Collective Responsibility and Fraud in Scientific Communities

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Some people rely on scientific data to decide whether a particular physical condition calls for a medical intervention. Others rely on scientific data to decide what they should and shouldn't eat. Still others attend to data from social psychology, economics, or climate science to determine which social policies they should support, and which they should resist. In these cases, and many others besides, the reliance on scientific data can shape a person's practical engagements with the world. This is at least part of the reason why scientists who intentionally manipulate data to confirm their hypotheses, or who publish results that they know to be false, are thought to violate moral as well as epistemic norms.

Fortunately, many scientists are committed to getting things right, and to telling the truth as they see it. And this should make outright fraud and fabrication the exception rather than the rule. Yet, questionable research practices that fall short of fraud and fabrication persist across the sciences (Fanelli 2009; John et al 2012). By dropping data points on the basis of gut feelings, refusing to publish data that contradicts their previous research, or citing papers that they take to be flawed or problematic, scientists can shape our understanding of the world in ethically problematic ways (Martinson et al 2005). It is

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difficult to draw a precise boundary between these questionable research practices and cases of outright fraud. But at minimum, a scientist who commits fraud must knowingly enter what they take to be falsehoods into the scientific record (Bright 2017, 291) This can take the form of fabricating data, claiming to have carried out an analysis without actually doing so, or claiming to have support for a hypothesis after knowingly omitting data that defeat this support. In many cases, fraudulent practices are carried out by scientists who know that they are acting against scientific norms (cf., Fallis 2009). But individual intentionality becomes less salient when responsibility becomes more diffuse and distributed.

Research in social ontology and the philosophy of science has started to explore the questions about who should be held responsible for harms that emerge as a result of the structure of scientific communities. Such approaches shift attention away from questions about individual responsibility, and toward the role of scientific communities in shaping the collection, analysis, and dissemination of scientific data. In this chapter, we build on this trend by examining some of the effects that institutional factors and collaborative practices have on moral responsibility in scientific communities.

1. A Theory of Fraud

Most scientists are concerned to pursue the truth, and many are concerned to produce elegant explanations of the phenomena they study. However, there is an enormous literature suggesting that scientists also operate within a *credit economy*, which

rewards *priority* in accordance with scientific *norms* (e.g., Latour & Woolgar 1979: ch. 5; Kitcher 1990; Dasgupta & David 1994; Stephan 1996; Zollman 2018). In this section, we provide an account of each of these italicized terms, and show how fraud can emerge through the interactions between these variables. Put briefly, we claim that scientists do not just seek truth, they also seek credit for their work; but scientific credit is only awarded to those who are perceived as being first to establish matters of scientific interest (that is, those who are seen to have priority for a discovery); and it is only awarded when research is thought to proceed in accordance with the norms that govern empirical research; however, we argue that there are some cases where the desire to seek credit can lead scientists to adopt questionable or fraudulent means of establishing priority for matters of scientific interest.

To begin with, most proponents of the theory of the scientific credit economy hold that scientists seek glory and esteem from their peers. Explicit markers of being held in high esteem include having numerous people cite one's work and discuss it favorably, receiving prestigious prizes or commendations, and having things like theorems, body parts, or disorders named in one's honor (cf. Cole & Cole 1970). They also include material and social advantages, such as the ease of finding a job, the location of the job that one receives, and the sorts of grants that one is likely to receive. Each of these factors, and many others besides, track and influence one's standing within the scientific community. A scientist with a high-status job is more likely to have their research cited widely and approvingly; having one's research cited widely and approvingly increases the likelihood that a scientist will receive grants to fund more research, and this in turn

increases the likelihood of citation. Over time, the relationships between markers of status tend to stabilize, yielding mutually reinforcing and resilient evidence of one's standing within a scientific community.

