Robin Decker–and various lab technicians and graduate students. Shortly before the end of Banneker's postdoctoral contract, after years of modest or disappointing research results, he suddenly seems to have made crucial progress in the development of R-7, a modified virus that has been designed to transform cancer cells into normal cells. After his newly gathered data hints that R-7 might be able to eradicate cancer tumors from mice, Glass and Mendelssohn concentrate all the lab's resources on follow-up research. These efforts result in the rushed publication of a much-anticipated research article in the prominent interdisciplinary science journal, Nature, and are touted as a major scientific breakthrough in the news media. Banneker's data is the basis for a successful grant proposal at the National Institutes of Health (NIH), the United States' largest biomedical research agency and source of public funding.

In order to verify the data and concomitant conclusions, Mendelssohn and Glass assign Decker, a post-doc whose own project shows relatively little promise, the task of reproducing Banneker's work *in vivo*. Her attempts fail several times and, after doubting her own research abilities, she begins to suspect that something might be wrong with Banneker's initial data. Decker discovers inconsistencies in Banneker's recordkeeping and adherence to experimental protocol–potentially a form of misconduct in and of itself–but it is her growing intuition that the data was intentionally manipulated that leads her to inform Mendelssohn and Glass of her suspicions. When an internal review exonerates Banneker of research misconduct, Decker becomes a whistleblower for NIH's Office for Research Integrity in Science, which oversees the probity of federally funded research activities. This results in a scientific controversy, and the Subcommittee on Science and Technology of the United States House of Representatives summons the involved members of the lab to appear at a series of public hearings. Meanwhile, further observations of R-7's effectiveness at the Philpott lab show that the initially reduced tumor cells in mice have begun to reappear. Though this is an intriguing finding in itself, failure to reproduce Banneker's results and growing internal doubts, as well as the negative publicity and external pressure, prompt Glass and Mendelssohn to retract the Nature paper. Other laboratories are also unable to fully reproduce Banneker's findings, and attention soon turns away from R-7 as a cancer treatment. The misconduct case is, however, dismissed on procedural and political grounds, when Decker's reputation is thrown into question.

Intuition offers a "what-if" narrative of research misconduct and "details the factors that allow an insufficiently substantiated claim to gain credence, however transitorily, in the scientific community" (Kirchhofer and Roxburgh, 2016: 159). It can be read as a critique of how scientific reward systems based on competition, originality, and positive results–exacerbated by funding pressures and the reward structures of individual and collective research careers–can interfere with norms of scientific responsibility and foster poor practices, outright misconduct, and, ultimately, false data and conclusions (Kalleberg, 2015: 313–314). The novel leaves open the question of whether Banneker's unreproducible research findings are the intended or unintended consequences of questionable research practices or outright scientific misconduct: "Perhaps his work with R-7 had been more about ideas than concrete facts; perhaps his findings had been intuitive rather than entirely empirical. He had not followed every rule" (Goodman, 2010: 320). In this multi-layered novel, the characters and research organizations are so entangled and embedded in various social configurations–postdocs, research group, directors, the Philpott Institute, other scientific and political institutions, and media–that they are never entirely in control of their own actions, let alone the outcomes.

Though this lack of autonomy affects the work of the postdoctoral researchers most acutely, it also constrains the

actions of the lab directors and limits the capacity of the allegedly independent research institution at the heart of the novel:

> Two to a bench, like cooks crammed into a restaurant kitchen, the postdocs were extracting DNA in solution, examining cells, washing cells with chemicals, bursting cells open, changing cells forever by inserting new genetic material. (. . .)
> In 1985, the Philpott was famous, but it was full of old instruments. Dials and needle indicators looked like stereo components from the early sixties. The centrifuge, designed for spinning down cells in solution, was clunky as an ancient washing machine. There wasn't enough money to buy new equipment. There was scarcely enough to pay the postdocs (Goodman, 2010: 3–4).

From the perspective of the postdoctoral researchers, the laboratory, indeed the institution of science in general, functions like a sort of prison workshop: "Years and years of manual labor went by. New results filtered through only on the rarest occasions, and always to other people. Miracles didn't happen, but Cliff and his friends kept on working. Like scientific sharecroppers, they slaved all day. They were too highly trained to stop. Overeducated for other work, they kept repeating their experiments. They kept trying to live on their seventeen-thousand-dollar salaries" (Goodman, 2010: 20). This realization comes to Banneker in the midst of a knowledge production crisis that threatens to undermine his hopes of ever obtaining a permanent job as a research scientist: After developing and testing the R-7 variant for two years, he has found no evidence of its effectiveness in reducing cancerous tumors. Decker's project-"an analysis of frozen samples of blood, collected over the years from cancer patients who had died of various forms of the disease" (Goodman, 2010: 7) in search of a unifying syndrome underlying their diverse conditions–has been similarly fruitless and short of positive results.

Both from an epistemic and institutional standpoint, the ideal laboratory has been imagined as an inner citadel that locks out all aspects of the natural and social world that defy control so that such research sites "not only improve upon natural orders, but (. . .) also upgrade social orders" (Knorr-Cetina, 1999: 28) of laboratory processes and research organization to become an enhancing instrument of scientific work. But Banneker, Decker, Glass, and Mendelssohn are not acting within such an epistemically unconstrained structure. External social forces and expectations constantly shape their activities. The postdocs are especially dependent on the tangible output of useful, publishable results to establish their reputations within the biomedical research community. The novel depicts a classical and still prevalent scientific reward structure that emphasizes originality in its various forms–new discoveries and paradigm-shifting breakthroughs–which causes "extreme inequality with regard to scientific productivity and the awarding of priority" (Stephan, 1996: 1,203). When Banneker's experiments are unsuccessful, Glass and Mendelssohn order him to abandon his hypothesis and work to support the lab's other ongoing projects. And when he defies their orders and runs a final set of experiments that suddenly indicate that R-7 might be able stop or even reverse cancer growth, they reverse gears and order the rest of the lab to shift their attention to Banneker's project. Though they admonish the post-docs that "[t]there is no such thing as your own project in this lab" (Goodman, 2010: 6), it is abundantly clear that, like Banneker, Robin and the other post-docs depend on the success of their own independent research ideas to demonstrate their ability to produce original scientific insights and insure their futures as research scientists.

Both epistemically and institutionally, control of goal definition and achievement is severely restricted for graduate students, postdocs, and early career researchers: "On the ground, in the lab, intuition was a restricted substance. Like imagination and emotion, intuition misled researchers, leading to willful interpretations. While scientists like Mendelssohn knew how to wield it properly, young researchers had their intuition tamped down lest, like the sorcerer's apprentice, they flood the lab with their conceits" (Goodman, 2010: 183). Thus, the novel presents the lab as a collective workshop in which research insights are "appropriated by the managers without further ado" (Zwart, 2017: 184). Its powerless postdocs have internalized the rules of the game whereupon scientific rewards and reputations are built on individual and not necessarily on collective accomplishments. In that sense, the laboratory resembles a feudal community: "There are the lords and ladies like Glass and Mendelssohn, and then the postdocs are the vassals paying tribute every year in the form of publications, blood, sweat, tears, et cetera" (Goodman, 2010: 211).

Yet the story illustrates in several events that Glass and Mendelssohn, reminiscent of actual feudal lords in medieval Europe, are far from omnipotent. Instead, their work, reputation, and positions equally depend on the output of postdocs and, in consequence, on successful grant applications in order to organize subsequent research in their laboratory. The lab is part of a non-university research institute that is presented as a "poor principality" (Goodman, 2010: 109), "has run a deficit for the past three years" (Goodman, 2010: 290), has no substantial institutional funding, and relies therefore on the ability of its constituent research groups to continuously attract research funds. It is "governed by strict Darwinian principles. Investigators broke even or went bankrupt, losing staff and space and equipment to their rivals (. . .) Lab directors without funding had little recourse; they took desperate measures: they switched fields, or retired, or sometimes left science altogether" (Goodman, 2010: 17). While simultaneously disagreeing on the exact way to proceed with Banneker's research, Glass and Mendelssohn agree to establish strong priority claims with regard to the potential results of the R-7 experiments in order to substantiate their grant proposal that could provide research funding for several years (Goodman, 2010: 71). Both acknowledge the dire economic situation of the lab that can only be overcome by eventually overplaying the classical reward game and rushing ahead with the publication of inconclusive results before someone else can stake similar claims. In hindsight, their prediction becomes a self-fulfilling prophecy. The lab's concentration on R-7 misspends its limited material and personal resources and reproduces poor research.

To sum up, *Intuition* exemplifies a science system in which the epistemic and institutional autonomy of postdocs, senior

researchers, and independent research institutes is severely constrained by internal and external factors. That rationalizes questionable research practices, unintentional negligence, or even intentional misconduct. While the internal reward structure can be considered as a historically and contextually contingent outcome of internal developments within the social system of science, the lack of institutional funding and the dependence on project-based research grants are decidedly external social forces that foster the structural importance of priority claims, peer-acknowledged reputations, and swift publications of research findings. These phenomena are, in the novel and in most if not all modern research systems, rooted in the economization of modern society and modern science in particular (Schimank, 2021: 148–149). Moreover, bad and fraudulent science is epistemically and socially useless, even harmful, and a waste of resources. Apart from detrimental financial implications, such poor and misleading research practices as depicted in the novel curb the progress of the biomedical sciences (Chevassus-au-Louis, 2019: 105–114). It is therefore not far-fetched for society at large to develop doubts with respect to the social responsibility and surplus value of contemporary research, especially with those projects that are funded by public bodies (Schomberg, 2013: 63).

At the mentioned congressional hearing a congressman attacks Glass, Mendelssohn, and their lab by claiming that science is "corrupted by (their) desire for more and still more funding, and a lust for quick results" and rewards "intellectual dishonesty" (Goodman, 2010: 269). While this is in part factual and an unintended consequence of established funding and reward structures, this is not entirely the result of internal processes within public research systems. The novel latently alludes several times to science policy paradigms that prioritize economic considerations for financial costs and the collective organization of research analogous to market competition, such as New Public Management or other forms of academic capitalism (Mirowski and Sent, 2008: 637; Berman, 2014: 420; Münch, 2014: 1–12; Schimank and Volkmann, 2017: 175; Jessop, 2018: 105). Accordingly, several characters in the novel–for instance the congressman who lamented what could be called the corrosion of the ideal scientific ethos (Sennett, 1999; Goodman, 2010: 262–263) emphasize that science should be used to improve the economy, the most important subsystem of modern society. Considering the tension between and the ambivalent impact of these internal and economic constraints on the governance of public research and the production of scientific knowledge, it remains to be seen how the ideal-typical imperative for the autonomy and social responsibility of science is manifested in cultural representations of science that look upon private and deliberately commercial biomedical research, such as with the novel discussed in the next section.

An Honest Look at Private Biomedical Research

Jennifer Rohn's *The Honest Look* (2010b) explores the early stages of Claire Cyrus's research career in a pharmaceutical research laboratory of a private company, the scientific and economic pressure to produce ground-breaking and assetizable treatments, and the vested interests of the corporate biomedical sciences (Haynes, 2016b: 36). Set in the Netherlands, the story features Cyrus as one of the few researchers in the world who can operate the so-called Interactrex 3000, "a must-have tool for those dedicated to finding cures for the killer diseases that have plagued mankind for centuries" (Rohn, 2010b: 2). This expensive machine, christened by her as Raison D'être (sic), "can peer into living cells and watch proteins interact in real time" (Herndon, 2010: 1,039). Having been trained by Maxwell Bennett, a renowned biologist, inventor of the Raison, and her former PhD supervisor at the University of Liverpool, she is successfully headhunted by Stanley Fischer, the CEO of NeuroSys, a biotech startup in the metropolitan area of Amsterdam. Because of the machine's potential significance for advancing the company's research into Alzheimer's disease, Cyrus begins to collaborate, among others, with Allan Fallengale, "a much older lecherous senior scientist" (Chester, 2011: 2,936), in order to check the effectiveness of the company's essential scientific asset, a potential drug for Alzheimer's called the Zapper.

Much more than *Intuition*, the novel presents the tangible scientific apparatus as "both a character and a foil for the main actant" (Pilkington, 2017: 301) to the effect that Cyrus's research produces remarkable findings from a purely epistemic standpoint that bears problematic implications for the biomedical potential and financial stability of the company. Machines and engineering techniques like the Interactrex are the eyes and backbone of modern biomedical research and underpin its capacity for scientific insight and pharmaceutical applicability. Hence, the story illustrates how the development, existence, and access to machines underpins the autonomy of research practices:

> The Raison still threw spectacular fits, but she was getting better at dealing with the machine. And the experiments were finally starting to work. She found being an expert at something so much more gratifying than her lowly PhD student experience. The Raison wasn't some prototype, cobbled together with gaffer tape and aluminum foil into a massive rattling thing that the rest of the department laughed at behind her back. It was a gleaming state-of-the-art machine, and the whole world was watching its first paces to see how it fared (Rohn, 2010b: 46).

NeuroSys, "a one-trick pony" (Rohn, 2010b: 12), has high expectations on the Zapper. Its corporate success depends on the treatment's practical capability and on the validity of the so-called Universal Aggregation Principle, its underlying "revolutionary theory explaining the pathology behind Alzheimer's disease" (Rohn, 2010b: 24). This method underpins the pure scientific, institutional, and economic capital of NeuroSys and its senior scientists:

> NeuroSys had been founded ten years previously to develop treatments for neurodegenerative diseases such as Alzheimer's. Its scientists, headed by Alan Fallengale and Ramon Ortega, had discovered a key vulnerability underlying these disorders and designed the company's first key drug: a compound called NS158, otherwise known as The Zapper. Patents were filed, NeuroSys was floated on the stock market, and it was rumored that patient trials were just around the corner. Emboldened by these successes, the company was expanding into other

> disorders, including stroke, and had recently convinced venture capitalists to fund their next phase. Hence, the purchase of the Interactrex 3000 and the hiring of Claire (Rohn, 2010b: 13).

Being new on the job, a junior scientist with insignificant clout, and the only one in the company capable of using the Interactrex, Cyrus is exclusively assigned to modify and improve the Zapper. Up to this point, due to her unique skill in operating the machine, her scientific independence is not constrained at all, at least from a purely epistemic point of view with respect to this particular task. Intentionally provided by the company's leading researchers to fully utilize the potential of the machine, this organizational configuration allows her to conduct comparatively autonomous research and collaborate almost on a peer-to-peer level with scientific and administrative superiors. Serendipitously, she discovers a fatal flaw in the underlying Universal Aggregation Principle that explains both the insufficient impact and a detrimental side effect of the drug: "Experiment, result, interpretation, the three links in the chain of this tidy, ordered profession that had seduced her. A conveyor belt of logic that only flowed in one direction, if you set up your experiments properly. No Universal Aggregation, no target for the Zapper: no cure for Alzheimer's" (Rohn, 2010b: 99). Based on Cyrus' findings, Joshua Pelinore, the company's leading bioinformatician, predicts "the drug in its current form (. . .) to seriously impair higher cognition in healthy brain cells–it may cause more problems than the Alzheimer's it's trying to cure" (Rohn, 2010b: 240).

Cyrus and her superiors know what would happen to NeuroSys if its scientific and economic mainstay turned out to be flawed: Investors would lose trust in the firm's potential and cease additional funding. Subsequently, the company would go bankrupt due to a lack of alternative revenue streams. "Nothing else we're working on is even close to being marketable" (Rohn, 2010b: 131). Over the further course of the story, Cyrus reluctantly informs her peers, the lab's superiors, and the company's management for "it was one thing to react reasonably about a potential flaw that might make necessary a minor chemical adjustment in an established drug (. . .) But it would be quite another to be faced with the destruction of a life work, a cherished theory and the entire reason for NeuroSys's existence" (Rohn, 2010b: 190–191). In reaction to this negative scientific breakthrough that dooms the corporate prospects of NeuroSys and threatens his individual scientific reputation and financial rewards, Fallengale, without consulting his longtime collaborators in the lab and the firm, sells the Interactrex to remove incriminating evidence and to prevent others from reproducing Cyrus's results. These actions enable him to collect his contractually secured milestone payment. He subsequently resigns from his position within the firm, long before anyone outside can find out the truth (Rohn, 2010b: 308).

In sum, the novel narrativizes three cases of scientific and institutional failures that encompass poor scientific practices and misconduct in private biomedical research. First, the practical utility of the treatment and the epistemic utility of the underlying theory turn out to be wrong, but it remains unclear how much resources were invested by the company to verify the validity of both by additional internal and external research. Second, Cyrus, because of her awareness of the potential personal and institutional implications of the Raison's findings, initially refrains from telling the truth. Third, Fallengale's reactions and his successful attempt to remove potentially incriminating evidence constitutes clear intentional scientific misconduct. In that sense, the novel may be read as a counterintuitive argument against an overload of individual and organizational autonomy, at least in epistemic terms, which can result in too much protected space and flexibility within the confines of the firm and with regard to the wider biomedical community (Whitley, 2014: 370–371). While Fallengale might have been motivated by heteronomous motives, his protected senior position within the organization and the extensive trust of his peers and collaborators in his integrity allowed him to act in the way he did. The resulting individual and organizational opportunity structures (Eisinger, 1973: 12) enable social irresponsibility toward scientific and corporate insiders and outsiders. The consequences harm not only the involved scientists, the company, or its investors. It also delays the progress of medical research and, in turn, the potential societal return, especially in the form of potential improvements in treating Alzheimer's, a disease that affects the lives of millions worldwide, and other forms of neurodegeneration.

Additionally, and in contrast to *Intuition*, *The Honest Look* sheds light on the ambivalent aspects of privatizing knowledge. The case of NeuroSys exemplifies the potential epistemic corruption of privatizing scientific insights and the closed practices of restrictive knowledge-control regimes that curb reviews by disciplinary peers (Hilgartner, 2017: 8–11; Sismondo, 2021). It shows how the transformation of knowledge into assets that can be owned, controlled, and capitalized is a salient feature of modern knowledge societies whose economic outlook is increasingly shaped by technoscientific capitalism (Birch and Muniesa, 2020: 19). Such knowledge can only become visible and testable, to a limited and controllable degree, if it appears to be an owned asset, for instance a patent, a machine, or a drug, that substantiates the social, mostly financial interests of the respective institution that controls it. Such research has ceased to be a public good that is, at least epistemically, owned by scientific communities; it is a practice that contradicts the classical scientific norm of communalism, according to which any scientific insight belongs to the whole research community (Merton, 1973: 273). That limits the capability of external actors within and outside of the scientific field to check the validity of these knowledge claims and objects. Thus, the tale of Claire Cyrus alludes to the dysfunctional consequences of dominant economic considerations that emphasize the need to maintain an organizational front that presents the product as valuable and the company as innovative, which supersedes the basic scientific imperative to find errors in data and to double-proof theoretical and empirical insights (Schimank, 2021: 162).

DISCUSSION

Intuition and *The Honest Look* display a cultural understanding of the autonomy and social responsibility as manifest and latent normative ideals in modern science systems that are insufficiently

implemented due to internal and external social constraints on the organization and practice of public and private biomedical research. While each novel portrays the economization of science as a serious impediment to scientific progress, *Intuition* depicts how economic considerations accelerate the classical reward structure of science. This limits the institutional and individual autonomy of research, especially for scientific organizations that lack substantial channels of core funding and for junior researchers who are obliged to produce positive, presentable results in order to demonstrate their ability to the scientific community. In contrast, *The Honest Look* shows how profit-driven research neglects classical research norms by curbing and circumventing the scrutiny of external members of scientific community. Given the biomedical background of the company, none of its depicted members appear to be purposely driven by the prospect of healing diseases or solving epistemic puzzles. Instead, they are culturally, institutionally, and financially more inclined to build a reputation with potential investors, shareholders, and consumers. In turn, the consequences of these different cases are similar: poor scientific practices, intentional and/or unintentional scientific misconduct, and obstacles to scientific and social progress. Taken together, both narratives also allude to the ambivalence of the autonomy and heteronomy of research. Both a lack and an overload of scientific autonomy can lead to socially irresponsible research practices and outcomes.