Credit is typically attained through priority, that is, by being seen as the first scientist to have established some matter of scientific interest (cf., Merton 1957). And it is commonly acknowledged that ascriptions of priority are governed by scientific norms, which specify when something should be counted as a successful discovery. We use the term "norms" loosely, as a way of covering any form of behavior that a community expects you to engage in, and would think it improper for you to deviate from. We cannot survey the literature on the social epistemic consequences of scientific social norms here (e.g., Longino 1990; Mayo-Wilson 2014; Heesen 2017b; Bright et al 2018; Rubin & O'Connor forthcoming). However, for our purposes, the important point is that scientific norms govern which research topics are worth pursuing, where things should be published, and the conditions under which scientific claims should be evaluated. For example, it is a norm that scientific claims should be published in accredited venues, and not just distributed over email or social media; and it is a norm that accredited venues should have a process of peer review, which verifies that the epistemic and social norms of a particular research community are properly upheld (Zuckerman & Merton 1971). This peer review process should ensure that proper statistical procedures or experimental protocols are followed; it should ensure that past priority claims are acknowledged through citation; and it should ensure that fraudulent claims are not entered into the scientific record.

Where things go well, aspirant credit seekers will have their work verified as following the proper epistemic and social protocols. Their work will be published and publicized by the journal they have submitted to. And this will allow them to gain whatever credit the scientific community sees fit to dispense when priority is granted for the discovery. However, in some contexts the norms that govern the assignment of scientific credit through priority can also tempt people to commit fraud, even though there are scientific norms against doing so. For, while many scientists are committed to the pursuit of truth, "a more immediate objective often intrudes into vision, that of establishing credit" Broad & Wade (1983: 52-53). And in some contexts, scientists may come to feel that fraudulent means are the only ones available for establishing priority on interesting discoveries. The pressure to pursue credit can therefore produce incentives to fabricate or manipulate data. This tends to happen where people's personal commitment to the restraining epistemic and social norms of science have been weakened; and this is especially likely where institutional pressures disincentivize the pursuit of truth, and where people begin to think that they will be able to get away with questionable research practices because pre-publication checking mechanisms turn out to be incapable of reliably detecting and preventing the publication of fraudulent research (cf., Zuckerman 1984, Ben-Yahuda 1986, Braxton 1993, Sovacool 2008, Casadevall 2012). What is important is that the desire for credit, and the ability to achieve it through establishing priority in accredited journals, can exist even where the commitment to the epistemic norms of science are either lacking or overpowered. In such cases pre-publication

checking mechanisms are rarely strong enough to prevent fraud from occurring.[2] Such effects are commonly acknowledged, even by those who are not entirely sympathetic to the standard theory of the cause of fraud. For example, a recent empirical study by Fanelli et al (2015: 13) suggests that the motivation for committing fraud is ‘‘to gain an unfair advantage in the race for priority and success’’. And in a paper focused on the case of ghost-management in pharmaceutical research, Elliot & Landa (2010) argue that “the structural logic of pharmaceutical public relations [i.e., *the socially entrenched credit economy in pharmaceutical research*] prevents its practitioners from engaging in ethical scientific communication, even if, as individuals, they are well intentioned.” We return to a more detailed account of these institutional pressures in Section 3. But for now, we simply suggest that a plausible understanding of what generates scientific fraud must acknowledge the impact of the credit economy.

2. Kinds of collective responsibility

Contemporary science often includes groups as aspirant credit seekers. And in many domains, collaborative science is becoming the norm. This is partly because of the complexity of addressing scientific questions; and it is partly because of the increased likelihood of receiving funding for interdisciplinary projects. Haixan Dang (submitted) draws on recent work in metaethics to suggest that there are three ways of construing

[2] While it is not exactly the same phenomenon, the problem of the garden of forking paths (Gelman & Lorken 2013) also contributed to the replication crisis by creating situations in psychology where checking mechanisms could not track norm compliance, while the temptation to establish priority and gain credit remained strong.

questions about collective responsibility in such scientific communities. First, there are questions about the parties to which a scientific outcome is attributable. Some scientific results are the result of individual efforts. Others are the result of the coordinated efforts of individuals working together. But in some cases, there will be a group who is attributable-responsible for an outcome, because of the claims that they make as a group, or the scientific procedures that they carry out as a group. Second, there are questions about who is answerable for a particular scientific claim. A group that is attributable-responsible for a scientific outcome will also be answerable-responsible for that outcome if they are in a position to produce the reasons that would justify it. As we argue below, answerability for scientific claims often becomes highly diffuse, in ways that make it impossible to find anyone below the level of a whole research team who is answerable-responsible for a scientific claim; and in some cases, it may be impossible to find any individual or group who is in a position to be answerable responsible for a scientific claim. Finally, there are questions about who should be held accountable for problematic scientific claims, and who should be praised for scientific insights; questions about accountable-responsibility turn on the proper target of praise or blame for a scientific product, and the proper targets of reward or punishment for making scientific claims that are true, erroneous, or fraudulent. And in general, a group can only be held accountable

for an outcome if they are attributable-responsible for it, and are in a position to adjust their behavior in light of the praise and blame that they receive.[3]

Our primary focus in this chapter is on collective responsibility for problematic scientific outcomes. However, numerous pressing questions about how to assign epistemic and moral responsibility to individuals and groups arise even in the context of good and neutral scientific outcomes (Merton 1968; Strevens 2006; Heesen 2017a). Specifically, there are questions about how credit should be divided among collaborators (Bruner & O'Connor 2017); and numerous scientific journals (e.g., *Nature, Science,* and *The New England Journal of Medicine*) have developed contributor policies that aim to make it clear who a scientific claim should be attributed to. In general, these policies are designed to clarify attributability, by making recording the names of everyone who makes a relevant claim, or carries out a relevant procedure. But by requiring all contributors be listed as authors, they also help to make it clear who should be held accountable for problematic outcomes, and who should receive credit for the reported research. And they constitute attempts to guard against the intrusion of hidden influences and conflicts of interest, by making it clear who has made specific contributions to a scientific paper. Consequently, these policies embed collaborative research more firmly within the networks of norms that govern the scientific credit economy. Each researcher receives

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[3] Dang (submitted) is primarily concerned with questions of epistemic responsibility. But she does note in passing that similar issues are likely to arise in the context of collective moral responsibility. We agree, and we argue in the remainder of this paper that questions about moral and epistemic responsibility are deeply connected, at least in the context of questions about scientific responsibility. Following Dang, we also suggest that an account of collective moral responsibility in science must begin by acknowledging that these three kinds of responsibility can converge or diverge as a result of the organization of a scientific research group.

credit for their own contributions; and where all collaborators know who is responsible for which aspects of a scientific project, it is easier to track down the points where distortions have emerged.

Many contributor statements go beyond considerations of attributability, in an attempt to clarify these distributed structures of answerability and accountability. This shouldn't be a surprise, especially where scientific research impacts the way that people conceptualize core values such as health, wellbeing, and treatment options. Such ethical considerations may help to explain why The International Committee of Medical Journal Editors (ICJME) recommends that someone should be counted as an author if and only if they have: 1) made a substantial contribution to the research design, or played a significant role in the acquisition, analysis, or interpretation of data; 2) drafted the manuscript or made substantial and intellectually significant revisions to it; 3) gave their approval on the final version of the manuscript; and 4) agreed to vouch for the accuracy and integrity of all aspects of the research. These robust constraints on authorship are designed "to ensure that contributors who have made substantive intellectual contributions to a paper are given credit as authors, but also that contributors credited as authors understand their role in taking responsibility and being accountable for what is published." (ICJME 2013) As a result, they draw considerations of attributability, answerability, and accountability together by specifying who carried out the research and writing, who can justify claims, and who should be held accountable if a problematic outcome arises.

This is all quite reasonable, as many forms of collaborative research preserve the unity of Dang's three proposed kinds of responsibility. Consider a small group of authors who toss a manuscript back-and-forth until they all agree that it is ready for submission (or who defer to statisticians and modelers when questions about their models arise). Such catch-and-toss collaborations preserve familiar forms of individual and shared responsibility, allowing each author to know who is able to address specific kinds of questions, and allowing collaborators to reconstruct justifications for their scientific claims where problems arise (Huebner et al 2017). Perhaps more importantly, they preserve the ability of co-authors to trust one another to follow scientific norms, and to figure out where problems have emerged in the process of collecting, analyzing, or interpreting data (cf., Andersen 2013). So, each contributor can justify their own contributions, and direct inquiries to collaborators where they lack relevant expertise: the scientists who are responsible for producing a scientific paper can be answerable for the claims that they make, and they can be treated as legitimate targets of praise or blame for the outcomes of their research.