Intuition and *The Honest Look* cannot only be considered as distinct literary outcomes out of a field of cultural production that observes and processes the cultural understanding of science in modern society. From a social studies of science standpoint, both narratives also offer an interpretation of the state and trajectory of contemporary science and modern society's capacity to identify and adapt to social problems. In this regard, the novels contrast an idealized notion of an autonomous and socially responsible science with research systems that are shaped by diverging internal and external social interests and produce epistemic and social uncertainty. In that sense, their representation of contemporary research resembles that of Kim Stanley Robinson's *Forty Signs of Rain* (2004). This first part of his *Science in the Capital* trilogy is another literary treatize on, among other things, science in modern society and offers a somewhat grand narrative of the structural constraints internal and external to the scientific field that also applies to Goodman's and Rohn's novels. *Forty Signs of Rain* presents a prototypical scenario portraying how and why a modern society, in this case the United States, is currently unable to tackle the grand challenges of human-induced climate change. This is in part due to knowledge and technology gaps, but the main reasons for this insufficient adjustment are socially constructed. Those societal actors advocating for socio-ecological adaptation and mitigation–the main protagonists of the novel taking that stance are researchers working for the National Science Foundation (NSF), the largest federal agency in the United States that funds basic research science and engineering–constantly experience how they do not control the necessary societal positions and do not possess sufficient resources to achieve this societal goal. This is in part due to the inefficacy of modern science systems that is characterized by a divergence between normative and actual cultural and institutional patterns in the organization and practice of research, illustrated by the following quotes:

> But science didn't work like capitalism. That was the rub, that was one of the rubs in the general dysfunction of the world. Capitalism ruled, but money was too simplistic and inadequate a measure of the wealth that science generated. In science, one built up over the course of a career a fund of "scientific credit," by giving work to the system in a way that could seem altruistic. People remembered what you gave, and later on there were various forms of return on the gift–jobs, labs. In that sense a good investment for the individual, but in the form of a gift to the group. It was the non zero-sum game that prisoners' dilemma could become if everyone played by the strategies of always generous, or, better, firm but fair. That was one of the things science was–a place that one entered by agreeing to hold to the strategies of cooperation, to maximize the total return of the game.
>
> In theory that was true. It was also the usual troop of primates. There was a lot of tit for tat. Defections happened. Everyone was jockeying for a lab of their own, or any project of their own. As long as that was generating enough income for a comfortable physical existence for oneself and one's family, then one had reached the optimal human state. Having money beyond that was unnecessary, and usually involved a descent into the world of hassle and stupidity. That was what greed got you. So there was in science a sufficiency of means, and an achievable limit to one's goals, that kept it tightly aligned with the brain's deepest savannah values. A scientist wanted the same things out of life as an Australopithecus; and here they were (Robinson, 2004, 133–134).

The characters in *Intuition* and *The Honest Look* experience a similar divergence between idealized norms and actual patterns. In addition to depicting how science works or fails to work, both novels illustrate how epistemic and socially dysfunctional scientific practices (that can lead, for instance, to scientific fraud) are the outcome of two interpenetrating social structures: those that are internal to the scientific community, such as the dissociating effects of classical reward systems and credibility cycles which are grounded in demonstrating scientific priority, producing a lot of fast-paced publications, and securing funding for further research (Braun, 1994: 32–33), and those that are partially or entirely external to the social field of science, notably public and private funding regimes (Chevassus-au-Louis, 2019: 164–175). Both structures constrain the institutional and epistemic autonomy of research and impede the capacity of science to produce insightful and applicable research. In turn, these dysfunctions can and do limit science's capacity to meet its societal responsibilities as a public good that goes beyond the production of mere economic assets for other social fields (Callon, 1994: 416–418). While a bijective interpretation of the novels, and fictional literature in general, is neither feasible nor desirable, the preceding analysis and this discussion leads to the assertion that through depicting internal and external distortions of the classical scientific ethos, both novels represent literary thought experiments of contemporary research that reinforce and

deconstruct the cultural ideal autonomy and social responsibility of science in modern societies.

CONCLUSION

In contrast to the ambiguous title of the paper, my reading of *Intuition* and *The Honest Look* has not necessarily produced completely novel insights into the autonomy and social responsibility of contemporary science in modern society. Instead, it has taken the cultural understanding of science as an avenue for a sociological exploration of how these notions are manifested in two salient literary narratives that imagine two different yet comparable tales of research misconduct. Based on the methodological assertion that the narrative depiction of science in contemporary popular culture bears the epistemic potential to offer conceptual insight as it puts social actors, actor constellations, and interactions within and beyond the institution of science at the center stage of their respective stories, the analysis has shown the multi-layered societal impact of and on private and public biomedical research, especially with respect to internal and external authority relations, reward structures, and funding regimes. In this regard, Goodman's novel emphasizes how internal aspects of the science system, especially its institutional structure and peer-based reward system, enable an ethos that encourages scientific malpractice and subsequently leads to the delay of epistemically more fruitful research projects. In contrast, Rohn's novel exemplifies the corrosion of the ideal scientific ethos by profit-driven practices in private research organizations. Together, both science novels problematize the ambivalence between scientific autonomy and social responsibility by displaying contemporary dynamics of modern science, notably with regard to the structural pressures due to the increasing economization of public and private research systems.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

As the single author of the paper, FH is responsible for the paper in its entirety.

FUNDING

This paper is an outcome of a project on the "Understandings of Scientific Autonomy and of the Societal Responsibility of Science in Science Novels from Three National Variants of the Culture of Modernity Fiction." It is part of "Fiction Meets Science," an academic research program funded by the Volkswagen Foundation.

ACKNOWLEDGMENTS

This paper is the outcome of my involvement with the Fiction Meets Science research program and many of its members. This includes, among others, Uwe Schimank, Sina Farzin, and other members of the FMS reading group on sociology. Special thanks go to all those, first and foremost to Susan M. Gaines, who helped me in copy editing the paper into a digestible version. Furthermore, I would like to thank the reviewers and editors of this research topic for their extremely helpful comments on earlier drafts of the manuscript.

REFERENCES

Adolf, M., and Stehr, N. (2016). *Knowledge: Is Knowledge Power?*. London, New York: Routledge. doi:10.4324/9781315543093

Ashcroft, B. (2017). *Utopianism in Postcolonial Literatures*. London, New York: Routledge.

Beck, U. (1992). *Risk Society: Towards a New Modernity*. Los Angeles, London, New Delhi, Singapore, Washington DC: SAGE.

Becker, H. S. (2007). "Telling about Society," in *Telling about Society*. Editor H. S. Becker (Chicago, London: University of Chicago Press), 2t14.

Berlin, I. (2005). "Two Concepts of Liberty," in *Liberty*. Editor H. Hardy (Oxford: Oxford University Press), 166–217.

Berman, E. P. (2014). Not Just Neoliberalism: Economization in US Science and Technology Policy. *Sci. Technol. Hum. Values* 39, 397–431. doi:10.1177/0162243913509123

Bhambra, G. K. (2015). "Modernity: History of the Concept," in *International Encyclopedia of the Social & Behavioral Sciences*. Editor J. D. Wright (Amsterdam: Elsevier), 15, 692–696. doi:10.1016/b978-0-08-097086-8.03134-2

Birch, K., and Muniesa, F. (2020). "Introduction: Assetization and Technoscientific Capitalism," in *Assetization*. Editors K. Birch and F. Muniesa (Cambridge, Massachusetts: The MIT Press), 1–42.

Bourdieu, P. (1991). The Peculiar History of Scientific Reason. *Sociol. Forum* 6, 3–26. doi:10.1007/BF01112725

Bowen, G. A. (2009). Document Analysis as a Qualitative Research Method. *Qual. Res. J.* 9, 27–40. doi:10.3316/QRJ0902027

Braun, V., and Clarke, V. (2006). Using Thematic Analysis in Psychology. *Qual. Res. Psychol.* 3, 77–101. doi:10.1191/1478088706qp063oa

Braun, D. (1994). *Structure and Dynamics of Health Research and Public Funding: An International Institutional Comparison*. Dordrecht, Boston: Kluwer Academic Publishers.

Callon, M. (1994). Is Science a Public Good? Fifth Mullins Lecture, Virginia Polytechnic Institute, 23 March 1993. *Sci. Technol. Hum. Values* 19, 395–424. doi:10.1177/016224399401900401

Chester, A. (2011). The Honest Look J. L. Rohn Cold Spring Harbor Laboratory Press, 2011, Pp. 343 ISBN: 978-1-936113-11-8. *Proteomics* 11, 2936–2937. doi:10.1002/pmic.201190069

Chevassus-au-Louis, N. (2019). *Fraud in the Lab: The High Stakes of Scientific Research*. Cambridge, Massachusetts: Harvard University Press. doi:10.4159/9780674242111

Collingridge, D. (1980). *The Social Control of Technology*. London: Pinter.

Collins, H. M., and Pinch, T. J. (1993). *The Golem: What You Should Know about Science*. Cambridge: Cambridge University Press.

Coser, L. A. (1972). "Introduction," in *Sociology through Literature*. Editor L. A. Coser (Englewood Cliffs, New Jersey: Prentice-Hall), 2–7.

Douglas, H. E. (2003). The Moral Responsibilities of Scientists (Tensions between Autonomy and Responsibility). *Am. Philos. Q.* 40, 59–68.

Dworkin, G. (1988). *The Theory and Practice of Autonomy*. Cambridge: Cambridge University Press. doi:10.1017/cbo9780511625206

Eisinger, P. K. (1973). The Conditions of Protest Behavior in American Cities. *Am. Polit. Sci. Rev.* 67, 11–28. doi:10.2307/1958525

Engelhardt, N., and Hoydis, J. (2019). "Introduction: Connectivities between Literature and Science in the Twenty-First Century," in *Representations of Science in Twenty-First-Century Fiction: Human and Temporal Connectivities*. Editors N. Engelhardt and J. Hoydis (Cham: Palgrave Macmillan), 1–17. doi:10.1007/978-3-030-19490-1_1

Farzin, S. (2019). "Literatur als Quelle und Methode soziologischer Zeitdiagnose," in *Deutungsmacht von Zeitdiagnosen: Interdisziplinäre Perspektiven*. Editors H. Hastedt, H. Depner, and A. Maaser (Bielefeld: Transcript), 137–148. doi:10.14361/9783839445921-009

Felski, R. (2008). *Uses of Literature*. Malden, Oxford: Blackwell. doi:10.1002/9781444302790

Gaines, S. M., Kirchhofer, A., Schaffeld, N., Schimank, U., and Weingart, P. (2013). *Fiction Meets Science: Background and Concept*. Bremen: Universität Bremen.

Gaines, S. M., Farzin, S., and Haynes, R. D. (2021). "Introduction:," in *Under the Literary Microscope: Science and Society in the Contemporary Novel*. Editors S. Farzin, S. M. Gaines, and R. D. Haynes (University Park, Pennsylvania: Penn State University Press), 1–18. doi:10.5325/j.ctv1mvw8k2.4

Gaines, S. (2001). Sex, Love and Science. *Nature* 413, 255. doi:10.1038/35095130

Genus, A., and Stirling, A. (2018). Collingridge and the Dilemma of Control: Towards Responsible and Accountable Innovation. *Res. Pol.* 47, 61–69. doi:10.1016/j.respol.2017.09.012

Gibbons, M. (1999). Science's New Social Contract with Society. *Nature* 402, C81–C84. doi:10.1038/35011576

Gläser, J., and Schimank, U. (2014). "Autonomie als Resistenz gegen Beeinflussung: Forschungshandeln im organisatorischen und politischen Kontext„" in *Autonomie revisited: Beiträge zu einem umstrittenen Grundbegriff in Wissenschaft, Kunst und Politik*. Editors M. Franzen, A. Jung, D. Kaldewey, and J. Korte (Weinheim: Beltz Juventa), 45–65.

Gläser, J., Ash, M. G., Bunstorf, G., Hopf, D., Hubenschmid, L., Janßen, M., et al. (2020). *The Independence of Research – a Review of Disciplinary Perspectives and Outline of Interdisciplinary Prospects*. Berlin: Technical University of Berlin.

Goodman, A. (2010). *Intuition*. London: Atlantic Books. doi:10.21236/ada518604

Hackett, E. J. (2014). Academic Capitalism. *Sci. Technol. Hum. Values* 39, 635–638. doi:10.1177/0162243914540219

Haynes, R. D. (2016a). Bringing Science into Fiction. *Z. für Anglistik Amerikanistik* 64, 127–148. doi:10.1515/zaa-2016-0015

Haynes, R. D. (2016b). Whatever Happened to the 'mad, Bad' Scientist? Overturning the Stereotype. *Public Underst Sci.* 25, 31–44. doi:10.1177/0963662514535689

Herndon, L. (2010). Science, Meet Poetry. *Cell* 143, 1039. doi:10.1016/j.cell.2010.12.006

Hilgartner, S. (2017). *Reordering Life: Knowledge and Control in the Genomics Revolution*. Cambridge, Massachusetts: The MIT Press. doi:10.7551/mitpress/10481.001.0001

Jarvis, M. (2017). AAAS Statement on Scientific Freedom and Responsibility. *Science* 358, 462. doi:10.1126/science.358.6362.462-b10.1126/science.358.6362.462-a

Jasanoff, S. (2004). "The Idiom of Co-production," in *States of Knowledge: The Co-production of Science and Social Order*. Editor S. Jasanoff (London: Routledge), 1–12.

Jessop, B. (2018). On Academic Capitalism. *Crit. Pol. Stud.* 12, 104–109. doi:10.1080/19460171.2017.1403342

Kaldewey, D. (2013). *Wahrheit und Nützlichkeit: Selbstbeschreibungen der Wissenschaft zwischen Autonomie und gesellschaftlicher Relevanz*. Bielefeld: Transcript. doi:10.14361/transcript.9783839425657

Kalleberg, R. (2015). "Scientific Misconduct, Plagiarism, and Institutional Control of Misconduct," in *International Encyclopedia of the Social & Behavioral Sciences*. Editor J. D. Wright (Amsterdam: Elsevier), 21, 313–317. doi:10.1016/b978-0-08-097086-8.03022-1

Kalter, F. (2015). "Action, Theories of Social," in *International Encyclopedia of the Social & Behavioral Sciences*. Editor J. D. Wright (Amsterdam: Elsevier), 1, 75–79. doi:10.1016/b978-0-08-097086-8.32002-5

Kant, I. (2011). *Groundwork of the Metaphysics of Morals: A German-English Edition*. Cambridge: Cambridge University Press. doi:10.1017/cbo9780511973741

Kirchhofer, A., and Roxburgh, N. (2016). The Scientist as 'Problematic Individual' in Contemporary Anglophone Fiction. *Z. für Anglistik Amerikanistik* 64, 149–168. doi:10.1515/zaa-2016-0016

Knorr-Cetina, K. (1999). *Epistemic Cultures: How the Sciences Make Knowledge*. Cambridge, Massachusetts: Harvard University Press.

Koschorke, A. (2018). *Fact and Fiction: Elements of a General Theory of Narrative*. Berlin, Bonn: De Gruyter. doi:10.1515/9783110349689

Kuzmics, H., and Mozetič, G. (2003). *Literatur als Soziologie: Zum Verhältnis von literarischer und gesellschaftlicher Wirklichkeit*. Konstanz: UVK.

Lepenies, W. (1992). *Between Literature and Science: The Rise of Sociology*. Cambridge: Cambridge University Press.

Longhito, S. (2007). No Lab Is an Island: A Review of Intuition by AllegraGoodman (2006), Dial Press. *FASEB j.* 21, 2272. doi:10.1096/fj.07-0802ufm

Longo, M. (2015). *Fiction and Social Reality: Literature and Narrative as Sociological Resources*. Burlington, Vermont, Farnham: Surrey Ashgate.

Luhmann, N. (2013). *Theory of Society*, Vol. 2. Stanford, California: Stanford University Press.

Matthies, A. (2016). *Spielbälle: Neuverhandlungen der Arbeitswelt im Medium Literatur*. Köln: Herbert von Halem Verlag.

Merton, R. K. (1936). The Unanticipated Consequences of Purposive Social Action. *Am. Sociological Rev.* 1, 894–904. doi:10.2307/2084615

Merton, R. K. (1973). "The Normative Structure of Science," in *The Sociology of Science: Theoretical and Empirical Investigations*. Editor N. W. Storer (Chicago, Illinois: University of Chicago Press), 267–278.

Mirowski, P., and Sent, E.-M. (2008). "The Commercialization of Science and the Response of STS„" in *The Handbook of Science and Technology Studies*. Editor E. J. Hackett (Cambridge: The MIT Press), 635–689.

Münch, R. (1986). *Die Kultur der Moderne. Band 1: Ihre Grundlagen und ihre Entwicklung in England und Amerika*. Frankfurt am Main: Suhrkamp.

Münch, R. (2014). *Academic Capitalism. Universities in the Global Struggle for Excellence*. London, New York: Routledge. doi:10.4324/9780203768761

Offe, C. (1989). "Bindung, Fessel, Bremse: Die Unübersichtlichkeit von Selbstbeschränkungsformeln„" in *Zwischenbetrachtungen: Im Prozeß der Aufklärung. Jürgen Habermas zum 60. Geburtstag*. Editors A. Honneth, T. McCarthy, C. Offe, and A. Wellmer (Frankfurt am Main: Suhrkamp), 739–774.

Panofsky, A. L. (2010). "7 A Critical Reconsideration of the Ethos and Autonomy of Science," in *Robert K. Merton: Sociology of Science and Sociology as Science*. Editor C. J. Calhoun (New York: Columbia University Press), 140–163. doi:10.7312/calh15112-007

Parsons, T., and Platt, G. M. (1973). *The American University*. Cambridge, Massachusetts: Harvard University Press. doi:10.4159/harvard.9780674423626

Pilkington, O. A. (2017). Popular Science versus Lab Lit: Differently Depicting Scientific Apparatus. *Sci. as Cult.* 26, 285–306. doi:10.1080/09505431.2016.1255722

Pilkington, O. A. (2019). "Introduction: What's in a Name?," in *Lab Lit: Exploring Literary and Cultural Representations of Science*. Editors O. A. Pilkington and A. G. Pilkington (Lanham, Maryland: Lexington Books), 1–12.

Polanyi, M. (1945). The Autonomy of Science. *The Scientific Monthly* 60, 141–150.

Renn, J. (2020). *The Evolution of Knowledge: Rethinking Science for the Anthropocene*. Princeton, Oxford: Princeton University Press. doi:10.2307/j.ctvdf0kpk

Resnik, D. B. (2009). *Playing Politics with Science: Balancing Scientific Independence and Government Oversight*. Oxford: Oxford University Press. doi:10.1093/acprof:oso/9780195375893.001.0001

Robinson, K. S. (2004). *Forty Signs of Rain*. New York: Bantam Books.

Rohn, J. (2008). Abstractions. *Nature* 451, ix. doi:10.1038/7175ixb

Rohn, J. (2010a). More Lab in the Library. *Nature* 465, 552. doi:10.1038/465552a

Rohn, J. (2010b). *The Honest Look*. New York: Cold Spring Harbor Laboratory Press.

Rosa, H. (2020). *The Uncontrollability of the World*. Cambridge, UK, Medford, MA: Polity Press.

Roxburgh, N. (2015). *Representing Public Credit: Credible Commitment, Fiction, and the Rise of the Financial Subject*. Abingdon, Oxon: Taylor & Francis. doi:10.4324/9781315646398

Schaffeld, N. (2016). Aspects of the Science Novel. *Z. für Anglistik Amerikanistik* 64, 121–125. doi:10.1515/zaa-2016-0014

Schimank, U., and Volkmann, U. (2017). *Das Regime der Konkurrenz: Gesellschaftliche Ökonomisierungsdynamiken heute*. Weinheim, Basel: Beltz Juventa.

Schimank, U. (2014). "Planung versus Evolution: Wie verändert sich das Soziale?," in *Handbuch der Soziologie*. Editors J. Lamla, H. Laux, H. Rosa, and D. Strecker (Konstanz: UTB), 116–130.

Schimank, U. (2015a). "Differentiation: Social," in *International Encyclopedia of the Social & Behavioral Sciences*. Editor J. D. Wright (Amsterdam: Elsevier), 6, 391–394. doi:10.1016/b978-0-08-097086-8.32040-2

Schimank, U. (2015b). Modernity as a Functionally Differentiated Capitalist Society. *Eur. J. Soc. Theor.* 18, 413–430. doi:10.1177/1368431014543618

Schimank, U. (2016). *Handeln und Strukturen: Einführung in die akteurtheoretische Soziologie*. Weinheim, München: Beltz Juventa.

Schimank, U. (2021). "Economization of Science: Insights from Science Novels," in *Under the Literary Microscope: Science and Society in the Contemporary Novel*. Editors S. Farzin, S. M. Gaines, and R. D. Haynes (University Park, Pennsylvania: Penn State University Press), 148–172. doi:10.5325/j.ctv1mvw8k2.11

Schomberg, R. von. (2013). "A Vision of Responsible Research and Innovation," in *Responsible Innovation: Managing the Responsible Emergence of Science and Innovation in Society*. Editors R. Owen, J. Bessant, and M. Heintz (Chichester: Wiley), 51–74.

Schreier, M. (2017). *Qualitative Content Analysis in Practice*. London: SAGE.

Sennett, R. (1999). *The Corrosion of Character: The Personal Consequences of Work in the New Capitalism*. New York City, New York: Norton.

Sevänen, E. (2018). Modern Literature as a Form of Discourse and Knowledge of Society. *Sociologias* 20, 48–85. doi:10.1590/15174522-020004803

Sismondo, S. (2021). Epistemic Corruption, the Pharmaceutical Industry, and the Body of Medical Science. *Front. Res. Metr. Anal.* 6, 614013. doi:10.3389/frma.2021.614013

Stehr, N. (2015). "Knowledge Society, History of," in *International Encyclopedia of the Social & Behavioral Sciences*. Editor J. D. Wright (Amsterdam: Elsevier), 13, 105–110. doi:10.1016/b978-0-08-097086-8.03160-3

Stephan, P. E. (1996). The Economics of Science. *J. Econ. Lit.* 34, 1199–1235.

Stilgoe, J., and Guston, D. H. (2017). "Responsible Research and Innovation,," in *The Handbook of Science and Technology Studies*. Editors U. Felt, R. Fouché, C. A. Miller, and L. Smith-Doerr (Cambridge, Massachusetts: The MIT Press), 853–880.

Thomas, C. (2006). Getting Away with it. *Nat. Med.* 12, 1235. doi:10.1038/nm1106-1235

Váňa, J. (2020). Theorizing the Social through Literary Fiction: For a New Sociology of Literature. *Cult. Sociol.* 14, 180–200.

Vogl, J. (2014). *The Specter of Capital*. Stanford, California: Stanford University Press.

Wagner, P. (2008). *Modernity as Experience and Interpretation*. Cambridge, Malden: Polity.

Wagner, P. (2012). *Modernity: Understanding the Present*. Cambridge, Malden: Polity.

Wagner, P. (2016). *Progress: A Reconstruction*. Cambridge, UK, Malden, Massachusetts: Polity.

Whitley, R. (2014). "How Do Institutional Changes Affect Scientific Innovations? the Effects of Shifts in Authority Relationships, Protected Space, and Flexibility," in *Organizational Transformation and Scientific Change: The Impact of Institutional Restructuring on Universities and Intellectual Innovation*. Editors R. Whitley and J. Gläser (Bingley: Emerald Group Publishing Limited), 367–406. doi:10.1108/s0733-558x_2014_0000042012

Wilholt, T., and Glimell, H. (2011). "Conditions of Science: The Three-Way Tension of Freedom, Accountability and Utility," in *Science in the Context of Application*. Editors M. Carrier and A. Nordmann (Dordrecht: Springer Science+Business Media B.V), 351–370. doi:10.1007/978-90-481-9051-5_21

Wilholt, T. (2012). *Die Freiheit der Forschung: Begründungen und Begrenzungen*. Berlin: Suhrkamp.

Znaniecki, F. (1934). *The Method of Sociology*. New York City, New York: Rinehart & Company.

Znaniecki, F. (1952). *Cultural Sciences: Their Origin and Development*. Champaign, Illinois: University of Illinois Press.

Zwart, H. (2017). *Tales of Research Misconduct: A Lacanian Diagnostics of Integrity Challenges in Science Novels*. Cham: Springer International Publishing. doi:10.1007/978-3-319-65554-3

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

frontiers in Research Metrics and Analytics

OPINION
published: 18 March 2022
doi: 10.3389/frma.2022.777781

How to Improve Research Funding in Academia? Lessons From the COVID-19 Crisis

*Vlasta Sikimić**

Weizsäcker Center, University of Tübingen, Tübingen, Germany

Keywords: public funding, academic misconduct, elitism, life sciences, COVID-19

INTRODUCTION

The current COVID-19 crisis has put both public and private funding of life sciences in the spotlight. One of the most frequent critiques of the scientific research conducted in industry is that researchers working for companies lack intellectual freedom. Moreover, from the perspective of the general public, industry research is always questioned because monetary interests might influence it. Sponsorship bias—a tendency of researchers working in the private sector to align their results with the interest of their funders—has been widely discussed in philosophy of science (e.g., Holman and Elliott, 2018; Leefmann, 2021). Some authors even go as far as opposing intellectual property in life sciences (Brown, 2008). Having all this in mind, epistemic trust in research conducted by companies is often lacking. However, it is questionable whether the academic sector alone, in its current state, can appropriately respond to global challenges. I argue that academic research requires substantial restructuring as similar objections can be raised both in the case of research done by academic institutions and in industry. Additionally, there are specific dangers connected with the current academic system such as elitism in science that are epistemically harmful. Though similar tendencies can also be detected in industry, academia has its own outdated rules that are reflected in its current culture.

It is important to note that not only academic institutions are publicly funded. Industry in certain contexts is also funded publicly, e.g., Moderna received almost one billion dollars from public sources for the development of its COVID-19 vaccine (Hussey, 2020). Different research schemes work better in certain contexts, but worse in others. In this sense, responsible science funding should be context-oriented.

When it comes to vaccine development, we witnessed many different funding approaches. For example, the Sputnik V vaccine was developed by a governmental institution. On the other end of the spectrum, the development of the BioNTech-Pfizer vaccine was mainly supported by pre-orders. Interestingly, the BioNTech-Pfizer collaboration even decided against taking funds from the US government to avoid the associated bureaucracy. Sinovac is another example for a private company that developed a COVID-19 vaccine without governmental funds. In the middle, we see public-private partnerships such as the joint-venture Sinopharm, the collaboration between the University of Oxford and AstraZeneca, and Moderna, a private company that received millions of dollars and logistic support from the US government.

In the context of mixed funding, it makes sense to ask whether certain academic institutions also support private interests. Moreover, publicly funded academic institutions might still have interests on their own, for instance building a reputation, being competitive, and financially profiting from that. From the perspective of individual researchers, the highly competitive nature of research in academia is fruitful ground for academic misconduct (e.g., Cartwright and Menezes, 2014). Furthermore, the publish or perish culture and limited contracts often motivate researchers

Edited by:
Jon Leefmann,
University of Erlangen Nuremberg, Germany

Reviewed by:
Elena Popa,
Flame University, India
Lena Woydack,
Charité Medical University of Berlin, Germany

***Correspondence:**
Vlasta Sikimić
vlasta.sikimic@uni-tuebingen.de

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *15 September 2021*
Accepted: *21 February 2022*
Published: *18 March 2022*

Citation:
Sikimić V (2022) How to Improve Research Funding in Academia? Lessons From the COVID-19 Crisis. Front. Res. Metr. Anal. 7:777781. doi: 10.3389/frma.2022.777781

to switch for a position in industry in which such existential pressures are not present (cf. Hayter and Parker, 2019).

There is a serious concern that academic prestige and elitism in both publicly and privately funded academic institutions have severe negative epistemic consequences. By elitist nature of science, I mean a broadly understood social construct where researchers with privileged backgrounds are favored over others. This extends to scientists belonging to a specific research institution, gender, origin, career stage, and other privileged groups. In this context, elitism affects both academics from the Global South and the ones employed by less prominent institutions in Western countries. As a result of elitism, the contribution of these researchers is not given equal weight as the input from the ones working in more famous, older or richer institutions.

During the pandemic, highly effective vaccines were developed in many different countries, including Russia and China. Moreover, we have to fight the pandemic in every country and every country needs the capacities to diagnose the disease, the experts to advise the government, and the ability to participate in clinical studies and vaccination campaigns. This emphasizes the need for epistemic decolonization as a prerequisite for a globalized academic effort.

Finally, the transparency of both academic and industry-related results is the key to building the necessary epistemic trust in science. This transparency is related both to the research data and the replicability of the results, as well as to the proper communication with the general public. A critical perspective is a necessary corrective requirement to make responsible scientific decisions and future improvements.

I will raise three arguments relevant for this debate. Firstly, research and development of COVID-19 vaccines is organized in various ways and most often involves a mixed funding approach involving public and private sponsors. Secondly, when assessing the epistemic consequences of mixed funding approaches a focus on industry sponsorship is one-sided. One also needs to take into account the non-epistemic interests of publicly funded research institutions and individual researchers. As I will point out, the working conditions for researchers in academia pose a constant threat to good scientific practice. Finally, I will argue that an attitude of elitism in both public and private research institutions and practices of epistemic colonization are major obstacles for reaching optimal decisions with regard to global health threats.

THE INTERPLAY BETWEEN PRIVATE AND PUBLIC FUNDING

Public and private funding do not always follow objective criteria. In this section, I will discuss how government spending is frequently not correlated with disease burden, neither on the global nor the national level, and how public funding can be awarded or withdrawn based on non-objective evaluations. Furthermore, I will highlight how governmental export restrictions influence the distribution of vaccines and protective equipment. Finally, I will use vaccine manufacturing as an example where private resources are needed to address a public health emergency.

Especially within the healthcare sector, private funding and patents have been extensively criticized (e.g., Bekelman et al., 2003; Brown, 2008). The reasons for this are manifold. One aspect concerns the focus on diseases typically encountered in richer countries, such as cardiovascular diseases and cancer (Trouiller et al., 2002). Companies typically invest more money into diseases that promise the highest revenues. However, also public funding is not only driven by disease burden—neither from the national nor the global perspective. Gillum et al. (2011) analyzed NIH funding for the year 2006 and found that the disease burden in the US only explains about one-third of the funding. While, for example, Diabetes mellitus received more funding than explained by the disease burden, research on depression received less than expected. Hence, neither public nor private entities necessarily focus on the most relevant issues, but instead on their own agendas.

From the perspective of the COVID-19 crisis, it is interesting to note that vaccines belong to a significantly underfunded category. In 2000, multinational vaccine companies invested <1 billion in the research and development of vaccines, which is <3% of their spending on pharmaceuticals (Régnier and Huels, 2013).

The objectivity of publicly funded science can be influenced by pressure to serve private interests, while academic institutions adopt cultures from the private sector (Azmanova, 2020). Even the selection of projects can already be skewed toward industry. To increase the (direct) applicability of research, some funding schemes require the involvement of private companies. Azmanova (2020) uses the example of Horizon 2020 to show that such programs do not only offload the investment risks to the society, while resulting patents are owned by companies, but also allow them to steer the research direction. In addition to influencing research agendas, some companies also manipulate the scientific discourse. For example, Monsanto sponsored ghostwriting in toxicology journals, influencing the opinion on its herbicide glyphosate (McHenry, 2018). As a long-term result, public trust in science gets challenged.

Different types of pressure can negatively influence the objectivity of researchers during the scientific process. For example, the evaluation of researchers based on publications, citations, and grants in combination with short-term contracts can negatively affect academic freedom (Zimmer, 2015). In addition, public funding holds sufficient examples of political interference into research agendas. Recently, the NIH canceled a program studying coronaviruses which was ongoing since 2014 due to political pressure (Rosenthal et al., 2020).

Currently, the standard division between research done in academia and industry is that applied research is done by industry while the foundational questions are tackled by academics. Public funding for research and development contributes up to two-thirds of the costs for developing drugs (Annett, 2021). In the future, to increase the robustness of the system and the possibility to develop medications cheaper and faster in the face of new challenges, more applied research could be done in academia. Finally, whether the development of infrastructure for mass drug

production and their distribution should be publicly funded remains a question for political theory.

It is important to distinguish between research conducted in academia and research conducted in industry and to keep in mind that this distinction is not equivalent to the distinction between public and private funding. Companies can also be funded by governments (depending on the political system of the country), while many academic institutions are privately funded. Furthermore, scientists working in academia funded by the public sector sometimes also receive grants from private companies or foundations. Thus, to the argument that privately funded researchers lack academic freedom because they either explicitly or implicitly depend on the interest of the investors (Bekelman et al., 2003) also applies to researchers working in academia who are funded by private sources. However, this argument will not hold for the researchers working in publicly funded companies.

During the COVID-19 crisis, the private sector has been perceived as harmful for the distribution of vaccines to the Global South, because the profit leaned toward the countries that were paying the most. This in turn results in human casualties, even greater inequalities between countries, and increased mutation potential of the virus which in turn affects the whole world. On the other hand, the distribution of fully publicly funded vaccines is also not based on the idea of equity and patents prevent production in other countries. The EU, India, and the US, and thereby most vaccine-developing countries, have imposed export restrictions on vaccines or ingredients (Ibrahim, 2021). In addition, only a few countries allow foreigners to be vaccinated, even in places where there is an abundance of vaccines. In order to overcome such problems, a shift in the international arena would need to happen, facilitating the transition from a self-centered competitive model to a collaborative model that promotes solidarity between countries. Thus, in this context life sciences research should be understood as a global endeavor.

In vaccine development during the COVID-19 pandemic, we witnessed collaborations from the private and public sphere, industry, and academia. One of the reasons is that the infrastructure for the production and distribution of large amounts of vaccine doses was provided by industries that have such resources and capacities. On the other hand, research and development of new drugs, vaccine techniques, and treatments might not be overly profitable from the perspective of big pharma companies that sometimes prefer to outsource these activities and buy tested products from smaller players.

IS THE CURRENT ACADEMIC SETTING WORSE THAN THE INDUSTRIAL ONE?

To understand what motivates researchers to move from academia to industry, one has to compare working conditions for highly educated workers such as life scientists in both. A recent survey with more than 3,000 researchers as participants revealed a much higher satisfaction of scientists working in industry than in academia (Woolston, 2021). Researchers from industry feel more optimistic about their careers. A difference in job satisfaction was also detected between participants with permanent jobs and the ones with fixed-term contracts (Woolston, 2021). Industry offers well-paid permanent positions which allow for security, future planning, and general stability in life. In academia, temporary contracts are dominant and often one cannot even choose a place of living easily. Additionally, since there are more temporary junior than permanent senior positions for life scientists in academia, many scholars will not get the opportunity to become professors (Hayter and Parker, 2019). The general atmosphere in academia is highly competitive and this leads to numerous problems. The academic culture is often described as masculine (Gonsalves et al., 2016), and the reduced promotion of females reflects in the so-called leaky pipeline. The leaky pipeline means that women get less frequently promoted into higher positions and more frequently leave academia (Blickenstaff, 2005).

Some of the implicit rules of academia reflect its traditional, masculine, and retrograde setup in which junior researchers are dependent on their supervisors, success is not always objectively attributed, traditional elitist discrimination is in place, etc. In academia (self)exploitation of researchers is frequently justified with the love for science and the freedom it promises (Busso and Rivetti, 2014; Woolston, 2021). Furthermore, Zheng (2018) identified the idea that academics work for their own reward as a myth primarily sustained by the lucky few with stable employment. For these reasons, young researchers in life sciences often turn from academia to industry (cf. Hayter and Parker, 2019). In the private sector profit is the main parameter that drives success, while in academia early-career researchers are dependent on the evaluation of their group leaders. If the group leader is problematic, e.g., exploitative or oppressive, it might be difficult to make any change in the academic setting where senior researchers are often hard to suspend or replace. In contrast, private companies usually employ a professional HR and monitor the performance of the supervisors.

The pressure to publish, spearheaded by job uncertainty, can lead to violations of research standards. Bibliographic data is often used as the most important parameter for evaluating scientific and academic achievements. Thus, scholarships, jobs, academic positions, and research funding are dependent on the publication record (e.g., Bird, 2006; Bedeian et al., 2009). In order to meet the very high publishing standards, scientists might turn to different types of violations of research conduct, such as publishing insufficiently supported results, double publishing, self-plagiarism, producing "minimal publishable unit" (Neill, 2008), etc. Tijdink et al. (2014) showed that publication pressure among European medical scientists strongly correlates with scientific misconduct. Moreover, 72% of the participants in the study evaluated the publication pressure as too high, while 15% of them confessed that they had participated in the fabrication, falsification, or manipulation of data in the previous 3 years. It should, of course, be noted that academic fraud is not limited to junior researchers with insecure job perspectives. Based on focus-group discussions with more than 50 scientists, Anderson et al. (2007) identified competition and the "winner-take-all-approach" as a driver for scientific misconduct. Fang et al. (2013) analyzed the demographic data from the United States Office

of Research Integrity which oversees misconduct investigations. Among the 228 scientists who committed misconduct, males were overrepresented, particular among faculty members.

Le Maux et al. (2019) used formal modeling to show that the monetary award of publishing in an influential journal increases academic misconduct. They concluded that if one wants to positively influence scientific output, publications in lower-ranked journals should also be rewarded.

One of the well-known examples of scientific misconduct with severe impact on society is a 1998 study by Wakefield and his co-authors published in *The Lancet*. The study fraudulently reported an MMR vaccine-induced syndrome characterized by chronic gastrointestinal symptoms and autism (Flaherty, 2011; Godlee et al., 2011). After the study became known to the general public, parents' distrust in the vaccination program increased, causing more parents to refuse to vaccinate their children. Vaccination rates in the UK fell from 91% in 1998 to <80% in 2003 (Flaherty, 2011). As a result of a lack of immunity, measles outbreaks began to occur in the UK. This example shows that academic research can also have severe negative consequences on trust in science.

The paper was based on 12 children, who were selected in favor of families reporting an association between autism and the MMR vaccine, and relied on parental recall and beliefs (Flaherty, 2011; Godlee et al., 2011). Furthermore, Wakefield received ∼$670 000 from attorneys of families allegedly harmed by vaccines and held a patent on a new vaccine (Flaherty, 2011). Wakefield and his team were found to be in a conflict of interest, while the data presented in the publication were considered fraudulent (Godlee et al., 2011). Even though many studies refute the association between autism and MMR (e.g., Taylor et al., 1999; Farrington et al., 2001; Takahashi et al., 2003), and the article itself was withdrawn in 2010, the impact of Wakefield's article is still present because the public confidence in the safety of vaccination has been compromised. One reason for the large impact of Wakefield's study, which was immediately criticized by the scientific community, was his marketing strategy, involving a public relations company and press conferences (Irzik and Kurtulmus, 2019). Moreover, about half of the media coverage about the alleged link between the MMR vaccine and autism gave equal weight to his claims and the scientific consensus, while about one third only reported his claims (Irzik and Kurtulmus, 2019). In combination with his authority as a doctor at a respected hospital and the fame of one of the most influential medical journals, all these factors all contributed to the impact of his claims.

There are also indirect and long-term consequences of this publication reflected in the loss of confidence in the epistemic authority of scientists. Moreover, not only may the general public lose trust in the epistemic authority of scientists, but other scientists may also lose trust in their peers. Wakefield's and colleagues' publication can be considered the individual case of scientific misconduct with the largest negative impact on public health. This enormous impact is partially caused by the image of academic researchers as objective and impartial observers.

In order to decrease academic misconduct, one should work on the improvements of work conditions in academia and offer permanent contracts comparable to those in industry. The creative process in science cannot be easily stimulated externally, but certain conditions influence the research output. Directing funds into a system that promotes research quality and academic honesty instead of hyperproduction and competitiveness would make academia more apt to respond to global challenges. This also includes more opportunities for researchers from less-known research centers by financing their projects.

The importance of including researchers from all countries in the scientific discourse together with their diverse perspectives becomes particularly salient in the context of global challenges. The elitist nature of academia makes epistemic inclusion of underprivileged groups more difficult. While funding can promote international collaboration among researchers, elitism remains a challenge that needs to be overcome by changing the academic culture.

DANGERS OF ELITISM IN SCIENCE

The danger of the elitist approach in science is that researchers from less famous scientific communities are discriminated. This can also have strong practical consequences. As part of the worldwide immunization during the COVID-19 pandemic, we witnessed that the European Medicines Agency (EMA) did not approve all the vaccines that the World Health Organization (WHO) approved, resulting in confusing policies, increased sense of inequality, skepticism toward certain vaccines, etc. In this way important results from less "prestigious" academic institutions may get hindered, fewer funds will be allocated to them, which again would enhance the current epistemic colonization. For example, in some European countries, foreigners immunized with all WHO approved vaccines are considered vaccinated, while others only accept EMA approved vaccines.

Epistemic colonization stands for imposing dominant epistemic attitudes and solutions to parts of the world which would originally have different epistemic tendencies. Mitova explains that the background assumption of epistemic colonization is that there can only be one best approach to science. The Global North, under this pretext, prescribes what counts as a rational and scientific solution disregarding the differences in the cultural context (Mitova, 2020). An important reason for fostering science in all institutions in a manner of equity is that for some solutions it is important to know local circumstances. For example, in 2018, a person was killed by his neighbors after he received a vaccine against Ebola because his neighbors thought he was infectious and would bring Ebola to their area. To avoid this risk, the WHO changed its vaccination strategy and gave people the option to be vaccinated in neighboring towns (Maxmen, 2019). The epistemic solutions and healthcare measures thus cannot just be imposed without prior knowledge of local circumstances. Collaborating with the local community and considering the local knowledge and beliefs in decision making is an important part of epistemic decolonization and can improve the effectiveness of public health care measures. For example, Liaw et al. (2011) established the utilization of local knowledge and community engagement as prerequisites for chronic disease care of Aborigine Australians.

Similarly, the 2014 Lancet Commission on Culture and Health identified the systematic neglect of culture as the biggest barrier to the advancement of health care (Napier et al., 2014).

To respond to global challenges, we need a coordinated global strategy which requires an inclusive and stimulating environment with a non-elitist approach. As a response to epistemic injustice, Anderson (2012) argues in favor of equality as a virtue of institutions. This should be strengthened by the request for equity which means supporting marginalized groups with positive actions. In science, this means trust, respect, and financial support for researchers irrespective of their country of origin and taking affirmative actions when necessary.