In such contexts, scientific misconduct is typically performed by a single contributor—and while co-authors may be epistemically blameworthy for trusting a fraudster, they will rarely be morally culpable for the actions of that fraudster (Andersen 2014). There may also be cases where co-authors refuse to intervene on forms of misconduct that they are aware of; and in such cases, an agency-based approach to complicity may be relevant to the assignment of responsibility (Bazargan-Forward 2017). Such cases are likely to be relatively rare in small-scale collaborations; and even where

they arise, we will typically be able to figure out who is answerable for scientific misconduct, and who should be held accountable if problems arise. However, these patterns of trust and deference can be compromised, and this can have an enormous impact on the relationship between attributability, answerability, and accountability for research outcomes (here too, we follow Dang; however, our conclusions diverge from hers). Specifically, we contend that accountability for mistakes can dissipate in massively distributed research groups, even where it is clear who a claim is attributable to, and who carried out various scientific tasks; and we argue that external values can compromise answerability, in part by obscuring lines of attributably. We consider these possibilities in Sections 3 and 4 respectively.

3. Radically collaborative science

Some contemporary research is carried out by very large research groups, consisting of multiple labs, from different scientific fields, working in different parts of the world. In such contexts, social and material pressures shape the collection, analysis, and interpretation of data; and local norms shape each researcher's decisions about when to treat something as data, and when to treat it as noise (Douglas 2004; 2009). The contributors to large collaborations also worry about tenure and promotion; they try to run successful labs; they try to place their graduate students in high-status positions; they strive to cultivate the respect of their colleagues; and they hope to receive grants that will fund their research. And in the large-scale collaborations that are becoming increasingly

common in biomedical research and climate science, economic and political factors also come into play, shaping the way that data are collected, interpreted, and presented. In these respects, large-scale collaborations are not that different from any other scientific research; but in this context, such factors make it difficult to determine who is answerable for a scientific claim, and who is accountable when problems arise (for extended treatments of these issues, see Huebner et al 2017; Kukla 2012; Winsberg et al 2014).

The scientific questions addressed by large-scale collaborations require contributions from multiple researchers, who draw on tools and techniques from different disciplines, and who are working in different locations. And since these research communities are constituted by scientists who operate within partially distinct credit economies, and who make judgments on the basis of partially distinct networks of disciplinary skills and norms, their research outputs are typically shaped by "a chaotic web of micro-interests and local values that penetrate the study bottom-up." (Winsberg et al 2014: 17) Numerous judgments are often made in parallel, and methodological adjustments are often made "on the fly in response to noncompliant research participants, unforeseen barriers to implementation and communication, surprising side effects, and so forth." (Huebner et al 2017: 103) But the uncertainties that evoke such adjustments are rarely predictable in advance; and methodological adjustments are often made differently by different researchers, and at different stages of the research process.

In this context, subtle forms of epistemic distortion can emerge, even where everyone is committed to engaging in good epistemic practices. There is no obvious way to keep track of the impact of the adjustments that are made across the collaboration; and

there is no way to know whether their effects will aggregate, cancel each other out, or amplify one another. At each stage of the research process, incoming data that has been shaped in accordance with local norms can be re-evaluated in accordance with locally salient considerations; and the impact of this fact is often obscured due to the individual or team who is responsible for writing papers and producing other research outputs. Since it is unlikely that they have participated in the collection or analysis of data, writers function as a distinct component of the collaboration, and they tend to write in accordance with their own normative assumptions about what is worth presenting and what can be ignored. Of course, the output of this process is typically interpretable. But it is often impossible to determine how the collaboration arrived at the reported results.[4]

To clarify the ethical implications of this situation, we return to Dang's (submitted) tripartite distinction regarding different kinds of responsibility. In radically collaborative research, papers and other research outputs are typically attributable to widely-distributed networks of researchers, technicians, statisticians, and writers. And the author list on a publication typically constitutes at least a partial record of attributability (though technicians and lab managers may not be listed as contributors). But there is rarely any contributor who fully understands the roles that are played by all of the other researchers; and there is rarely any contributor who can vouch for the results of every other researcher (Winsberg et al 2014; Wray 2017: 127). Consequently, such research

[4] Drawing on an argument advanced by Leonard & Winsberg (2010: 256-257), we might see such collaborations as kludged architectures, which display a sort of fuzzy modularity: each research group is organized in accordance with the norms of their local credit economy, so their data and inferences are shaped by a mixture of principled science and locally salient practices of credit-seeking; and the interactions between these groups shape the content of the information that is propagated through such collaboration, as data are continuously exchanged between research groups, and as queries are made for more data or further interpretations.

cannot proceed in accordance with the constraints on authorship advanced by the ICMJE (2013). And as a result, the research can only be attributable to the collaborative group, who have collectively produced the relevant claims and carried out the relevant procedures. As information is propagated through a radical collaboration, this situation can be complicated by errors that are introduced as a result of explicit manipulation, or as an artifact of divergent norms operating in different parts of the collaboration; and where such errors are entered into the scientific record, it will be difficult to discover or correct them, as the precise locus of attributability for the error will be obscured by the size and complexity of the experimental or observational design. Where these errors yield problems with research outcomes, there may be no individual or collective agent who is in a position to produce the reasons that would justify a problematic claim; and if this occurs, it there will be no individual or group who can be answerable-responsible for the problematic claims that have been made (Huebner et al 2017; Kukla 2012). Finally, when there is no one who controls all of the knowledge that is necessary to justify the procedures that have been used in the production of scientific claims in such a context, there will be no individual or group who can be a proper target of praise or blame for scientific outcomes; and this can yield a situation where individual, shared, and collective accountability all dissipate (Winsberg et al 2014).

The scope of these kinds of research projects can thus compromise the otherwise stable link between the best features of the scientific credit economy and the process of scientific research. This can occur when highly distributed and massively interdisciplinary research leads to the breakdown of the norms that typically constrain

individuals in the pursuit of scientific credit, because the communal oversight of adherence to those norms has been weakened. First, co-authors on such projects will often object to the retraction of a paper, attempting to hold on to any credit they have received for a massively multi-authored paper. Second, there is little motivation to take part in attempted replications of large-scale collaborative research projects with problematic implications. The credit one receives as the 164th author in a field of 400 authors is both minimal and diffuse; and the credit one would receive for pursuing a replication, which is not likely to establish priority, is even more minimal. Third, no one can reasonably be punished if problems arise within a large-scale collaboration, as both answerability and accountability become so diffuse in a network of hundreds of distributed researchers that it only makes sense to hold the collaboration as such accountable; unfortunately, attempts at censoring radical collaborations are likely to fail, since such collaborations are often kludged together for a specific purpose, and they often dissipate after the projects are completed. Fourth, and finally, this kind of research has a serious impact on the checking mechanisms that are typically at play in the peer review process. When "research projects engage a greater proportion of the scientists working in a specialty, there are fewer and fewer competent scientists available to referee the resulting research." (Wray 2017: 129ff) In the context of massively interdisciplinary research, which draws on many different research areas, it is unlikely that anyone is competent to evaluate the research project as a whole; consequently, the more limited pool of referees is likely to impose significant constraints on the ability of the scientific

community to track cases of where questionable research practices are employed, or where fraud has been carried out.

Such collaborations thus compromise the checking mechanisms that serve to prevent fraudulent claims from being entered into the scientific record. And where problems emerge—whether unintentionally, or as a result of fabrication and manipulation—it becomes difficult, and perhaps impossible, to figure out who is answerable-responsible for the scientific claims that are made, and who should be held accountable when problems arise, even if it is possible to attribute a paper to the widely distributed research group. Intriguingly, similar problems arise where heavily managed and systematically biased research is produced in accordance with a single set of non-scientific values. Here too, questions arise about who is responsible for the production of a scientific claim, who is answerable for it, and who should be held accountable when problems arise.