CONCLUSIONS

Though private funding of life sciences has been criticized as epistemically vulnerable and rightly so, I pointed out that simply turning to public funding of academia will not solve all the problems such as academic misconduct, biases in science, and its elitist nature. On the contrary, research in academia would benefit from significant restructuring to deal with global challenges that require fast solutions. Though privately funded bodies can have selfish incentives, countries themselves as fund providers can be governed by their egoistic motives. Moreover, in order to make academia more epistemically efficient, funding that would allow for permanent contracts for early-career academics would be beneficial. Since global challenges require coordinated action from all over the world and since science is a collaborative process, decreasing the elitist nature of science through funding diverse research from less known countries and institutions would bring positive results. These points were already known in epistemology of science, but the COVID-19 pandemic made them even more prominent and urgent.

Some of the ways of increasing the diversity in academia and strengthening international ties are funding schemes that promote global collaborations. These collaborations need to be constructed in a socially and epistemically just manner so that researchers from the Global South can take lead in projects instead of having marginal roles, e.g., data collection (Koskinen and Rolin, 2021). It is especially important to foster an inclusive academic environment where researchers from the Global South get fair acknowledgment and empowerment. Finally, funding agencies should promote a different academic culture which will allow researchers from currently underprivileged groups to be equally represented and flourish over time.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

REFERENCES

Anderson, E. (2012). Epistemic justice as a virtue of social institutions. *Soc. Epistemol.* 26, 163–173. doi: 10.1080/02691728.2011.652211

Anderson, M. S., Ronning, E. A., De Vries, R., and Martinson, B. C. (2007). The perverse effects of competition on scientists' work and relationships. *Sci. Eng. Ethics* 13, 437–461. doi: 10.1007/s11948-007-9042-5

Annett, S. (2021). Pharmaceutical drug development: high drug prices and the hidden role of public funding. *Biol. Futura* 72, 129–138. doi: 10.1007/s42977-020-00025-5

Azmanova, A. (2020). *Capitalism on Edge*. Columbia: Columbia University Press. doi: 10.7312/azma19536

Bedeian, A. G., Van Fleet, D. D., and Hyman, H. H. (2009). Scientific achievement and editorial board membership. *Organ. Res. Methods* 12, 211–238. doi: 10.1177/1094428107309312

Bekelman, J. E., Li, Y., and Gross, C. P. (2003). Scope and impact of financial conflicts of interest in biomedical research: a systematic review. *JAMA* 289, 454–465. doi: 10.1001/jama.289.4.454

Bird, S. J. (2006). Research ethics, research integrity, and the responsible conduct of research. *Sci. Eng. Ethics* 12, 411–412. doi: 10.1007/s11948-006-0040-9

Blickenstaff, C. J. (2005). Women and science careers: leaky pipeline or gender filter? *Gend. Educ.* 17, 369–386. doi: 10.1080/09540250500145072

Brown, J. R. (2008). Politics, method, and medical research. *Philos. Sci.* 75, 756–766. doi: 10.1086/594521

Busso, S., and Rivetti, P. (2014). What's love got to do with it? Precarious academic labour forces and the role of passion in Italian universities. *Rech. Sociol. Anthropol.* 45, 15–37. doi: 10.4000/rsa.1243

Cartwright, E., and Menezes, M. L. (2014). Cheating to win: dishonesty and the intensity of competition. *Econ. Lett.* 122, 55–58. doi: 10.1016/j.econlet.2013.10.016

Fang, F. C., Bennett, J. W., and Casadevall, A. (2013). Males are overrepresented among life science researchers committing scientific misconduct. *MBio* 4, e00640–e00612. doi: 10.1128/mBio.00640-12

Farrington, C. P., Miller, E., and Taylor, B. (2001). MMR and autism: further evidence against a causal association. *Vaccine* 19, 3632–3635. doi: 10.1016/S0264-410X(01)00097-4

Flaherty, D. K. (2011). The vaccine-autism connection: a public health crisis caused by unethical medical practices and fraudulent science. *Ann. Pharmacother.* 45, 1302–1304. doi: 10.1345/aph.1Q318

Gillum, L. A., Gouveia, C., Dorsey, E. R., Pletcher, M., Mathers, C. D., McCulloch, C. E., et al. (2011). NIH disease funding levels and burden of disease. *PLoS ONE* 6, e16837. doi: 10.1371/journal.pone.0016837

Godlee, F., Smith, J., and Marcovitch, H. (2011). Wakefield's article linking MMR vaccine and autism was fraudulent. *BMJ* 342:c7452. doi: 10.1136/bmj.c7452

Gonsalves, A. J., Danielsson, A., and Pettersson, H. (2016). Masculinities and experimental practices in physics: The view from three case studies. *Phys. Rev. Phys. Educ. Res.* 12:020120. doi: 10.1103/PhysRevPhysEducRes.12.020120

Hayter, C. S., and Parker, M. A. (2019). Factors that influence the transition of university postdocs to non-academic scientific careers: An exploratory study. *Res. Policy* 48, 556–570. doi: 10.1016/j.respol.2018.09.009

Holman, B., and Elliott, K. C. (2018). The promise and perils of industry-funded science. *Philosophy Compass* 13:e12544. doi: 10.1111/phc3.12544

Hussey, C. (2020). *Moderna Announces Expansion of BARDA Agreement to Support Larger Phase 3 Program for Vaccine (mRNA-1273) Against COVID-19*. Available online at: https://investors.modernatx.com/news/news-details/2020/Moderna-Announces-Expansion-of-BARDA-Agreement-to-Support-Larger-Phase-3-Program-for-Vaccine-mRNA-1273-Against-COVID-19-07-26-2020/default.aspx

Ibrahim, I. A. (2021). Overview of export restrictions on COVID-19 vaccines and their components. *Insights* 25:10. Available online at: https://www.asil.org/insights/volume/25/issue/10

Irzik, G., and Kurtulmus, F. (2019). What is epistemic public trust in science? *Br. J. Philos. Sci.* 70, 1145–1166 doi: 10.1093/bjps/axy007

Koskinen, I., and Rolin, K. (2021). "Structural epistemic (in) justice in global contexts," in *Global Epistemologies and Philosophies of Science* (Abingdon, Oxfordshire; NewYork, NY: Routledge). doi: 10.4324/9781003027140-12

Le Maux, B., Necker, S., and Rocaboy, Y. (2019). Cheat or perish? A theory of scientific customs. *Res. Policy* 48:1. doi: 10.1016/j.respol.2019.05.001

Leefmann, J. (2021). How to assess the epistemic wrongness of sponsorship bias? The case of manufactured certainty. *Front. Res. Metrics Analyt.* 6, 14. doi: 10.3389/frma.2021.599909

Liaw, S. T., Lau, P., Pyett, P., Furler, J., Burchill, M., Rowley, K., et al. (2011). Successful chronic disease care for Aboriginal Australians requires cultural competence. *Aust. N. Z. J. Public Health* 35, 238–248. doi: 10.1111/j.1753-6405.2011.00701.x

Maxmen, A. (2019). Science under fire: ebola researchers fight to test drugs and vaccines in a war zone. *Nature* 572, 16–17. doi: 10.1038/d41586-019-02258-4

McHenry, L. B. (2018). The Monsanto Papers: poisoning the scientific well. *Int. J. Risk Safety Med.* 29, 193–205. doi: 10.3233/JRS-180028

Mitova, V. (2020). Decolonising knowledge here and now. *Philoso. Papers* 49, 191–212. doi: 10.1080/05568641.2020.1779606

Napier, A. D., Ancarno, C., Butler, B., Calabrese, J., Chater, A., Chatterjee, H., et al. (2014). Culture and health. *Lancet* 384, 1607–1639. doi: 10.1016/S0140-6736(14)61603-2

Neill, U. S. (2008). Publish or perish, but at what cost? *J. Clin. Investig.* 118, 2368–2368. doi: 10.1172/JCI36371

Régnier, S. A., and Huels, J. (2013). Drug versus vaccine investment: a modelled comparison of economic incentives. *Cost Effect. Resour. Allocation* 11, 1–9. doi: 10.1186/1478-7547-11-23

Rosenthal, P. J., Bausch, D. G., Goraleski, K. A., Hill, D. R., Jacobson, J. A., John, C. C., et al. (2020). Keep politics out of funding decisions for medical research and public health. *Am. J. Trop. Med. Hyg.* 103, 931–932. doi: 10.4269/ajtmh.20-0850

Takahashi, H., Suzumura, S., Shirakizawa, F., Wada, N., Tanaka-Taya, K., Arai, S., et al. (2003). An epidemiological study on Japanese autism concerning routine childhood immunization history. *Japan. J. Infect. Dis.* 56, 114–117. Available online at: https://www.niid.go.jp/niid/images/JJID/56/114.html

Taylor, B., Miller, E., Farrington, C. P., Petropoulos, M. C., Favot-Mayaud, I., Li, J., et al. (1999). Autism and measles, mumps, and rubella vaccine: no epidemiological evidence for a causal association. *Lancet* 353, 2026–2029. doi: 10.1016/S0140-6736(99)01239-8

Tijdink, J. K., Verbeke, R., and Smulders, Y. M. (2014). Publication pressure and scientific misconduct in medical scientists. *J. Emp. Res. Hum. Res. Ethics* 9, 64–71. doi: 10.1177/1556264614552421

Trouiller, P., Olliaro, P., Torreele, E., Orbinski, J., Laing, R., and Ford, N. (2002). Drug development for neglected diseases: a deficient market and a public-health policy failure. *Lancet* 359, 2188–2194. doi: 10.1016/S0140-6736(02)09096-7

Woolston, C., (2021). Stagnating salaries present hurdles to career satisfaction. *Nature* 599, 519–521. doi: 10.1038/d41586-021-03041-0

Zheng, R. (2018). Precarity is a feminist issue: Gender and contingent labor in the academy. *Hypatia* 33, 235–255. doi: 10.1111/hypa.12401

Zimmer, R. J. (2015). *What is Academic Freedom For? In Who's Afraid of Academic Freedom?* Columbia: Columbia University Press. doi: 10.7312/columbia/9780231168809.003.0011

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's Note: All claims expressed in this article are solely those of the authors and do not necessarily represent those of their affiliated organizations, or those of the publisher, the editors and the reviewers. Any product that may be evaluated in this article, or claim that may be made by its manufacturer, is not guaranteed or endorsed by the publisher.

frontiers | Frontiers in Research Metrics and Analytics

HYPOTHESIS AND THEORY
published: 27 April 2022
doi: 10.3389/frma.2022.775336

Addressing Conflicts of Interest and Conflicts of Commitment in Public Advocacy and Policy Making on CRISPR/Cas-Based Human Genome Editing

*Alexander Christian**

DCLPS, Institute of Philosophy, Heinrich-Heine-University, Duesseldorf, Germany

Leading experts on CRISPR/Cas-based genome editing—such as 2020 Nobel laureates Jennifer Doudna and Emmanuelle Charpentier—are not only renowned specialists in their fields, but also public advocates for upcoming regulatory frameworks on CRISPR/Cas. These frameworks will affect large portions of biomedical research on human genome editing. In advocating for particular ways of handling the risks and prospects of this technology, high-profile scientists not only serve as scientific experts, but also as moral advisers. The majority of them currently intend to bring about a "responsible pathway" toward human genome interventions in clinical therapy. Engaging in advocacy for such a pathway, they issue moral judgments on the risks and benefits of this new technology. They declare that there actually is a responsible pathway, they draft resolutions on temporary moratoria, they make judgments on which groups and individuals are credible and should participate in public and semi-public debates, so they also set the standards for deciding who counts as well-informed, as well as the standards of evidence for adopting or rejecting research policies. This degree of influence on public debates and policy making is, at the very least, noteworthy. This contribution sounds a note of caution with regard to the endeavor of a responsible pathway to human genome editing and in particular scrutinizes the legitimacy of expert-driven research policies given commercial conflicts of interest and conflicts of commitment among first-rank scholars.

Keywords: conflict of interest, conflict of commitment, CRISPR/Cas, policy making, human genome editing

OPEN ACCESS

Edited by:
Robert Ranisch,
University of Potsdam, Germany

Reviewed by:
Oliver Feeney,
University of Tübingen, Germany
Kirstin R. W. Matthews,
Rice University, United States

***Correspondence:**
Alexander Christian
alexander.christian@hhu.de

Specialty section:
This article was submitted to Research Policy and Strategic Management, a section of the journal Frontiers in Research Metrics and Analytics

Received: *13 September 2021*
Accepted: *17 March 2022*
Published: *27 April 2022*

Citation:
Christian A (2022) Addressing Conflicts of Interest and Conflicts of Commitment in Public Advocacy and Policy Making on CRISPR/Cas-Based Human Genome Editing. Front. Res. Metr. Anal. 7:775336. doi: 10.3389/frma.2022.775336

INTRODUCTION

The CRISPR/Cas technology has changed the landscape of biomedical research and genome engineering (Jinek et al., 2012; Hsu et al., 2014; Lin Y. et al., 2014). Due to its significant advantages over alternative technologies based on zinc-finger nuclease (ZFN) or TAL effector nuclease (TALEN), we now have access to more cost-effective, more precise, and more broadly applicable genome editing tools (Doudna and Charpentier, 2014; Ledford, 2015). Yet the prospects of human genome editing in controversial scenarios—in particular heritable editing—raises a series of complicated bioethical and legal ethical issues (Chan and Sternberg, 2019). The resolution of these issues has become an urgent matter in the wake of a

research scandal surrounding the biophysicist He Jiankui in 2018.[1] He was responsible for an experiment in which a CCR5-Δ32 mutation in human embryos was induced *via* CRISPR/Cas9 to bring about an immunity against HIV infections. It resulted in a renewed interest in an ongoing debate on the regulatory framework for future research on heritable human genome editing and pleas on a moratorium on human germline editing.[2] Some experts have argued against a moratorium (Konig, 2019; Macintosh, 2019), whereas others have proposed risk-averse policies and endorsed a moratorium on clinical research, which could give policy makers and legislators time to establish international frameworks and develop ethical and legal guidelines on a national level (Lander et al., 2019). What complicates these debates is the fact that the CRISPR/Cas technology is a very economically valuable sector within the fast growing market of biotechnology (Brinegar et al., 2017). It is thus not surprising that many of the leading experts have ties to biomedical and pharmaceutical companies, e.g., receive funding for projects from pharmaceutical companies, have founded companies working with CRISPR/Cas themselves, own shares of biomedical companies or serve on scientific advisory boards.

My focus in this paper will be on conflicts of interest and conflicts of commitment in the context of public advocacy and public policy making on heritable human genome editing. According to a classical definition given by Thompson, a conflict of interest is "a set of conditions in which professional judgment concerning a primary interest (such as a patient's welfare or the validity of research) tends to be unduly influenced by a secondary interest (such as financial gain)" (Thompson, 1993, p. 375). Typically, conflicts of interest in biomedical research and medical practice emerge from financial ties between scientists and medical professionals and representatives of commercial entities like pharmaceutical companies. Yet it is important to note that every institutional system which works with financial or social incentives can produce conflicts of interest. For instance, if a professional agent has high expectations regarding her own work and is emotionally and motivationally dependent on positive feedback from professional peers, than the urge for acknowledgment by peers (secondary interest) can be in conflict with her professional obligations in research, like carefulness in conducting medical experiments (primary interest), when she rushed to publications in hope of acknowledgment. Often professional agents are not aware that they have conflicts of interest or act in a situation in which there is a more or less severe risk of biased decision-making (Bornstein and Emler, 2001; Felser and Klemperer, 2011, p. 29), which is why one of the most common strategies to cope with conflicts of interest is their declaration. This enables third parties to be aware of potential biases. While conflicts of interest, in particular the influence of commercial interests in biomedical research are widely discussed (cf. Lieb et al., 2011, 2018; Krimsky, 2018), the related concept of conflict of commitment receives less attention. Patricia Werhane and Jeffrey Doering define it as follows:

> *"Conflicts of commitment are conflicts between at least two sets of professional obligations. Conflicts of commitment differ from conflicts of interest because conflicts of commitment involve the distribution of focus and effort between two sets of professional obligations, rather than a conflict between professional and financial/recognition interests. Conflicts of commitment are those conflicting commitments where competing obligations prevent honoring both commitments or honoring them both adequately." (Werhane and Doering, 1995)*

Since conflicts of commitment emerge from professional obligations—and not from a conflict between primary interests (professional obligations) and secondary interests (like financial incentives and acknowledgment)—they are much harder to avoid on an institutional and individual level. One example for this is the commitment to contribute an equal or contractually defined distribution of time and attention to research, teaching, administrative duties, science communication, and public advocacy. Another example for conflicts of commitment is the conflict between prima facie legitimate research interests on the one hand and professional responsibilities in debates on research policies on the other, which affect the pursuit of those research interests. Think about a biomedical scientist who is committed to understand certain aspect of the development of human embryos, who also serves on an ethics committee which is tasked with the development of guidelines for human embryo research. Here, research interests in certain topics (a primary interest) could negatively affect the moral evaluation of the acceptability of experiments with human embryos (also a primary interest). In such a situation the researcher might favor self-serving guidelines, which enable the pursuit of certain research questions with regard to human embryos. Both types of conflicts create a risk for the moral integrity and objectivity of research and publications processes, efforts of science communication as well as policy making processes with regard to the CRISPR/Cas technology, as will be illustrated and discussed in later sections of this contribution.[3] One particular problem in this context is that it is hard to distinguish between conflicts of commitment (resulting from conflicting professional obligations) and conflicts of interest (resulting from the presence of commercial interests), which is why both types of conflicts are addressed in this contribution and information is presented which allows for conclusions into the motivation for certain professional decisions.

I will argue that we need to establish stronger precautionary measures with regard to the disclosure of conflicts of interest and conflicts of commitments of leading experts at CRISPR/Cas-based genome editing by showing four things: First, information

[1] Chinese family names are written before the first name, thus He is the family name and Jiankui the first name.

[2] Although the current debate on a moratorium on human germline editing is related to CRISPR/Cas, other types of genome editing technologies would be affected by such a moratorium as well, these include engineered nucleases like meganucleases, Zinc finger nucleases (ZFN), transcription activator-like effector nucleases (TALENs) and Nucleobase Modification (BASE Editing). I would like to thank one reviewer for mentioning this point.

[3] I will speak of "*the* CRISPR/Cas technology" (definite description for the sum of basic knowledge on CRISPR/Cas mechanisms and technical applications), while one might also conceive of it as rather a series of technologies derived from CRISPR/Cas immune systems in bacteria and archaea.

on commercial conflicts of interest of leading experts is sometimes not readily available and lacking in detail. Second, conflicts of interest usually are not disclosed in the context of public advocacy for specific research policies. This makes it very hard for participants in public discussions of ethical implications of the CRISPR/Cas technology to understand the actual economic interests in the background of certain advocated positions within the spectrum of risk-affirmative and risk-aversive positions. Third, the extent to which scientific experts on CRISPR/Cas are currently being relied on in public debates and policy making disregards philosophical insights into important differences between moral expertise and scientific expertise as well as deference to experts of either types. Fourth, the magnitude of influence experts on CRISPR/Cas have on public and semi-public debates as well as on policy making processes raises a political problem of legitimate representation. After making the case for increasing the transparency of public and semi-public debates with regard to conflicts of interest and conflicts of commitment, I will also shortly indicate the limitations of this approach for safeguarding the integrity of public debates and securing a responsible conduct with CRISPR/Cas.

This contribution is structured as follows: Section CRISPR/Cas9—What it is and what it does gives a brief and informal overview on the CRISPR/Cas technology in the context of human genome editing. Section An epic scientific misadventure familiarizes the reader with the research scandal surrounding the first attempt by He Jiankui to edit the genome of human embryos resulting in the birth of several children and summarizes critical reactions to this scandal from biomedical scientists and bioethicists. Section Experts in moral debates on the ethical issues of CRISPR/Cas technology and policy making first highlights two major consequences of this case with regard to the regulation of CRISPR/Cas-based human genome editing: the debate of a moratorium on heritable human genome editing and the work toward a responsible pathway. It then identifies the various ways in which scientific experts participate in public and semi-public ethical debates and in public policy making. Section A plea for caution then brings forward a series of philosophical concerns relating to this kind of involvement of biomedical experts and raises a note of caution with regard to the lack of transparency about commercial conflicts of interest and conflicts of commitment among experts in the CRISP/Cas technology. In the final section Toward more transparency, I discuss precautionary measures to safeguard the integrity and transparency of public and semi-public debates on ethical issues with the CRISPR/Cas technology.

CRISPR/CAS9—WHAT IT IS AND WHAT IT DOES

The CRISPR/Cas technology is basically a toolkit for building molecular scissors (endonucleases), which can make genetic alterations at specifically chosen places in a DNA sequence (customized sequence specificity). CRISPR associated proteins (Cas) can be utilized for genome editing in various species (Jinek et al., 2012; Ran et al., 2013). One particular important type of endonuclease is Cas9. It has been shown that the CRISPR/Cas immune system of *Streptococcus pyogenes* can be used to create an active endonuclease complex consisting of Cas9 and guide RNA (gRNA/sgRNA). Due to the guide RNA, Cas9 can target specific genetic sequences and make genetic alterations, e.g., in human cells. With the help of this genetic tool, which was further developed into an entire toolkit for multiple purposes, scientists can comparatively easily target specific genetic sequences and make several types of changes (Makarova et al., 2015; Moon et al., 2019). Further details on the basics of CRISPR/Cas-based genome editing are explained inter alia in Yamamoto (2015), Gaj et al. (2016), and Luo (2019), I focus on the basics here.