4. Ghostwriting and market values

In recent years, for example, a troubling pattern has emerged in pharmaceutical research: despite strong evidence from clinical trials, as well as enthusiastic support from physicians, problematic outcomes emerge when people begin to use a drug (e.g., high rates of cardiovascular disease, diabetes, or hypertension, or suicidal ideation); we later learn that the drug company was aware of the potential for problems before the drug went to market, that they actively worked to downplay the risks of using the drug, and used

ghostwritten publications to get the drug quickly and efficiently to market (Elliot & Landa 2010). This pattern began to emerge in the late-90s, as drugs such as Paxil, Neurontin, Fen-Phen, and Zoloft triggered a range of aversive effects; and there are many detailed accounts of these cases (see Sismondo & Doucet 2010 for a review). In pharmaceutical research, it is often necessary to increase the efficiency and speed of publication; and professional writers are often used to ease the pressures on researchers and clinicians, who may have little time to write a paper. And in these cases, and many others besides, research was either carried out or written up "on behalf of pharmaceutical companies, and then published under the name of academics who had played little role earlier in the research and writing process." (Sismondo 2007: 1429)

By definition, these forms of ghostwriting must remain hidden from view. So we don't know how commonplace they are. Moreover, there is little consensus about how to understand the ethical implications of ghostwriting. It is widely assumed that moral problems are likely to arise when professional writers are employed to rush a paper to publication, and when research is guided by the interests of pharmaceutical companies. Philosophers and journal editors have worried that using industry-sponsored ghostwriters can compromise the pursuit of truth by centering market values; and it is sometimes suggested that plagiarism, or a nearby violation of academic integrity, occurs when a scientist puts their name on a paper that they have played little role in writing or revising (Anekwe 2009; McHenry 2010). We contend that ghostwriting also yields a countervailing force that can compete with, and perhaps overcome the allegiance to the

epistemic norms of science. And given the theory of fraud outlined in Section 1, this is precisely the kind of situation that one ought to be concerned about.

When professional writing services are used in pharmaceutical research, the roles of various named authors often remain obscure. In several cases, there is evidence that industry interests have played a role at every possible point during the research process: from the specification of experimental design, to the analysis and interpretation of data, ghost-managed research aims to make it "easier for pharmaceutical companies to use scientific research to market their products." (Sismondo & Doucet 2010: 275) But achieving these ends requires both increasing the prevalence of research supporting a product in the academic literature, and doing so in ways that make use of high-impact journals, with the assistance of prominent scholars, universities, or research institutions. Put differently, within the existing scientific credit economy, ghostwritten articles can only be useful as marketing tools if "they appear to come from a disinterested source." (Moffat & Elliott 2007: 27) And where there is evidence of a robust conflict of interest, scientific norms preclude the publication and dissemination of data. To work around these norms, pharmaceutical companies have taken advantage of the fact that many academics are willing to participate in ghost-managed research; as credit seekers, academics "have a strong interest in publications, particularly in prominent journals." (Sismondo & Doucet 2010: 275) And this is what leads to cases where pursuing scientific truth is disincentivized, while the desire to pursue credit by co-authoring problematic research is enhanced.

It is worth pausing to make it clear why someone would willingly accept credit for a paper, when they played no role in its production. As we see it, two related factors are at play in shaping this kind of situation.

1. Industry-sponsored and ghost-managed research tends to be placed in higher-impact journals, and it tends to be cited more frequently than non-industry-supported research (Healy & Cattell 2003; Sismondo & Doucet 2010: 278). In part, this may be because strategic 'publication plans' are used to shepherd industry-supported research through the publication process, from the presentation of research at high prestige conferences, to the placement of publications in prominent journals; and professional writers are paid to write compelling and readable articles on the basis of this research. Consequently, medical journals may be more likely to publish ghost-managed research. But in part, this may also be because pharmaceutical companies often purchase large numbers of reprints to distribute to physicians (Sismondo & Doucet 2010: 275).

2. Existing norms governing scientific authorship allow some people to put their names on nearly completed papers. Cultures of honorary authorship allow the leader of a large lab to be listed as an author, even where they have contributed little to a manuscript. And authorship is sometimes conferred on the basis of theoretical contributions that were made in conversation. There may be good

reasons to worry about the prevalence of these practices. But on their own, they need not compromise the pursuit of scientific truth.