It is important to note that the CRISPR/Cas technology has several advantages over alternative methods for genome editing, like ZFN and TALEN. For instance, nuclease design and assembly is easy and feasible in most labs, the success rate of nuclease design is high, the target specificity is high with most guide RNAs, the target range is potentially unlimited, multiplexing is highly feasible and it is not sensitive to CpG methylation (Gilles and Averof, 2014). This means that the CRISPR/Cas technology costs only a fraction of alternative methods, is faster and less labor intensive, it is very precise and it can be used to target a large range of genome sections. In biomedical research, CRISPR/Cas is widely seen as one of the most promising approaches to making genetic alterations that might benefit human health—I focus on human health research here. Potential applications include genome editing for (i) the treatment of monogenetic diseases like cystic fibrosis based on a mutation of the CFTR gene (Veit et al., 2016), (ii) the treatment of polygenetic and multifactorial diseases like Alzheimer's dementia *via* intervention on APP, PSEN1, and PSEN2 (Bekris et al., 2010), and (iii) reducing the risk of polygenetic and multifactorial diseases, e.g., reducing dispositions for breast and ovarian cancer *via* intervention on BRCA1 and BRCA2 (Kuchenbaecker et al., 2017).

Several technical obstacles come along with genome editing in general and heritable editing in particular. It is important to mention these technical problems here, because the majority of experts on the CRISPR/Cas technology currently lean toward a clinical moratorium on heritable human genome editing, which would leave room for basic research on these technical issues. Also, finding technical solution to solve or cope with these problems is considered a necessary requirement for a responsible transition to clinical research on heritable genome editing. I highlight just a few of the issues currently discussed:

1. Off-target editing: CRISPR/Cas9 sometimes edits genetic sequences other than the intended sequence (identified by the guide RNA sequence). The error rate of specific applications of endonuclease complexes is an object of current research (Cho et al., 2014; Lin S. et al., 2014; Zhang et al., 2015; Park and Beal, 2019).
2. Genetic mosaicism: Genome editing in a zygote or an early embryo comes with a significant chance that some of the cells in the resulting organism will not have the desired edit (Mehravar et al., 2019). Having two or more genetically different sets of cells in one's body might result in health issues (Biesecker and Spinner, 2013).

3. On-target effects: Some genes that cause serious genetic diseases also give carriers some protection against infectious diseases when the gene in question is present in one copy. A paradigmatic example is the HBB gene. If someone inherits two copies of HbS (mutated version of HBB) from both parents, then this individual will suffer from sickle cell anemia. Yet if someone only inherits one copy of HbS from either parent (or the mutation occurs naturally), then this patient will suffer from less dramatic health effects and gains some protection against malaria (Archer et al., 2018).
4. Ability to select appropriate gene targets: Due to our currently limited knowledge of human genes, genetic variation, and interactions between genes and the environment, it is not clear whether we are in a position to make a well-justified decision on appropriate gene targets (National Academies of Sciences, 2017) and avoid or minimize risks like on-target effects.
5. Access to and pricing of clinical medical treatments: One major worry is that medical treatments based on CRISPR could be extremely expensive and thus not broadly available for patients in the long term. There are several reasons for extreme pricing of novel gene therapies, including the necessity to recoup the development costs, higher effectiveness of novel therapies compared to other treatments as well as technical challenges of production and delivery in clinical practice (Wilson and Carroll, 2019).

These and other technical and ethical issues have led to a general hesitancy with regard to heritable genome editing in clinical practice which would involve genome alterations in human embryos and the birth of genetically altered humans (Brokowski, 2018).[4]

AN EPIC SCIENTIFIC MISADVENTURE

One factor that majorly contributed to the current hesitancy with regard to heritable human genome editing and in particular clinical applications of the CRISPR/Cas technological is the research scandal that has unfolded around the Chinese biophysicist He Jiankui between 2018 and 2020. The case has been widely commented (Greely, 2019), although crucial details on experimental procedures, financial connections to companies within the biotechnology and biomedical sector as well as support from political and scientific intuitions remain unclear—not to mention individual support from scientists who were in contact with He (Cohen, 2019b).

In the following, I will briefly explain the scientific background of He's experiment, which Francis Collins dubbed an "epic scientific misadventure," give a rather coarse-grained overview on the timeline of events, and then provide an overview on the legal and moral fallout (Cohen, 2018b).[5] More fine-grained reconstructions of the events between 2018 and 2020 can be found in various recently published sources (Baylis, 2019a; Greely, 2019, 2021; Kirksey, 2020). The description of this case illustrates the negative effects resulting from the presence of secondary interest, which gave rise to recent efforts to regulate human germline research with CRISPRS/Cas and similar technologies. Since some of those interests could also present in elite scientists who make a regulatory effort regarding heritable genome editing, one could make an argument from analogy for a more cautious stance toward the role of leading scientists in policy making.

He obtained his PhD in 2010 under Michael W. Deem at Rice University and subsequently worked on CRISPR/Cas9 gene-editing as a postdoc under Stephen Quake at Stanford University. After returning to his country of origin (People's Republic of China) in 2012 within the Thousand Talents Program (TTP),[6] he was employed at the Southern University of Science and Technology (SUSTech) in Shenzhen, Guangdong province, China. There He was in charge of a lab funded by the Chinese government and received 1 million yuan (around 144.000 USD) in angel funding (high risk funding for start-ups) for new companies from TTP as well as other funding from private investors (Kirksey, 2020). Such financial ties as well as working conditions, which encourage novel and commercially interesting research projects, are commonly found top tier research institutions. Thus, it is unsurprising that He founded at least two companies. Direct Genomics,[7] based in Shenzen (founded in 2012), is concerned with the development of a single-molecule sequencing device based on a technology previously developed by Stephen Quake and formerly licensed by Helicos Biosciences.[8] Vienomics Biotech was founded in 2016 and offers genome sequencing and screening for cancer patients and at-risk groups.

Relatively unknown within the Western scientific community, He announced on November 25, 2018 that he had successfully edited the genome of two embryos using *in vitro* fertilization and CRISPR/Cas9. He had targeted CCR5, a gene that is essential for HIV-1 to induce its viral DNA into cells. His experiment was based on the observation that a deletion of 32 base pairs in CCR5 on chromosome 3 is responsible for a resistance to HIV-1 infection (Samson et al., 1996). Such a deletion, called CCR5-Δ32, results in the production of non-functional copies of the CCR5 protein found on the surfaces of T-cells, which are white blood cells in the immune system. Humans with two dysfunctional copies of CCR5 are virtually resistant against HIV-1 infections, since HIV-1 viruses cannot establish a connection to T-cells with a crippled form of CCR5 (Brelot and Chakrabarti,

[4]For more details on these and other challenges, see Chan and Sternberg (2019). For a comparative overview on the various reports and ethics statements of ethics commissions, see Brokowski (2018), for an overview on policies on human germline editing, see Baylis et al. (2020). I refer to Carolyn Brokowski's meticulous work instead of adding all the references individually.

[5]https://www.nih.gov/about-nih/who-we-are/nih-director/statements/statement-claim-first-gene-edited-babies-chinese-researcher (retrieved August 31, 2021).

[6]This program was criticized by the US National Institutes of Health (NIH) for taking unfair advantage of US research efforts by hiring scientists with Chinese heritage who had been trained in the US. It was also criticized for a lack of transparency with regard to commercial relationships between US research and Chinese business ventures and some suspected that it was an attempt to conduct research espionage (Hvistendahl, 2014; Cohen and Malakoff, 2019; Mervis, 2019, 2020a,b; Staff, 2020).

[7]The company's website is currently offline, a snapshot can be found on archive.org. https://web.archive.org/web/20181228100437; http://www.directgenomics.com/ (retrieved March 10, 2022).

[8]Helicos went bankrupt in 2012 and SeqLL bought all its intellectual property and hardware. http://seqll.com (retrieved September 1, 2021).

2018). However, even individuals with two dysfunction copies of CCR5 can still contract an infection with HIV-2.

He's announcement came as a huge surprise and shock to the scientific and medical community, because it was also announced that resulting from this experiment two genetically edited babies had been born—pseudonymized as Lulu and Nana (Normile, 2018). The parents participating in this particular experiment were couples where the male was HIV positive and the female was HIV negative. One day later, on November 26, 2018, SUSTech distanced itself from the experiment and declared that He was on leave since February 2018 and that the experiment was not affiliated with SUSTech. The experiment contradicted SUSTech's codes of conduct for biomedical research and He therefore lost his position as an associate professor at SUSTech (Normile, 2019a). On November 28, 2018; He gave a talk on his experiment at the Second International Summit on Human Genome Editing in Hong Kong, which was met with almost exclusive rejection by the audience—more on that later.[9] On December 1, 2018; He as well as his family were put under house arrest and detained in a guest house of SUSTech in Shenzen, guarded by government agents. After 1 year, on December 30, 2019—during the ongoing international debate on the legal and ethical implications of this case—He as well as two other scientific collaborators were convicted by the Shenzhen Nanshan District People's Court. He was sentenced to 3 years in prison for illegal medical practice and also fined 3 million yuan (US $465,000/390,000 EUR). His colleagues involved in the experiment received lesser prison sentences and fines. Zhang Renli received a 2-year prison sentence and was fined 1 million yuan (US $144,000/130,000 EUR. Qin Jinzhou was sentenced to 18 months in prison and fined 500,000 yuan (USD$ 72,000/65,000 EUR) (Normile, 2019b; Cohen and Normille, 2020; Cyranoski, 2020; Kirksey, 2020). On January 1st, 2020, Chinese news agency Xinhua announced this verdict and also reported that a third baby was born.[10] Currently, there is no information on the health condition of the children available.

The experiment was almost unanimously condemned as immoral, scientifically premature, probably illegal in the People's Republic of China, and a general failure of scientific self-regulation. In an early case description, legal scholar and bioethicist Henry Greely thus called it a "reckless ethical disaster" and "fiasco" (Greely, 2019). Types of criticism regarding this case are three-fold. They include objections pertaining to (i) a lack of transparency regarding scientific and organizational aspects of the case, (ii) bioethical issues (e.g., a lack of medical necessity due to the availability of alternative methods for conceiving healthy offspring, misclassification of the experiment as a treatment, etc.) as well as (iii) the experiment's unlawfulness and the general disregard for protocol in biomedical research expressed by it. The following list of items comprises just some of the issues with the experiment that are currently discussed in medical ethics and research ethics, it is meant to give the reader an idea of the magnitude of the violations against good medical and scientific practice:

1. *Inappropriate consent form*: The 23 page long informed consent form is written in very technical language and includes no discussion of off-target effects or undesirable on-target changes. By not mentioning a common method applied in the context of intrauterine insemination and *in vitro* fertilization in cases in which one partner is HIV positive, He intentionally or recklessly depicted the experimental and more risky treatment as the favorable alternative. Furthermore, the form failed to provide information about alternative methods of preventing an HIV infection. The consent form was not approved by an institutional review board, either. Finally, staff members without specific training took only 120 min to explain the experiment to participants (Greely, 2019; Jonlin, 2020; Kirksey, 2020; Shaw, 2020).
2. *Lack of transparency*: He bypassed peer review by announcing the result of the experiment in a video posted on youtube.com on November 25, 2018.[11] He provided no research paper explaining the exact experimental procedure and results of the experiment. It is still unclear (September 2021), but based on screenshots from his presentation at the Human Genome Editing in Hong Kong, one can assume that only one of the two siblings has two copies of CCR5 edited, while the other sibling still has a functional CCR5 gene. Therefore, one of the siblings can still be infected with HIV (Cohen, 2018a). The health status of the third child, which resulted from an experiment with a different couple, is currently unknown.
3. *Violations of research protocol*: The experiment was neither registered before the clinical research was done, nor thoroughly checked or approved by an independent ethical review board. He forged ethical review papers in order to enlist volunteers for the procedure (Normile, 2019b), and had raised his own funds, deliberately evading institutional oversight.
4. *No medical necessity*: The immunization *via* CRISPR/Cas-based germline intervention against HIV infection was not a medical necessity, since alternative medical procedures to prohibit an infection with HIV exist and are routinely employed in *in vitro* fertilization, e.g., sperm washing (Savasi et al., 2007; Zafer et al., 2016; Carvalho et al., 2021).
5. *Illegal medical procedures*: He used sperm washing in order to separate sperm from sperm fluid, which contained HIV viruses. Yet IVF procedures in general and sperm washing in particular are currently banned in China for HIV infected couples. This is also problematic, since offering the participation in such an experiment can be seen as a strong incentive for HIV infected couples or gay couples, wishing a healthy and genetically related offspring without having heterosexual intercourse for the sake of procreation.
6. *Problem of target selection*: He target CCR5 for genome editing, although CCR5 has a protective role in immune

[9]The entire talk as well as the discussion are available on youtube.com. https://www.youtube.com/watch?v=tLZufCrjrN0 (retrieved September 3, 2021).

[10]http://www.xinhuanet.com/english/2019-12/30/c_138666892.htm (retrieved December 17, 2021).

[11]https://www.youtube.com/watch?v=th0vnOmFltc, Retrieved 01-09-2021. One of the first science journalists reporting on the case was Antonio Regalado (MIT Technology Review), who published an investigative article on the same day. https://www.technologyreview.com/2018/11/25/138962/exclusive-chinese-scientists-are-creating-crispr-babies/ (retrieved September 1, 2021).

reactions against the West Nile virus, which is common in Europe, Africa and North America (Kohlmeier et al., 2008; Cyranoski, 2018a), and a CCR5 deficiency predisposes to fatal outcome in influenza virus infections (Falcon et al., 2015).[12]

7. *Not a medical treatment, but genetic enhancement*: CCR5-Δ32 might, in addition to establishing a resistance against infections with HIV-1, even enhance certain cognitive dispositions, since CCR5 is linked to improved memory function in mice (Zhou et al., 2016) as well as enhanced recovery from strokes and traumatic brain injuries in humans (Joy et al., 2019). More fundamentally, not having a rare favorable genetic disposition is obviously not a disease, thus creating this genetic disposition is not a medical therapy in a strict sense of reducing or eliminating disease, but rather an instance of enhancement resulting in risk reduction.
8. *Failure to provide appropriate health care provisions*: The children whose genetic material has been altered will learn at some point in their life that they are the result of an experiment in heritable human genome editing, yet the provision of psychological and pedagogical support for the family was not taken into consideration. Also, if the observation under point 1 is correct, one of the siblings can still be infected with HIV.

It can be assumed that He anticipated at least some of this criticism, since he published a research paper in *The CRISPR Journal* in 2018 (Jiankui et al., 2018), which was retracted due to the circumstances surrounding this case and a lack of full and open disclosure of conflicts of interest on November 26, 2018. This paper encouraged "[…] the scientific community to support the public in making informed decisions about gene surgery's clinical utility, limitations, risks, regulatory needs, and future role in society" (Jiankui et al., 2018, p. 2). The authors in particular formulate five core principles for gene surgery in human embryos, including mercy for families affected by heritable diseases, restriction of gene surgery to the prevention of serious diseases, respect for child's autonomy, rejection of genetic determinism, and equal access to gene surgery (Jiankui et al., 2018, p. 2). It is challenging to not conceive this contribution as a *post-hoc* attempt to rationalize the experiment and create a flimsy impression of moral integrity and social responsibility, especially given the lack of disclosure of the experiment in this publication. Also, it is quite astonishing that this contribution sustained the peer review process, since it barely refers to the bioethical debates regarding heritable human genome editing (see Getz and Dellaire, 2020). Against the main thesis of this investigation, Jiankui et al. (2018) can be seen as a rather obvious example of an attempt to influence the public debate on the moral acceptability of clinical research on gene surgery, which brings about a heritable change of a human germline.

Commentators highlight three main motivational factors for He's experiment, listed here in random order: (i) He worked in an environment that provided strong financial incentives, as he received angel funding from TTP as well as a yet not fully identified amount of private funding for his laboratory, private companies, and future business endeavors (Coleman, 2018; Baylis, 2019a; Qiu, 2019; Kirksey, 2020; Greely, 2021). (ii) He had strong career ambitions and—according to many of those who corresponded with him before his detention—wanted to be the first in creating genetically altered human beings (Belluck, 2017; Greely, 2021). (iii) Furthermore, due to his experience with the suffering of HIV and AIDS patients in China, he seemed to have had genuine sympathy with patients who might benefit from his research.[13]

The case is now inextricably linked to the development of CRISPR/Cas (Baylis, 2019a; Kirksey, 2020; Davies, 2021; Greely, 2021; Isaacson, 2021) and a paradigmatic example of a rogue scientist who, due to immense interests in scientific reputation and vested commercial interests, circumvented laws and bioethical standards. In the context of this contribution, this case serves to make the urgency of establishing effective regulation obvious. It will also make it at least initially plausible that further regulations to cope with commercial conflicts of interest as well as conflicts of commitment are needed, as the identified motivational factors (i–iii) suggest.

EXPERTS IN MORAL DEBATES ON THE ETHICAL ISSUES OF CRISPR/CAS TECHNOLOGY AND POLICY MAKING

Moral worries on the matter of human germline editing and calls for a broad societal discussion on the bioethical issues predate the He Jiankui case. In fact, the debate about the ethics of human genome editing can be traced back to the debate on eugenics movements in the 1950s (Kevles, 1985). Yet, it took until the 1970s for scientists to imagine genetic interventions on an individual level, which go beyond the restriction and encouragement of certain patterns of procreational behavior. This development was stimulated by new research on restriction enzymes and recombinant DNA and led to the 1975 Asilomar Ban on recombinant DNA technology (Berg et al., 1975). With the rise of bioethics in the 1980s, bioethicists took then newly established ethical frameworks, in particular the principlism developed by Beauchamp and Childress (2001), and considered germline editing by appealing to the principle of beneficence and non-maleficence (e.g., Fletcher and Anderson, 1992):

> *"[…] searches for cure and prevention of genetic disorders by germ-line therapy arise from principles of beneficence and nonmaleficence, which create imperatives to relieve and prevent basic causes of human suffering." (Fletcher and Anderson, 1992)*

Generally speaking, the debate on human germline editing after the development of technologies for genetic engineering, which allow for target specific genome interventions, was for the longest time focused on the transition from basic to clinical research and considered attempts to change the genome

[12] Also, in a study published in 2019 (Wei and Nielsen, 2019a), which was later retracted due to bias in the underlying data of the UK Biobank (Callaway, 2019; Wei and Nielsen, 2019a,b; Maier et al., 2020), critics worried that a homozygous CCR5-Δ32 mutation is associated with an increased mortality.

[13] https://www.youtube.com/watch?v=aezxaOn0efE (retrieved September 3, 2021).

of human embryos as a hypothetical scenario. Yet, after a team of scientists from China (Liang et al., 2015) announced that they had used CRISPR/Cas9 to edit human tripronuclear zygotes, new efforts were taken to prohibit premature heritable genome editing. Further instances of the debate on gene surgery and heritable human genome editing include, in particular, subsequent statements made by various science organizations (The National Academies of Sciences, Engineering and Medicine, 2017). Inter alia, the German National Academy of Sciences Leopoldina in cooperation with other scientific organizations in Germany wrote in 2015:

> *It is important to have an objective debate that informs all stakeholders in a clear and transparent manner about the status of research and development into the techniques, and to ensure that any decisions taken are based on sound scientific evidence. (National Academy of Sciences Leopoldina et al., 2015)*

The scandal surrounding He Jiankui has thus fueled, but not initiated two debates which were already present in bioethics, but used to be a hypothetical scenario. Since He's "epic scientific misadventure," the scenario is now conceived as an imminent reality and thus, a top priority. Therefore, the *debate on a moratorium on heritable human genome editing* gained traction right after He's talk at the Second International Summit on Human Genome Editing (Cohen, 2019a; Davies, 2019; Dyer, 2019; Hough and Ajetunmobi, 2019; Konig, 2019; Lander et al., 2019; Macintosh, 2019; Wolinetz and Collins, 2019). Currently, many scientific, juridical and administrative issues are under discussion. Regarding the scope of a moratorium, leading scientists seem to lean toward a moratorium with regard to clinical studies on human germline editing, which leaves open the possibility to do basic research on technical aspects of CRISPR/Cas in basic research (Lander et al., 2019; Wolinetz and Collins, 2019). The latter is seen as necessary to engage in well-informed risk-benefit analyses fundamental to a translational pathway toward clinical applications. Another issue is the precise way to implement a global moratorium, e.g., *via* an exclusion from funding sources, outlawing certain types of research or self-imposed restrictions. Also, due to the relatively ready accessibility of the CRISPR/Cas technology, it is unclear how compliance with a moratorium might be enforced in private companies and countries without national regulatory frameworks on human genome editing or where an institutional structure is missing. From a philosophical point of view, there is the question of how a moratorium is compatible with commonly shared values of scientific freedom (Wilholt, 2010, 2012) and what the relevance of any actual hindrance of scientific progress might be (Konig, 2019; Macintosh, 2019). While the demand for a moratorium is certainly understandable, the justification for a moratorium on heritable human genome editing (or other scopes of a moratorium) would have to show that the case for a moratorium is stronger than the combined justificatory power of well-established arguments for positive and negative types of freedoms assembled under the generic concept of freedom of science. The latter pertain to, e.g., research freedom as a derivative of intellectual autonomy, its political value and epistemic utility (Wilholt, 2010). An ill-justified moratorium could potentially infringe on fundamental liberty or political rights.