Through the interaction of these factors, a situation emerges where a scientist might come to believe that they can get away with putting their name on a ghost-written paper. And in this context, they have some incentive to use this low-cost strategy of pursuing academic credit. Of course, many scientists will refuse to take part in this kind of research, as scientific norms play a more significant role in their decision-making than do industry norms. However, the social organization of this kind of research does open up the possibility of engaging in questionable research practices, and potentially fraudulent behavior.

Industry-financed writers can take advantage of this situation to produce articles that promote a company's interests, while a "key opinion leader" accepts credit for a paper that they have played no substantial role in writing (McHenry 2010). Such practices obscure the role that industry interests have played in determining which data are presented and which data are ignored; and this makes ghost-authored papers look like they are ordinary contributions to scientific knowledge. But perhaps more significantly, ghostwriting and ghost-management can bring about the diffusion of collective moral responsibility in a way that is similar to the diffusion of responsibility that emerges in the contest of large-scale collaborative research (See Kukla 2012). To see what this amounts to, we return to Dang's (submitted) tripartite distinction between different kinds of responsibility.

To begin with, where papers are ghostwritten, it becomes much more difficult—if not completely impossible—to figure out who a scientific product should be attributed to. In the most extreme cases, the named authors have not made any substantial contribution to the research design, and they have not played any significant role in the acquisition, analysis, or interpretation of data; they have not drafted the manuscript, or made substantial and intellectually significant revisions to it. They have simply signed off on the final version of the manuscript. Put somewhat differently, it is not just that the causal story is complicated (as it was in the cases we discussed in Section 3), attributable-responsibility is compromised in these cases because the people who are given credit for the relevant research have played no causal role in its production. Consequently, papers are attributed to people who are in no real position to vouch for the research, or to offer justifications where they are called upon to do so; and this means that there may be no one who is answerable-responsible for the research product. That said, the presence of a ghostwriter doesn't itself compromise answerability and accountability. These further problems arise precisely because ghost-managed manuscripts are produced with the intent of obfuscating the role of monetary interests; and where such interests can be hidden, this will increase the potential for manipulations of data that accord with the interests of pharmaceutical companies. Put somewhat differently, where research is ghost-managed and where manuscripts are ghostwritten, considerations of attributability are intentionally distorted; and this is done in a way that makes it difficult for anyone to figure out who precisely is answerable-responsible and who is accountable-responsible for the research.

The research guiding decisions that are made in this context are "rarely, if ever, made by isolated individuals." (Biddle and Kukla 2016: 229) They are typically made by loosely connected networks of pharmaceutical representatives, researchers, scientists, and professional writing companies. This distribution and obfuscation of attributably causes a situation where answerability becomes diffused, as no identifiable individual or group has the capacity to produce the reasons that would justify a scientific outcome; and the diffusion of accountability follows, as there is no identifiable individual or group who is a proper target of praise or blame for the scientific outcome, and no identifiable individual or group who is justly accorded reward or punishment for making claims that are true, erroneous, or fraudulent. While there may be some cases where it is possible to figure out who collected, analyzed, and interpreted the data for a ghost-written paper, the lack of transparency at every level of the research process can make it impossible to figure out who is answerable, and who should be held accountable for problematic research. And pushing on the named author or "key opinion leader" will at most lead to the acknowledgment that they appended their name to a research product they did not really contribute to. The result is that where corporate influences intrude, and where conflicts of interest distort the pursuit of truth, both answerability and accountability can be compromised. Where pressures from industry shape research decisions from the top-down, they will tend to privilege market interests over the pursuit of scientific truth (Moffatt and Elliott 2007; Sismondo 2009). This pressure opens up the possibility of fraud, as well as other questionable research practices. And since ghostwritten papers are designed to bring a product rapidly to market, or to increase its market share, the norms

governing the authors behavior are likely to be those of the market, not those of the scientific community. In this context, supporting data will often be highlighted, while contradictory findings will tend to remain unpublished. And this can yield distortions of the scientific process, which can have massively problematic effects.