The other debate that has been impelled in the wake of He's experiment concerns the exact criteria of a *pathway toward different types of clinical applications.* This debate relates heritable human genome editing to a whole range of bioethical issues, including the usage of human embryonic stem cells and products of synthetic biology like cell-based models of embryos or embryoids (Aach et al., 2017). Many national ethics councils and committees currently seem to agree with the following requirements (Brokowski, 2018; Baylis et al., 2020): (1) No human germline editing should be tried until risks and benefits are sufficiently known. (2) More time for ethical debates and establishing national and international legal framework on the editing of chromosomal and mitochondrial genetic information is required (Lander et al., 2019). (3) A broad societal discourse informed by scientists, moral and theological scholars is necessary. Finally, (4) societal consent could be necessary to adopt a positive stance toward certain types of clinical applications. It seems possible that some types of genome therapy which would affect the human germline, such as the treatment of some severe heritable monogenetic diseases, might find wide public acclaim in many societies (given that the risk-benefit ratio is positive).[14]

It is within the debate on a translational pathway to human genome editing that scientific experts on the CRISPR/Cas technology exercise particular influence. They take on crucial roles in establishing an international framework and helping to develop national policies (Baylis, 2019a). Typical functions experts take on in this context include (a) expert consulting in policy making processes, for instance by appearing in public hearings or writing scientific reports on risks and benefits of specific applications of the CRISPR/Cas technology. (b) Experts also serve as moderators and adopt a guiding function in initiating and maintaining a dialogue on ethical issues of the CRISPR/Cas technology. This currently often happens in semi-public formats, for instance after workshops and conferences, when renowned experts write scientific statements concerning the grant policy strategies they deem fit to find a purported balance between scientific freedom and respecting other ethical values. More recently, philosophers have begun to criticize such forums, because they are in stark contrast to the idea of a clear and transparent debate which includes all stakeholders—and not just scientists working with CRISPR/Cas (Stengers, 2018; Baylis, 2019a). (c) Experts engage in science communication by providing laypersons with the empirical knowledge about the CRISPR/Cas technology necessary to address the ethical issues. (d) Finally, experts engage in public advocacy for specific policies. This function is often considered unproblematic both in the debate on a moratorium on CRISPR/Cas and the debate on a translational pathway. The worry is that leading experts in the field of CRISPR/Cas could be affected by conflicts

[14] Also, the WHO proposed a global registry for human research with CRISPR/Cas (Cohen, 2019b). This proposal was recently adopted, the upcoming registry will be a part of the Clinical Trials Registry Platform (ICTRP), which can be accessed under https://trialsearch.who.int (retrieved September 1, 2021).

of (commercial) interest and conflicts of commitment. More concretely, if scientists have founded biomedical companies, have strong interest in peer recognition as well as a character defining urge to understand the nature and possible applications of CRISPR/Cas, then efforts undertaken by them to explain CRISPR/Cas-based genome editing, prospective applications as well as risks and benefits in clinical practice could intentionally or unintentionally foster their own research interests and moreover accommodate their recognitional or financial interests. Finding evidence for this concern is in my view extremely demanding and in the following I make a case for a more cautious stance toward the role of experts in this debate, due to our inability or limited ability to rule out conflicts of interest and conflicts of commitment. The train of thought here is that ignorance in conflicts of interest and conflicts of commitment of experts implies the adoption of less trust in the impartiality of those experts. I explicitly do not insinuate any form of corruption among these leading experts.

The previously presented overview on functions of scientific experts on the CRISPR/Cas technology in public and semi-public debates as well as policy making shows the ways in which leading scientists working as ethics architects and issue advocates (Baylis, 2019a) can gain a high degree of *intrinsic influence in public and semi-public debates and regulatory processes*, insofar as they serve as consults and moral authorities, but also *extrinsic influence* on the organizational features of public and semi-public debates. An example for intrinsic influence in debates can be seen in the linguistic framing of the debate on a translational pathway. Jennifer Doudna speaks about a "responsible pathway," "a viable path toward responsible use" and "a prudent way forward" (Baltimore et al., 2015; Doudna, 2019). As a Nobel prize winner, she has more opportunities to frame the problem in these terms and receives more attention, compared to critics. Also, when the debate is framed as the search for a responsible use, the basic question of whether there is a responsible use at all is almost off the table. A typical example for extrinsic influence are conferences (e.g., the International Summit on Human Genome Editing), which are organized as semi-public events and are generally not suitable for a broad societal discourse with many stakeholders. Also, the currently held public forums for discussing the ethical implications of CRISPR/Cas are often organized by scientists who have control over the selection and influence of participants, be they religious leaders, patients' and disability rights activists, social scientists, legal scholars or governmental representatives (Doudna and Sternberg, 2017). For instance, the agenda for the Third International Summit on Human Genome Editing (to be held in March 2023 at the Francis Crick Institute in London) reveals a number of speakers working on bioethical issues.[15] Yet, it is unclear whether those experts representing special interest groups will actually participate in the formulation of a final statement regarding ethical aspects of clinical applications. Also a ratification by the participants of a final statement on ethical issues is currently not planned, thus one needs to assume that any ethical assessment results from the internal deliberation of the organizers.

This high degree of intrinsic and extrinsic influence of a handful of individuals might be concerning in and of itself. When it is paired with commercial conflicts of interest as well as conflicts of commitment, it certainly poses a serious threat to the epistemic and moral integrity of decision-making processes in this context. Research has shown commercial conflicts of interest in biomedical research to be epistemically corrupting factors in research and publication processes[16] as well as in policy making and the development of clinical and research guidelines (Hakoum et al., 2020; Nejstgaard et al., 2020; Tabatabavakili et al., 2021).

A PLEA FOR CAUTION

In the context of the regulation of the CRISPR/Cas technology, not much attention is currently directed at commercial conflicts of interest and conflicts of commitment among biomedical researchers. The scientific community is presently rather occupied with the real possibility that other rogue scientists emerge. The concern about individuals surging forward on human germline editing has been further stoked by an announcement of molecular biologist Denis Rebrikov in 2019 (Cyranoski, 2019a,b), who is currently exploring the possibility to edit a gene linked to deafness (GJB2) with the help of CRISPR/Cas. Rebrikov is employed at Pirogov Medical University in Moscow and one can assume that such an experiment would be illegal in Russia, since the Russian federal law on biomedical cell products from 2016 bans the production of human embryos for research purposes and their implantation (Matthews and Moral, 2020).[17] As unsettling as such an announcement may be, it is dangerous to let (upcoming) scandals concerning individual deviant researchers detract from the risks that spring from the influence scientists exercise on public debate within the bounds of current regulations.

Above, I indicated that a mixture of career aspirations, commercial interests and sympathy with HIV/AIDS patients was likely the motivational background for He and his colleagues' violation of Chinese law, bioethical guidelines, and principles of good scientific practice in their experiment on CRISPR/Cas-based human germline editing. Inasmuch as these factors are actually good explanations for the blatant misconduct that has occurred in this case, any motivational setup in scientific experts who exhibit a comparable pattern of career aspirations, commercial interests and strong personal ideas about medical priorities must be considered a risk factor for compromised judgment in context of public and semi-public debates as well as policy making. This leads us to two *unsettling questions*: (1) Do we have reason to believe that outspoken public advocates for a specific type regulation on genome editing technologies

[15]https://royalsociety.org/-/media/events/2022/03/2022-human-genome-editing-summit/summit-agenda.pdf?la=en-GB&hash=CB1180F8AB4C942433E8DBE1463B9B1E (retrieved December 17, 2021).

[16]See the following systematic reviews on the corrupting influence of financial conflicts of interest on medical research (Bes-Rastrollo et al., 2013; Feuerstein et al., 2013; Lieb et al., 2016; Mandrioli et al., 2016; Probst et al., 2016; Narain et al., 2017; Zhang et al., 2018; Guntin et al., 2019; Hansen et al., 2019; Hendlin et al., 2019; Crow et al., 2022).

[17]This case is in particular deplorable since it fosters stereotypical "wild East" allegations.

have conflicts of interest and conflicts of commitments? (2) What are the risks resulting from conflicts of interest and conflicts of commitment in CRISPR/Cas policy making?

There are at least two reasons why we trust leading scientists and give them intrinsic and extrinsic influence on our discursive culture and various policy making processes. For one, we generally have trust in the various systems brought into place to designate academic rank, give scientific credit and acknowledgment for scientific achievements. These systems include, e.g., academic qualification systems (undergraduate programs, graduate programs etc.), peer review systems in journals, science award committees, and organization committees of scientific workshops and conferences. Generally, we trust these systems—or the individuals behind these systems—and assume that they correctly assign academic credentials and ranks within the organizational structure of scientific institutions. Secondly, there is also a tendency to assume that a high degree of scientific acknowledgment by scientific peers for an individual scientist also signals a certain integrity in that person, or even moral expertise with regard to her research field. In the following, I want to challenge our somewhat unconditional trust in experts by pointing toward crucial issues with commercial conflicts of interest among CRISPR/Cas experts.

Information about conflicts of interests and conflicts of commitment among experts on CRISPR/Cas engaging in the debate about its regulation is not easily accessible. It is often difficult to find information about the precise nature of conflicts of interest, including financial compensation. In the following, I will focus on the example of Jennifer Doudna, because she is one of the inventors of the CRISPR/Cas technology and thus one of the leading experts in this field. She is also actively involved in public debates on the ethics of CRISPR/Cas and has been for at least 8 years, highlighting the importance of a broad societal debate and a "thoughtful approach" to human genome editing. She is pleading for a moratorium on clinical applications of CRISPR/Cas and argues for strong national regulations as well as harsh sanctions against those who violate established policies—e.g., at minimum a loss of funding and publication privileges (Doudna, 2019). Doudna also has multiple financial ties to pharmaceutical companies, she has founded companies working with CRISPR/Cas and serves on corporate scientific advisory boards. On her laboratory's website, she lists several conflicts of interest. The subpage can be found on the bottom section/footer of the page, an area commonly reserved for copyright information, sitemaps, privacy policies, terms of use and contact details (see footer on https://doudnalab.org/, retrieved 08-25-21), which can be readily ignored by users. Information on conflicts of interest is not presented in detail in her curriculum vitae. In her short bio, there is only this rather non-descript hint:

> "In addition to her scientific achievements, Doudna is also a leader in public discussion of the ethical implications of genome editing for human biology and societies, and advocates for thoughtful approaches to the development of policies around the safe use of CRISPR technology. Doudna is an investigator with the Howard Hughes Medical Institute, senior investigator at Gladstone Institutes, and the President of the Innovative Genomics Institute. She co-founded and serves on the advisory panel of several companies that use CRISPR technology in unique ways." (https://doudnalab.org/bio/, retrieved 08-25-21).

The information on the website identifies her as a cofounder of Caribou Biosciences, Editas Medicine, Scribe Therapeutics, Intellia Therapeutics and Mammoth Biosciences. In addition to this, she is also a scientific advisory board member of Vertex, Caribou Biosciences, Intellia Therapeutics, eFFECTOR Therapeutics, Scribe Therapeutics, Mammoth Biosciences, Synthego, Algen Biotechnologies, Felix Biosciences, The Column Group and Inari. Furthermore, she is a Director at Johnson and Johnson and Tempus, and her research projects have been sponsored by Biogen, Pfizer, AppleTree Partners, and Roche. It is important to highlight here that unlike other elite scientists who made fundamental contributions to CRISPR/Cas, Doudna actually declares commercial conflicts of interests in a semi-transparent way on her website. Emmanuelle Charpentier's website, for instance, only includes links to CRISPR Therapeutics and ERS Genomics—two companies she co-founded.[18] Fang Zhang's Website only mentions that he is a founder of Sherlock Biosciences and the public companies Arbor Biotechnologies, Editas Medicine, and BEAM Therapeutics, yet tangible details about financial interests are not available.[19]

This reveals a situation in which secondary interests are present, but in which there is no direct, centralized way to quantify the magnitude of these interests. For sure, secondary interest are not by definition illegitimate, but rather a natural part of professional agents' life in complex socio-cultural and economic settings. Prospects of commercial applications can also be a part of a well-reasoned justification for a specific research agenda and policy decision on CRISPR/Cas technology. The issue lies elsewhere. If conflicts of interest and commitment of experts who engage in public debates are not declared or declared in an uninformative way, then participants in these debates have incomplete knowledge on the motivational background for experts' stances on the issues that are being debated. Thus, participants are not well-informed when agreeing or disagreeing with approaches relating to matters like a moratorium or regulatory efforts toward clinical applications. In particular, they lack background knowledge about reasons to inquire into the nature of some expert's contribution to the debate: they may overlook an occasion to wonder whether they are listening to a relatively disinterested expert *explaining* the CRISPR/Cas technology or to a speaker who is heavily invested in commercial endeavors relying on this technology and intends to make a case in a scientific priority dispute.

In her book—written together with Michael H. Sternberg—on the development of the CRISPR/Cas technology, Doudna is quite clear about her reservations concerning editing the human germline (Doudna and Sternberg, 2017). In a chapter on curative applications of CRISPR/Cas she writes:

[18]https://www.emmanuelle-charpentier-pr.org/ (retrieved December 17, 2021).
[19]https://mcgovern.mit.edu/profile/feng-zhang/ (retrieved December 17, 2021).

> *"I am extremely excited and enthusiastic about virtually all the phenomenal progress being made with CRISPR—save for the advancements on one front. I think we should refrain from using CRISPR technology to permanently alter the genomes of future generations of human beings, at least until we've given much more thought to the issues that editing germ cells will raise. Until we have a better understanding of all the attendant safety and ethical issues, and until we have given a broader range of stakeholders the opportunity to join the discussion, scientists would do well to leave the germline alone. But, really, whether we'll ever have the intellectual and moral capacity to guide our own genetic destiny is an open question—one that has been on my mind since I began to realize what CRISPR was capable of. For this reason and others, I've come to see a clear boundary between the procedures described in this chapter and those involved in germline editing. We should think twice before crossing that line. And then we should think again."* (*Doudna and Sternberg, 2017*)

A careful reader of Doudna and Sternberg (2017) will certainly have the impression that Doudna is honestly interested in the responsible advancement of the CRISPR/Cas technology for the sake of humanity. Other sources suggest that she was even morally appalled by He's experiment (Cyranoski, 2018b). Yet, her public talks about the CRISPR/Cas are more focused on the development and functioning of the CRISPR/Cas technology as well as medical and commercial prospects. Ethical issues are usually mentioned as such, but not elaborated in detail.[20] This is problematic, because in shorter statements Doudna directs the public debate about ethical implications of the CRISPR/Cas technology to certain outcomes without engaging in the details of the bioethical debates (Doudna, 2019) which concern, for instance, the usage of human embryos, embryonic stem cells and animal experimentation. Yet, she is considered by the public as one of the experts on the *ethics* of CRISPR/Cas and thus has access to public forums.[21]

This is reason enough to think that at least some of the leading experts in the CRISPR/Cas technology are in a situation which combines (i) a high level of expertise in scientific and clinical aspects of the CRISPR/Cas technology, which is relevant for the moral discourse, paired with (ii) self-declared commercial conflicts of interest (Greely, 2021) and (iii) a strong influence on public understanding of CRISPR/Cas as well as debates on the regulation of this technology. For instance, leading experts have the opportunity to publish opinion pieces in top-tier scientific journals and other media outlets, give plenary talks and television interviews. In the following I will explain why such a situation can introduce severe bias into the discourse on the ethical implications of CRISPR/Cas.

There are several ways in which experts in the CRISPR/Cas technology can influence public discourses and policy making processes and thereby might bring their research and commercial interests to bear on any international framework to be developed for genome editing. (a) Experts can advocate for a moratorium with regard to clinical studies of CRISPR/Cas-based human germline editing and highlight the importance of basic research on the safety and efficiency of CRISPR/Cas. This can be done without reacting to critics like (Guttinger, 2018) who point out that the ultimate proof of safety and efficiency of CRISPR/Cas-based human germline editing must be done in human *in vivo* and cannot be figured out in basic research. (b) The stipulation that a responsible pathway toward clinical applications is the only option that reconciles scientific progress and ethical concerns (Baylis, 2019b; Hurlbut, 2019) avoids the question of principle with regard to human germline editing. (c) Focusing on prospects of human genome editing, like cures for diseases and clinical applications within the next 10 years disregards the fact that developments in other fields in biomedical research suggest that translation time is probably much longer. For instance, after several decades of research, we only have one FDA reviewed and approved clinical therapy based on human stem cells, hematopoietic stem-cell transplantation (HSCT) (Felfly and Haddad, 2014; Mahla, 2016). (d) A persistent positive linguistic framing of the issue, in particular the normative enhancement of a neutral concept like "translational pathway" by speaking about a "*prudent* way forward" or "*responsible* pathway" is conducive to the conception that a safe translational pathway is possible and preferable to a permanent moratorium. Also, the former seems to require just the bare minimum of risk-assessment based on basic research about the CRISPR/Cas technology. (e) A voluntarily or involuntarily induced moral fallout, which leads from the alleged necessity to gain knowledge about specific aspects of the CRISPR/Cas technology (see point b) to the moral acceptability of the usage of human embryos and human stem cells in basic research on CRISPR/Cas without engaging in the deep and complicated ethical issues with this practice (Devolder, 2015). The same is valid for the moral acceptability of synthetic human-like entities with embryo-like features in basic research. (f) Another problematic issue is that scientists can simply select and promote an ethical framework which creates a window of opportunity for their research, without seriously engaging in the ethical reasoning behind it. This is a problem which commonly arises when scientific methods are morally problematic and their application requires an ethically well-reasoned justification. For instance, in basic research on off-target editing and other methodological aspects of the CRISPR/Cas technology, animal experiments are currently considered a step toward research on human genome editing. Animal experiments in general are widely criticized for their lack of objectivity and lack of moral justification. Now, it is certainly possible to pseudo-justify animal experimentation in basic research on CRISPR/Cas without seriously considering the moral wrongness of animal experiments or arguments against animal experimentation. For example, in a recent book on so-called animal research ethics, which was prominently featured in 2020 in Science (Grimm, 2020), Beauchamp and Grazia assume from the beginning that

[20] As an example: https://www.youtube.com/watch?v=gC_x2XKJjQo (retrieved January 9, 2021).

[21] For instance, Doudna recently (in 2021) gave the Schrödinger Lecture at the Imperial College London (https://www.imperial.ac.uk/news/215993/nobel-laureate-discusses-science-ethics-genome/, retrieved October 3, 2022) and spoke about ethical implications as well as the societal discourse with regard to CRISPR. Also, in an interview with The Harvard Gazette she considered herself as a relative novice in the field of ethics (https://news.harvard.edu/gazette/story/2018/05/crispr-pioneer-jennifer-doudna-explains-gene-editing-technology-in-prather-lectures/, retrieved October 3, 2022).

advocates of strong animal rights—those who reject the idea that the suffering of nonhuman animals in involuntary experiments is the sort of thing that can be outweighed by expected social benefits—are not "reasonable" and "open minded" (Beauchamp and DeGrazia, 2020). It is all too easy for scientists to simply adopt such an ethical framework as a pro forma stance, since it suits research interests, without considering the arguments against such a framework.

These hypothetical examples suggest that commercial conflicts of interest and conflicts of commitment, such as the economic success of your industry partners or a strong epistemic desire to find an answer to a research question, constitute a risk in public and semi-public debates as well as in policy making. These interests could bring scientists to make a case for a policy or a more general research framework which primarily suits their interests. Although not an example for outright corruption, these practices can still can be considered *manipulative* and warrant a more cautious stance toward the influence of leading experts.

In addition to these ploys, I will bring forward three further arguments to raise concerns with regard the influence of experts in public advocacy and policy making: First, since information on commercial conflicts of interest of leading experts is sometimes not readily available or declared in an uninformative way (leaving out precise financial information, etc.), our ability to assess the validity of advocated stances on a moratorium and a translational pathway is equally limited. This situation is inacceptable, especially since the CRISPR/Cas technology is a step toward changing the shared heritage of humanity. If the talk of a broad and transparent societal discourse on human genome editing has any meaning, then it must include informational transparency with regard to the commercial interests of scientists who exercise their right and their responsibility to participate in this discourse.

Second, the reliance on leading experts on the science of CRISPR/Cas in public debates and policy making to clarify ethical issues is also in conflict with philosophical insights into important differences between scientific expertise and moral expertise as well as deference to experts of either type. While experts on CRISPR/Cas are absolutely essential in helping laypeople understand the foundations and applications of this technology, it is far from obvious why we should regard them as experts in the ethical issues associated with CRISPR/Cas and defer to their moral decisions about these issues. For instance, empirical and methodological knowledge on CRISPR/Cas is certainly highly important in correctly reconstructing, evaluating and deciding a moral problem like the case for a moratorium. Yet, empirical and methodological knowledge—say, about off-target events or on-target effects—does not imply any superior capacity to justify a certain weighing of the associated risks and potential benefits or a capacity to frame the issue as a case of risk-benefit analysis in the first place.

A final issue is that experts on CRISPR/Cas may achieve relatively high influence on public debate and decision making due to their standing within the academic system, their relationships to private companies and political decision makers—yet they lack a public mandate. First-rank experts meet virtually no resistance in gaining access to public and semi-public debates. However, given the reasons presented in this section, it seems that we should meet them with not an especially high, but perhaps even reduced initial trust when it comes to their ethical assessment of the procedures in question. In any case, we should require more initial information on possible corrupting factors, even when we at the same time trust their epistemic and methodological assertions owing to their academic credentials.

TOWARD MORE TRANSPARENCY

What precautionary measures should we adopt in the face of these problems? There are at least three types of measures that could promote the integrity and political legitimacy of decision-making processes and public debates on the regulation of the CRISPR/Cas technology.

First, we need scientists to disclose information on conflicts of interest publicly and in more detail. One recent example of an attempt at such a central registry is a platform which already enables journalists and interested citizens to acquire information about commercial conflicts of interest. The *Dollars for Profs Project* by Sisi Wei, Annie Waldman and David Armstrong from ProPublica was started on December 6, 2019 (https://projects.propublica.org/dollars-for-profs/, retrieved 01-09-2021). This system is a great tool in figuring out commercial conflicts of interest, yet it is vastly incomplete. It lists information obtained from the National Institutes of Health *via* public record request filed at multiple public state universities. Yet, many universities decline to reveal conflicts of interests of their scientists.[22] ProPublica is a newsroom which intends to help investigative journalism in the public interest in the US. Thus, it lacks both the scientific legitimacy of other types of registries, for instance, state funded registries on clinical trials, as well as the necessary worldwide coverage. Information on conflicts of interest obviously has to be made available in a more comprehensive and scientifically established way. One way in which this could be done might be by having the WHO found a publicly available registry on conflicts of interest for researchers. In addition to this, research funding agencies could make it mandatory to register conflicts of interest and conflicts of commitments in this registry, the data being updated on a yearly basis.

Second, we need to change our stance on high-profile experts and their access to public debates. The declaration of conflicts of interest and conflicts of commitment should also be a requirement for access to large audiences, which need this information prior to talks in order to understand the proper economic context of certain policy positions. For instance, a TED Talk from a leading expert in CRISPR/Cas should include a disclaimer of the speaker's commercial conflicts of interest which gives the audience a good idea about the magnitude of vested financial interests.

Third, we need to pressure advocates of particular options for handling CRISPR/Cas to give a precise rationale for their favored policies in a more or less standardized fashion. This is a more

[22]https://www.chronicle.com/article/many-public-universities-refuse-to-reveal-professors-conflicts-of-interest/ (retrieved January 9, 2021).

demanding requirement: We should ask scientists involved in debates on the ethical issues with the CRISPR/Cas technology to write and sign a mission statement and upload this mission statement in the registry mentioned before. Such a statement could include answers to a series of questions, which relate to the development of CRISPR/Cas policies:

1. *Organizational feature of public and semi-public debates on ethical issues of the CRISPR/Cas technology*: What organizational model for public and semi-public debates do you prefer for what reasons? What is your role in public and semi-public debates? Who should have access to debates on ethical issues of the CRISPR/Cas technology? Should others defer to your moral assessment? etc.
2. *Responsible pathway to clinical applications*: Do you advocate for a responsible pathway to clinical applications with the aim of heritable human genome editing, for treatment, risk reduction or enhancement? Do you advocate for a responsible pathway to clinical applications with the aim of somatic human genome editing, for treatment, risk reduction or enhancement? etc.
3. *Moratorium on human genome editing*: In case you agree that we should implement a moratorium: What is the scope of the moratorium? What is your justification for a moratorium and how do the arguments for a moratorium outweigh arguments in favor of research freedom? How should we implement a moratorium? In case you disagree that we should implement a moratorium: Why do potential risks not override the justification for a moratorium? How should we, alternatively, prohibit misapplications of CRISPR/Cas in various scenarios? etc.
4. *Moral framework based in your thinking about (1), (2) and (3)*: What are your reasons for adopting specific moral frameworks relating to the usage of non-human animals in basic research, the usage of human embryos and human embryonic stem cells, the selection of target diseases? etc.
5. *Conflicts of interest and conflicts of commitments*: Given your answers to questions in (1) to (3): how would the respective measures affect your financial situation or affiliation to commercial entities? etc.

The three measures proposed here aim at increasing the transparency of public and semi-public debates by requiring detailed disclosure of conflicts of interest and conflicts of commitment. Yet, reviewing the arguments against reliance on the moral expertise of scientists, the argument from a lack of political mandate as well as the list of ploys that might be used to influence public debates (see section Experts in moral debates on the ethical issues of CRISPR/Cas technology and policy making), we should see transparency as only the very first step toward securing a better discourse setting. In the context of this contribution, I can only gesture at some strategies which go beyond the mere minimal requirement of transparency. Based on the recent literature on science communication (Davies and Horst, 2016; Medvecky and Leach, 2019), there are at least two further recommendations which might supplement improved transparency requirements. The first is to put a stronger focus on the ethics of science communication (Medvecky and Leach, 2019). The second is to work toward a diversification of formats for science communication and dialogues between multiple stakeholders (Riise, 2012). The ethics of science communication should be included in curricula in postgraduate education, e.g., research ethics and scientific publication ethics courses. Here the didactic aim should be to make clear that integrity of the communication of science is a condition for a constructive relationship between science and society and for functional policy making.

A diversification of formats for science communication is important to come closer to the ideal of an ethical debate between multiple stakeholders and activists. Alternative types of venues should be created to increase the likelihood of citizens and activists actually engaging in open debates about the ethical issues of CRISPR/Cas. These types of venues could include science cafés, student or science parliaments, student or pupil forums, junior science cafés, citizens' conferences, consensus conferences, citizens' exhibitions, twenty-first century town meetings and joint fact finding (Riise, 2012). In addition to this, one core principle in organizing these venues for debating ethical issues should be to withhold the right to select and invite representatives for the various groups of stakeholders from experts working in CRISPR/Cas technology who have conflicts of interest with respect to the issues discussed. Many universities and research institutions have established offices for science communication and citizen science who could handle the organization, so that a clear separation between the invitation of interest groups and scientific responsibilities—like review of submissions, selection of keynote speakers—is guaranteed.

CONCLUSION

The main thesis of this contribution was that we should establish stricter and more comprehensive requirements regarding the disclosure of conflicts of interest and conflicts of commitments in the context of debates on CRISPR/Cas-based human genome editing and change our stance toward the idea that scientific experts can naturally be treated as moral experts.

The promises and prospects of the CRISPR/Cas technology for scientific progress and economic prosperity set strong incentives to disregard established principles of good scientific practice, codes of conduct from bioethics and research protocols. These codes have been established to safeguard the epistemic and moral integrity of research and publications processes as well as protecting society and the environment. The case of He Jiankui illustrates both a failure of science to effectively anticipate the dangers of the new CRISPR/Cas technology and the necessity for an organized attempt to establish boundaries on an international and national level. Two current debates on CRISPR/Cas that can be seen as directly motivated by the case of He concern a moratorium on specific types of genome editing (in particular heritable human genome editing as well as genetic enhancement) and the conditions of a responsible pathway to clinical applications. Within this context, this paper indicated serious potential problems resulting from the presence of conflicts of interest in CRISPR/Cas policy making.

Three measures were proposed to address these problems: a registry for conflicts of interest of scientists, a change in our attitude toward leading experts on the CRISPR/Cas technology in the context of science advocacy, and a mission statement for scientists engaged in public advocacy for CRISPR/Cas policies. The latter would foster our ability to evaluate certain positions in the debates about a moratorium and a so-called responsible pathway toward human germline editing. In addition to these measures to increase transparency in public and semi-public debates on the ethical implications of the CRISPR/Cas technology, I also indicated the need to promote ethical science communication as a topic in postgraduate education as well as the diversification of venues for science communication and the separation of the invitation of interest groups and scientific responsibilities to set the stage for public debates.

Throughout this contribution I tried to make a case for a more cautious stance with regard to conflicts of interests and gave some reasons to believe that conflicts of commitment, i.e., conflicts between a set of primary interest resulting from the adoption of different professional roles, could be a serious issue in policy making processes relating to heritable genome editing. Yet, it is plausible to assume that the mechanisms described in section A plea for caution constitute a more general issue, which is similar to what James Kidd described in a series of publications as "epistemic corruption" (Kidd, 2015, 2019, 2020; Biddle et al., 2017, p. 172–173). Kidd's version of the concept of epistemic corruption describes the phenomenon that "[…] damage [is] done to people's epistemic character by their subjection to conditions or processes that erode epistemic virtues such as curiosity and thoughtfulness and facilitate the epistemic vices like dogmatism or closedmindedness" (Kidd et al., 2021, p. 152). Kidd primarily focusses on epistemic corruption in academic education and is generally concerned with a loss of epistemic virtues in professional agents. What I describe as conflicts of commitment in policy making, which take the form of biased decision making in moral deliberation or the participation in moral deliberation as an (ideally) impartial informant, could count as a corruption of moral virtues due to the presence of epistemic interests. I am concerned that something like this could exist in ethical debates on the limits of biomedical research—e.g., in debates on the morality of animal experimentation, genome editing, human stem cells (etc.). For instance, if a scientist depends on the usage of human embryonic stem cells in her research, she might lean in favor, since she has epistemic interests conducting research with stem cells. Likewise in CRISPR/Cas research, experts might favor a responsible pathway, since their epistemic preferences are not compatible with a moratorium on basic research, thus they adjust their moral framework and advocate for moral guidelines, which create sufficient space for their research.[23] One reason for such a pattern of thinking might be a commonly found purely epistemic axiology of science ("axiology" means a theory of aims for a research field), which defines the aim of research in purely epistemic terms, e.g. finding empirical adequate theories or figuring out a technical solution for a certain problem (etc.). Adopting a restrictive stance regarding basic research then seems hardly justifiable or even necessary anymore. Also one could make a case, that—due to such a purely epistemic axiology—epistemic interest would prima facie count as primary interest. Yet, if you adopt a mixed axiology, according to which the aim of research consist, for instance, in finding research knowledge which is socially valuable and attained with morally acceptable means, then you could make a case that social utility of research topics and moral acceptability of research methods is routinely in conflict with epistemic preferences. Thus, it constitutes a genuine case of a conflict of commitment between epistemic preferences which dominate your professional roles as a seeker of scientific knowledge, e.g., in a laboratory, and your moral obligations as someone who participates in scientific self-regulation by developing research policies.

DATA AVAILABILITY STATEMENT

The original contributions presented in the study are included in the article/supplementary material, further inquiries can be directed to the corresponding author.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

ACKNOWLEDGMENTS

I would like to thank Frauke Albersmeier for very helpful comments on earlier drafts of this article. I also would like to thank both reviewers for their kind and very constructive reviews.

[23]Just an illustrative comparison, those scientists affected by conflicts of commitment in policy making are like individuals who adopt a pescatarian diet since they have a culinary preference to eat fish, although they would, in principle, agree to a vegan diet if pressured to give good moral reason for their dietary choices.

REFERENCES

Aach, J., Lunshof, J., Iyer, E., and Church, G. M. (2017). Addressing the ethical issues raised by synthetic human entities with embryo-like features. *ELife* 6, e20674. doi: 10.7554/eLife.20674

Archer, N. M., Petersen, N., Clark, M. A., Buckee, C. O., Childs, L. M., and Duraisingh, M. T. (2018). Resistance to plasmodium falciparum in sickle cell trait erythrocytes is driven by oxygen-dependent growth inhibition. *Proc. Natl. Acad. Sci. U.S.A.* 115, 7350–7355. doi: 10.1073/pnas.1804388115

Baltimore, D., Berg, P., Botchan, M., Carroll, D., Charo, R. A., Church, G., et al. (2015). A prudent path forward for genomic engineering and germline gene modification. *Science* 348, 36–38. doi: 10.1126/science.aab1028

Baylis, F. (2019a). *Altered Inheritance: CRISPR and the Ethics of Human Genome Editing*. Harvard University Press. doi: 10.4159/9780674241954

Baylis, F. (2019b). Questioning the proposed translational pathway for germline genome editing. *Nat. Hum. Behav.* 3, 200. doi: 10.1038/s41562-019-0544-3

Baylis, F., Darnovsky, M., Hasson, K., and Krahn, T. M. (2020). Human germline and heritable genome editing: the global policy landscape. *CRISPR J.* 3, 365–377. doi: 10.1089/crispr.2020.0082

Beauchamp, T. L., and Childress, J. F. (2001). *Principles of Biomedical Ethics*, 5th Edn. Oxford: Oxford University Press.

Beauchamp, T. L., and DeGrazia, D. (2020). *Principles of Animal Research Ethics*. Oxford: Oxford University Press. doi: 10.1093/med/9780190939120.001.0001

Bekris, L. M., Yu, C.-E., Bird, T. D., and Tsuang, D. W. (2010). Review article: Genetics of alzheimer disease. *J. Geriatr. Psychiatry Neurol.* 23, 213–227. doi: 10.1177/0891988710383571

Belluck, P. (2017). *Gene-Edited Babies: What Chinese Scientist Told an American Mentor*. New York: The New York Times.

Berg, P., Baltimore, D., Brenner, S., Roblin, R. O., and Singer, M. F. (1975). Summary statement of the asilomar conference on recombinant dna molecules. *Proc. Natl. Accad. Sci. U.S.A.* 72, 1981–1984. doi: 10.1073/pnas.72.6.1981

Bes-Rastrollo, M., Schulze, M. B., Ruiz-Canela, M., and Martinez-Gonzalez, M. A. (2013). Financial Conflicts of interest and reporting bias regarding the association between sugar-sweetened beverages and weight gain: a systematic review of systematic reviews. *PLoS Med.* 10, e1001578. doi: 10.1371/journal.pmed.1001578

Biddle, J. B., Kidd, I. J., and Leuschner, A. (2017). Epistemic corruption and manufactured doubt: the case of climate science. *Public Affairs Q.* 31, 165–187. doi: 10.2307/44732791

Biesecker, L. G., and Spinner, N. B. (2013). A genomic view of mosaicism and human disease. *Nat. Rev. Genet.* 14, 307–320. doi: 10.1038/nrg3424

Bornstein, B. H., and Emler, A. C. (2001). Rationality in medical decision making: a review of the literature on doctors decision-making biases. *J. Evaluat. Clin. Pract.* 7, 97–107. doi: 10.1046/j.1365-2753.2001.00284.x

Brelot, A., and Chakrabarti, L. A. (2018). Ccr5 revisited: How mechanisms of hiv entry govern aids pathogenesis. *J. Mol. Biol.* 430, 2557–2589. doi: 10.1016/j.jmb.2018.06.027

Brinegar, K., Yetisen, A. K., Choi, S., Vallillo, E., Ruiz-Esparza, G. U., Prabhakar, A. M., et al. (2017). The commercialization of genome-editing technologies. *Crit. Rev. Biotechnol.* 37, 924–932. doi: 10.1080/07388551.2016.1271768

Brokowski, C. (2018). Do CRISPR germline ethics statements cut it? *CRISPR J.* 1, 115–125. doi: 10.1089/crispr.2017.0024

Callaway, E. (2019). Geneticists retract study suggesting first crispr babies might die early. *Nature* 574, 307–307. doi: 10.1038/d41586-019-03032-2

Carvalho, W. A. P., Catafesta, E., Rodart, I. F., Takata, S., Estevam, D. L., and Barbosa, C. P. (2021). Prevention of HIV transmission with sperm washing within fertile serodiscordant couples undergoing non-stimulated intrauterine insemination. *AIDS Care* 33, 478–485. doi: 10.1080/09540121.2020.1739201

Chan, S., and Sternberg, S. H. (2019). Special issue on ethics of human genome editing. *CRISPR J.* 2, 68. doi: 10.1089/crispr.2019.29051.cfp

Cho, S. W., Kim, S., Kim, Y., Kweon, J., Kim, H. S., Bae, S., et al. (2014). Analysis of off-target effects of crispr/cas-derived rna-guided endonucleases and nickases. *Genome Res.* 24, 132–141. doi: 10.1101/gr.162339.113

Cohen, J. (2018a). Ethics aside, does the crispr baby experiment make scientific sense? *Science* 371, eaaw2115. doi: 10.1126/science.aaw2115

Cohen, J. (2018b). An 'epic scientific misadventure': nih head francis collins ponders fallout from CRISPR baby study. *Science* 30, eaaw237. doi: 10.1126/science.aaw2371

Cohen, J. (2019a). Moratorium for germline editing splits biologists. *Science* 363, 1130–1131. doi: 10.1126/science.363.6432.1130

Cohen, J. (2019b). The untold story of the circle of trust behind the worlds first gene-edited babies. *Science* 10. doi: 10.1126/science.aay9400

Cohen, J., and Malakoff, D. (2019). Scientists caught in U.S. crackdown on China. *Science* 364, 811–812. doi: 10.1126/science.364.6443.811

Cohen, J., and Normille, D. (2020). China delivers verdict on gene editing of babies. *Science* 367, 130–130. doi: 10.1126/science.367.6474.130

Coleman, Z. (2018). *The Businesses Behind the Doctor Who Manipulated Baby DNA*. Tokio: Nikkei Asia.

Crow, M., Hartwell, M., Anderson, J. M., Tritz, D., and Vassar, M. (2022). Evaluation of conflicts of interest among systematic review authors on pharmacological therapies for alcohol use disorder: a cross-sectional study. *Subst. Abuse* 43, 278–281. doi: 10.1080/08897077.2021.1941509

Cyranoski, D. (2018a). Baby gene edits could affect a range of traits. *Nature* 10. doi: 10.1038/d41586-018-07713-2

Cyranoski, D. (2018b). CRISPR-baby scientist fails to satisfy critics. *Nature* 564, 13–14. doi: 10.1038/d41586-018-07573-w

Cyranoski, D. (2019a). Russian biologist plans more crispr-edited babies. *Nature* 570, 145–146. doi: 10.1038/d41586-019-01770-x

Cyranoski, D. (2019b). Russian 'CRISPR-Baby' scientist has started editing genes in human eggs with goal of altering deaf gene. *Nature* 574, 465–466. doi: 10.1038/d41586-019-03018-0

Cyranoski, D. (2020). What crispr-baby prison sentences mean for research. *Nature* 577, 154–155. doi: 10.1038/d41586-020-00001-y

Davies, K. (2019). Walk the line: Debating a germline editing moratorium. *CRISPR J.* 2, 74–76. doi: 10.1089/crispr.2019.29052.kda

Davies, K. (2021). *Editing Humanity—The CRISPR Revolution and the New Era of Genome Editing*. New York: Pegasus Books.

Davies, S. R., and Horst, M. (2016). *Science Communication*. London: Palgrave Macmillan. doi: 10.1057/978-1-137-50366-4

Devolder, K. (2015). *The Ethics of Embryonic Stem Cell Research*. Oxford: Oxford University Press. doi: 10.1093/acprof:oso/9780199547999.001.0001

Doudna, J. (2019). Crispr's unwanted anniversary. *Science* 366, 777. doi: 10.1126/science.aba1751

Doudna, J. A., and Charpentier, E. (2014). The new frontier of genome engineering with crispr-cas9. *Science* 346, 1258096. doi: 10.1126/science.1258096

Doudna, J. A., and Sternberg, S. H. (2017). *A Crack in Creation - Gene Editing and the Unthinkable Power to Control Evolution*. Boston: Houghton Mifflin Harcourt.

Dyer, O. (2019). Scientists call for moratorium on editing heritable genes. *BMJ* 364, 1256. doi: 10.1136/bmj.l1256

Falcon, A., Cuevas, M. T., Rodriguez-Frandsen, A., Reyes, N., Pozo, F., Moreno, S., et al. (2015). Ccr5 deficiency predisposes to fatal outcome in influenza virus infection. *J. Gen. Virol.* 96, 2074–2078. doi: 10.1099/vir.0.000165

Felfly, H., and Haddad, G. G. (2014). Hematopoietic stem cells: potential new applications for translational medicine. *J. Stem Cells* 9, 163–197.

Felser, G., and Klemperer, D. (2011). *Psychologische Aspekte von Interessenkonflikten*. doi: 10.1007/978-3-642-19842-7_3

Feuerstein, J. D., Akbari, M., Gifford, A. E., Cullen, G., Leffler, D. A., Sheth, S. G., et al. (2013). Systematic review: the quality of the scientific evidence and conflicts of interest in international inflammatory bowel disease practice guidelines. *Aliment. Pharmacol. Ther.* 37, 937–946. doi: 10.1111/apt.12290

Fletcher, J. C., and Anderson, W. F. (1992). Germ-line gene therapy: a new stage of debate. *Law Med. Health Care* 20, 26–39. doi: 10.1111/j.1748-720X.1992.tb01171.x

Gaj, T., Sirk, S. J., lan Shui, S., and Liu, J. (2016). Genome-editing technologies: Principles and applications. *Cold Spring Harb. Perspect. Biol.* 8, a023754. doi: 10.1101/cshperspect.a023754

Getz, L. J., and Dellaire, G. (2020). Back to basics: application of the principles of bioethics to heritable genome interventions. *Sci. Eng. Ethics* 26, 2735–2748. doi: 10.1007/s11948-020-00226-0

Gilles, A. F., and Averof, M. (2014). Functional genetics for all: engineered nucleases, crispr and the gene editing revolution. *EvoDevo* 5, 43. doi: 10.1186/2041-9139-5-43

Greely, H. T. (2019). Crisprd babies: human germline genome editing in the he jiankui affair. *J. Law Biosci.* 6, 111–183. doi: 10.1093/jlb/lsz010

Greely, H. T. (2021). *CRISPR People - The Science and Ethics of editing Humans*. Cambridge: The MIT Press. doi: 10.7551/mitpress/13492.001.0001

Grimm, D. (2020). Is it time to replace one of the cornerstones of animal research? *Science* 25, eabd5197. doi: 10.1126/science.abd5197

Guntin, J. A., Patel, D. V., Cardinal, K. L., Haws, B. E., Khechen, B., Yoo, J. S., et al. (2019). The influence of conflicts of interest on outcomes in the lumbar disc arthroplasty literature. *Spine* 44, 1162–1169. doi: 10.1097/BRS.0000000000003015

Guttinger, S. (2018). Trust in science: Crisprcas9 and the ban on human germline editing. *Sci. Eng. Ethics* 24, 1077–1096. doi: 10.1007/s11948-017-9931-1

Hakoum, M. B., Bou-Karroum, L., Al-Gibbawi, M., Khamis, A. M., Raslan, A. S., Badour, S., et al. (2020). Reporting of conflicts of interest by authors of primary studies on health policy and systems research: a cross-sectional survey. *BMJ Open* 10, e032425. doi: 10.1136/bmjopen-2019-032425

Hansen, C., Lundh, A., Rasmussen, K., and Hrbjartsson, A. (2019). Financial conflicts of interest in systematic reviews: associations with results, conclusions, and methodological quality. *Cochrane Database Syst Rev.* 8, MR000047. doi: 10.1002/14651858.MR000047.pub2

Hendlin, Y. H., Vora, M., Elias, J., and Ling, P. M. (2019). Financial conflicts of interest and stance on tobacco harm reduction: a systematic review. *Am. J. Public Health* 109, e1–e8. doi: 10.2105/AJPH.2019.305106

Hough, S. H., and Ajetunmobi, A. (2019). A crispr moratorium isn't enough: we need a boycott. *CRISPR J.* 2, 343–345. doi: 10.1089/crispr.2019.0041

Hsu, P., Lander, E., and Zhang, F. (2014). Development and applications of crispr-cas9 for genome engineering. *Cell* 157, 1262–1278. doi: 10.1016/j.cell.2014.05.010

Hurlbut, J. B. (2019). Human genome editing: ask whether, not how. *Nature* 565, 135–135. doi: 10.1038/d41586-018-07881-1

Hvistendahl, M. (2014). Show me the money. *Science* 346, 411–415. doi: 10.1126/science.346.6208.411

Isaacson, W. (2021). *The Code Breaker - Jennifer Doudna, Gene Editing, and the Future of the Human Race.* New York: Simon & Schuster.

Jiankui, H., Ferrell, R., Yuanlin, C., Jinzhou, Q., and Yangran, C. (2018). Retracted: draft ethical principles for therapeutic assisted reproductive technologies. *CRISPR J.* 1, e450–e453. doi: 10.1089/crispr.2018.0051

Jinek, M., Chylinski, K., Fonfara, I., Hauer, M., Doudna, J. A., and Charpentier, E. (2012). A programmable dual-rnaguided dna endonuclease in adaptive bacterial immunity. *Science* 337, 816–821. doi: 10.1126/science.1225829

Jonlin, E. C. (2020). Informed consent for human embryo genome editing. *Stem Cell Rep.* 14, 530–537. doi: 10.1016/j.stemcr.2020.03.010

Joy, M. T., Assayag, E. B., Shabashov-Stone, D., Liraz-Zaltsman, S., Mazzitelli, J., Arenas, M., et al. (2019). Ccr5 is a therapeutic target for recovery after stroke and traumatic brain injury. *Cell* 176, 1143–1157.e13. doi: 10.1016/j.cell.2019.01.044

Kevles, D. (1985). *In the Name of Eugenics: Genetics and the Uses of Human Heredity*. Berkeley: University of California Press.

Kidd, I. J. (2015). "Educating for intellectual humility," in *Intellectual Virtues and Education*, ed. J. Baehr (Routledge) 54–70. doi: 10.4324/9781315714127-4

Kidd, I. J. (2019). Epistemic corruption and eduction. *Episteme* 16, 220–235. doi: 10.1017/epi.2018.3

Kidd, I. J. (2020). "Epistemic corruption and social oppression," in *Vice Epistemology*, ed. I. James Kidd (Routledge), 69–86. doi: 10.4324/9781315146058-6

Kidd, I. J., Chubb, J., and Forstenzer, J. (2021). Epistemic corruption and the research impact agenda. *Theory Res. Educ.* 19, 148–167. doi: 10.1177/14778785211029516

Kirksey, E. (2020). *The Mutant Project*. St. New York: Martins Press. doi: 10.46692/9781529217315

Kohlmeier, J. E., Miller, S. C., Smith, J., Lu, B., Gerard, C., Cookenham, T., et al. (2008). The chemokine receptor ccr5 plays a key role in the early memory cd8+ t cell response to respiratory virus infections. *Immunity* 29, 101–113. doi: 10.1016/j.immuni.2008.05.011

Konig, H. (2019). Germline-editing moratorium — why we should resist it. *Nature* 458. doi: 10.1038/d41586-019-01292-6

Krimsky, S. (2018). *Conflicts of Interest in Science: How Corporate-Funded Academic Research Can Threaten Public Health.* New York: Skyhorse.

Kuchenbaecker, K. B., Hopper, J. L., Barnes, D. R., Phillips, K.-A., Mooij, T. M., Roos-Blom, M.-J., et al. (2017). Risks of breast, ovarian, and contralateral breast cancer for brca1 and brca2 mutation carriers. *JAMA* 317, 2402–2416. doi: 10.1001/jama.2017.7112

Lander, E. S., Baylis, F., Zhang, F., Charpentier, E., Berg, P., Bourgain, C., et al. (2019). Adopt a moratorium on heritable genome editing. *Nature* 567, 165–168. doi: 10.1038/d41586-019-00726-5

Ledford, H. (2015). Crispr, the disruptor. *Nature* 522, 20–24. doi: 10.1038/522020a

Liang, P., Xu, Y., Zhang, X., Ding, C., Huang, R., Zhang, Z., et al. (2015). Crispr/cas9-mediated gene editing in human tripronuclear zygotes. *Protein Cell* 6, 363–372. doi: 10.1007/s13238-015-0153-5

Lieb, K., Klemperer, D., Kolbel, R., and Ludwig, W.-D. (eds.). (2018). *Interessenkonflikte, Korruption und Compliance im Gesundheitswesen*. Berlin: Medizinisch Wissenschaftliche Buchgesellschaft.

Lieb, K., Klemperer, D., and Ludwig, W.-D. (eds.). (2011). *Interessenkonflikte in der Medizin*. Berlin: Springer. doi: 10.1007/978-3-642-19842-7

Lieb, K., von der Osten-Sacken, J., Stoffers-Winterling, J., Reiss, N., and Barth, J. (2016). Conflicts of interest and spin in reviews of psychological therapies: a systematic review. *BMJ Open* 6, e010606. doi: 10.1136/bmjopen-2015-010606

Lin, S., Staahl, B. T., Alla, R. K., and Doudna, J. A. (2014). Enhanced homology-directed human genome engineering by controlled timing of CRISPR/Cas9 delivery. *ELife* 3, e4766. doi: 10.7554/eLife.04766

Lin, Y., Cradick, T. J., Brown, M. T., Deshmukh, H., Ranjan, P., Sarode, N., et al. (2014). Crispr/cas9 systems have off-target activity with insertions or deletions between target dna and guide rna sequences. *Nucleic Acids Res.* 42, 7473–7485. doi: 10.1093/nar/gku402

Luo, Y. (ed.). (2019). *CRISPR Gene Editing, Vol. 1961*. New York, NY: Springer. doi: 10.1007/978-1-4939-9170-9

Macintosh, K. L. (2019). Heritable genome editing and the downsides of a global moratorium. *CRISPR J.* 2, 272–279. doi: 10.1089/crispr.2019.0016

Mahla, R. S. (2016). Stem cells applications in regenerative medicine and disease therapeutics. *Int. J. Cell Biol.* 2016, 1–24. doi: 10.1155/2016/6940283

Maier, R., Akbari, A., Wei, X., Patterson, N., Nielsen, R., and Reich, D. (2020). No statistical evidence for an effect of ccr5-32 on lifespan in the uk biobank cohort. *Nat. Med.* 26, 178–180. doi: 10.1038/s41591-019-0710-1

Makarova, K. S., Wolf, Y. I., Alkhnbashi, O. S., Costa, F., Shah, S. A., Saunders, S. J., et al. (2015). An updated evolutionary classification of crisprcas systems. *Nat. Rev. Microbiol.* 13, 722–736. doi: 10.1038/nrmicro3569

Mandrioli, D., Kearns, C. E., and Bero, L. A. (2016). Relationship between research outcomes and risk of bias, study sponsorship, and author financial conflicts of interest in reviews of the effects of artificially sweetened beverages on weight outcomes: a systematic review of reviews. *PloS ONE* 11, e0162198. doi: 10.1371/journal.pone.0162198

Matthews, K. R., and Moral, D. (2020). National human embryo and embryoid research policies: a survey of 22 top research-intensive countries. *Regenerat. Med.* 15, 1905–1917. doi: 10.2217/rme-2019-0138

Medvecky, F., and Leach, J. (2019). *An Ethics of Science Communication.* Cham: Springer International Publishing. doi: 10.1007/978-3-030-32116-1

Mehravar, M., Shirazi, A., Nazari, M., and Banan, M. (2019). Mosaicism in crispr/cas9-mediated genome editing. *Dev. Biol.* 445, 156–162. doi: 10.1016/j.ydbio.2018.10.008

Mervis, J. (2019). New doe policies would block many foreign research collaborations. *Science* 573, eaaw9541. doi: 10.1126/science.aaw9541

Mervis, J. (2020a). Moffitt cancer center details links of fired scientists to chinese talent programs. *Science* 19, eaba9662. doi: 10.1126/science.aba9662

Mervis, J. (2020b). Florida center details fired scientists' links to China. *Science* 367, 351–352. doi: 10.1126/science.367.6476.351

Moon, S. B., Kim, D. Y., Ko, J.-H., and Kim, Y.-S. (2019). Recent advances in the crispr genome editing tool set. *Exp. Mol. Med.* 51, 1–11. doi: 10.1038/s12276-019-0339-7

Narain, A. S., Hijji, F. Y., Yom, K. H., Kudaravalli, K. T., and Singh, K. (2017). Cervical disc arthroplasty: do conflicts of interest influence the outcome of clinical studies? *Spine J.* 17, 1026–1032. doi: 10.1016/j.spinee.2017.03.018

National Academies of Sciences, Engineering, and Medicine. (2017). *Human Genome Editing*. Washington, DC: National Academies Press.

National Academy of Sciences Leopoldina., National Academy of Science and Engineering., and German Research Foundation. (2015). *DFG, and Union of German Academies of Sciences and Humanities: Academien Union*. Halle (Saale): The Opportunities and Limits of Genome Editing.

Nejstgaard, C. H., Bero, L., Hrbjartsson, A., Jrgensen, A. W., Jrgensen, K. J., Le, M., et al. (2020). Conflicts of interest in clinical guidelines, advisory committee reports, opinion pieces, and narrative reviews: associations with recommendations. *Cochrane Database Syst. Rev*. 12, MR00040. doi: 10.1002/14651858.MR000040.pub3

Normile, D. (2018). Shock greets claim of crispr-edited babies. *Science* 362, 978–979. doi: 10.1126/science.362.6418.978

Normile, D. (2019a). *Scientist behind CRISPR Twins Sharply Criticized in Government Probe, Loses Job. J. Sci.* doi: 10.1126/science.aaw7557

Normile, D. (2019b). Chinese scientist who produced genetically altered babies sentenced to 3 years in jail. *Science* 10, eaba7347. doi: 10.1126/science.aba7347

Park, S., and Beal, P. A. (2019). Off-target editing by crispr-guided dna base editors. *Biochemistry* 58, 3727–3734. doi: 10.1021/acs.biochem.9b00573

Probst, P., Grummich, K., Klaiber, U., Knebel, P., Ulrich, A., Bchler, M. W., et al. (2016). Conflicts of interest in randomised controlled surgical trials: systematic review and qualitative and quantitative analysis. *Innov. Surg. Sci.* 1, 33–39. doi: 10.1515/iss-2016-0001

Qiu, J. (2019). *Chinese Government Funding May Have Been Used for Crispr Babies Project, Documents Suggest*. STAT News: Boston.

Ran, F. A., Hsu, P. D., Wright, J., Agarwala, V., Scott, D. A., and Zhang, F. (2013). Genome engineering using the crispr-cas9 system. *Nat. Protocols* 8, 2281–2308. doi: 10.1038/nprot.2013.143

Riise, J. (2012). *From Public to Policy*. Dordrecht: Springer. doi: 10.1007/978-94-007-4279-6_19

Samson, M., Libert, F., Doranz, B. J., Rucker, J., Liesnard, C., Farber, C.-M., et al. (1996). Resistance to hiv-1 infection in caucasian individuals bearing mutant alleles of the ccr-5 chemokine receptor gene. *Nature* 382, 722–725. doi: 10.1038/382722a0

Savasi, V., Ferrazzi, E., Lanzani, C., Oneta, M., Parrilla, B., and Persico, T. (2007). Safety of sperm washing and art outcome in 741 hiv-1-serodiscordant couples. *Hum. Reprod.* 22, 772–777. doi: 10.1093/humrep/del422

Shaw, D. (2020). The consent form in the chinese crispr study: in search of ethical geneediting. *J. Bioeth. Inquiry* 17, 5–10. doi: 10.1007/s11673-019-09953-x

Staff, S. (2020). Former los alamos physicist gets probation for failing to disclose china ties. *Science*. doi: 10.1126/science.abe8452

Stengers, I. (2018). *Another Science Is Possible: A Manifesto for Slow Science*. Cambridge: Polity Press.

Tabatabavakili, S., Khan, R., Scaffidi, M. A., Gimpaya, N., Lightfoot, D., and Grover, S. C. (2021). Financial conflicts of interest in clinical practice guidelines: a systematic review. *Mayo Clin. Proc. Innov. Qual. Outcomes* 5, 466–475. doi: 10.1016/j.mayocpiqo.2020.09.016

The National Academies of Sciences, Engineering and Medicine (Committee on Human Gene Editing). (2017). *Human Genome Editing*. Washington, DC: National Academies Press.

Thompson, D. F. (1993). Understanding financial conflicts of interest. *N. Engl. J. Med.* 329, 573–576. doi: 10.1056/NEJM199308193290812

Veit, G., Avramescu, R. G., Chiang, A. N., Houck, S. A., Cai, Z., Peters, K. W., et al. (2016). From cftr biology toward combinatorial pharmacotherapy: expanded classification of cystic fibrosis mutations. *Mol. Biol. Cell* 27, 424–433. doi: 10.1091/mbc.e14-04-0935

Wei, X., and Nielsen, R. (2019a). Ccr5-32 is deleterious in the homozygous state in humans. *Nat. Med.* 25, 909–910. doi: 10.1038/s41591-019-0459-6

Wei, X., and Nielsen, R. (2019b). Retraction note: CCR5-Δ32 is deleterious in the homozygous state in humans. *Nat. Med.* 25, 1796. doi: 10.1038/s41591-019-0637-6

Werhane, P., and Doering, J. (1995). Conflicts of interest and conflicts of commitment. *Prof. Ethics Multidiscip. J.* 4, 47–81. doi: 10.5840/profethics199543/413

Wilholt, T. (2010). Scientific freedom: its grounds and their limitations. *Stud. Hist. Philo. Sci. Part A* 41, 174–181. doi: 10.1016/j.shpsa.2010.03.003

Wilholt, T. (2012). *Die Freiheit Der Forschung: Begründungen Und Begrenzungen*. Berlin: Suhrkamp.

Wilson, R. C., and Carroll, D. (2019). The daunting economics of therapeutic genome editing. *CRISPR J.* 2, 280–284. doi: 10.1089/crispr.2019.0052

Wolinetz, C. D., and Collins, F. S. (2019). Nih supports call for moratorium on clinical uses of germline gene editing. *Nature* 567, 175–175. doi: 10.1038/d41586-019-00814-6

Yamamoto, T. (ed.). (2015). *Targeted Genome Editing Using Site-Specific Nucleases*. Tokyo: Springer. doi: 10.1007/978-4-431-55227-7

Zafer, M., Horvath, H., Mmeje, O., van der Poel, S., Semprini, A. E., Rutherford, G., et al. (2016). Effectiveness of semen washing to prevent human immunodeficiency virus (hiv) transmission and assist pregnancy in hiv-discordant couples: a systematic review and meta-analysis. *Fert. Steril.* 105, 645–655.e2. doi: 10.1016/j.fertnstert.2015.11.028

Zhang, L., Dai, F., Brackett, A., Ai, Y., and Meng, L. (2018). Association of conflicts of interest with the results and conclusions of goal-directed hemodynamic therapy research: a systematic review with meta-analysis. *Intensive Care Med.* 44, 1638–1656. doi: 10.1007/s00134-018-5345-z

Zhang, X.-H., Tee, L. Y., Wang, X.-G., Huang, Q.-S., and Yang, S.-H. (2015). Off-target effects in crispr/cas9-mediated genome engineering. *Mol. Ther. Nucleic Acids* 4, e264. doi: 10.1038/mtna.2015.37

Zhou, M., Greenhill, S., Huang, S., Silva, T. K., Sano, Y., Wu, S., et al. (2016). Ccr5 is a suppressor for cortical plasticity and hippocampal learning and memory. *eLife* 5, e20985. doi: 10.7554/eLife.20985.030

Conflict of Interest: The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

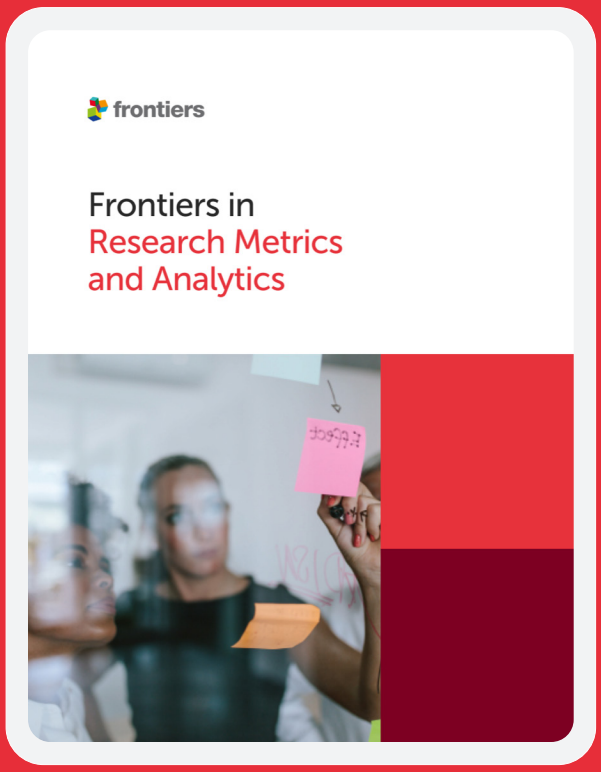
Frontiers in
Aging Neuroscience

Explores what drives excellence in methods of analysis

Provides a forum for the study of measuring, evaluating, and improving the efficiency, reliability, and transparency of research and innovation in all areas of scientific inquiry and applications.

Discover the latest Research Topics

See more →



Frontiers

Avenue du Tribunal-Fédéral 34
1005 Lausanne, Switzerland
frontiersin.org

Contact us

+41 (0)21 510 17 00
frontiersin.org/about/contact