As with the cases we discussed in the previous section, these kinds of ghost-managed and ghost-authored research make it difficult to assign responsibility for fraudulent research to any particular individual. Epistemically, it is often unclear who the pertinent claims should be attributed to, even where there is a fact of the matter. In the case of widely distributed research, it is not clear that anyone is actually answerable or appropriately accountable—no one stands in the right kind of relationship to provide a justification or to be held accountable if the results of a study are inappropriate. In the case of ghostwritten research, by contrast, the intentional obfuscation of authorship makes it at least impractical to design institutional mechanisms that can be used to hold people accountable for the research that they have actually done. Where fraudulent claims emerge, there will be someone who is causally responsible for entering the problematic claim into the scientific record. But it is not the named author; and the process that lead to the production of the claim is likely to be a such a complex and intractable mixture of legitimate science and industry-based interests that it will be impossible to track down where the distortion was introduced in the research process. What is more, since the group membership and structure is opaque for the same reasons the individual contributions are, it is just as difficult to hold the group answerable or accountable for any wrongdoing. If there are many cases where no individual nor research group may be

held accountable for fraud, this leads naturally to the next question: what ought to be done about ameliorating fraud?

5. Communal Responsibility for Ameliorating Fraud

The standard theory of fraud affects the dominant theories of how to respond to scientific fraud. It is commonly suggested that attempts to address the prevalence of scientific fraud should aim to reduce alienation from the norms of science and to increase respect for more distinctively scientific norms (e.g. Bright Forthcoming; Nosek et al 2017, though see Bright 2017 for a note of skepticism). An alternative suggestion is that we can do more to encourage the effective use of checking mechanisms (e.g. Bruner 2013; and discussed in Lee 2013, Romero 2016). Whichever response is preferred—they are not inconsistent—such solutions proceed in a technocratic vein, by assessing the extent to which the causes of fraud can be addressed so as to ensure that there is less of it. We, however, are interested in a more distinctively moral question: who ought to be morally responsible for reducing the prevalence of fraud?

In an ideal world, each individual would work to avoid making fraudulent claims, and each individual would hold others accountable where fraud was detected. Over time, this would become the norm, yielding a fraud-free science. But fraud has not been eliminated, incentives to commit fraud remain in place, and we cannot plausibly assume full compliance with scientific norms. So we must look for non-ideal strategies for ameliorating the problematic effects of fraud, including the spread of falsehoods, the

failure of policies based upon false beliefs, the erosion of trust in the scientific literature, and the breakdown of respect for the norms that allow for epistemically successful publications.

Here too, we build on Dang's three senses of 'collective responsibility'. As we noted above, someone is attributable-responsible if they made the pertinent claim or carried out the relevant procedure. We can typically attribute fraudulent claims or procedures to particular scientists. Doing so can become difficult or impossible in the two cases we have just discussed; but even here, there must be some individual or group who is causally responsible for producing a claim, even if we can't figure out precisely who is attributable-responsible for it. And this can yield situations where standards of answerability and accountability become difficult to establish. Recall that someone is answerable-responsible for a claim if they are in a position to produce the reasons that would justify it. Answerability for scientific claims (fraudulent or not) is often diffuse, and whole research teams may sometimes be collectively answerable-responsible for scientific claims. Moreover, in the kinds of cases we have discussed above, answerability tends to break down, generating a situation where no one can answer for problematic claims. Perhaps the scientific community as a whole might somehow be answerable for fraudulent claims that arise in such cases, though difficult questions remain as to what this would amount to. Finally, someone is accountable-responsible if they are the proper target of praise or blame for a scientific product, or if they are justly accorded reward or punishment for making claims that are true, erroneous, or fraudulent. Given the highly social nature of science, we contend that the most plausible thing to say is that the

community as a whole is accountable for fraud; and to the extent that we are justified in treating individuals as accountable for fraud, this is because this is a plausible way for the community to fulfill its responsibility for preventing and ameliorating the mal-effects of fraud. This is a complex and contentious claim, so it will help to work through it in more detail.

What does it mean to say that the scientific community as a whole is accountable for the amelioration of fraud? On the one hand, the scientific community is unlikely to satisfy any plausible theory of the constraints on agency. As a 'community' it is far too dis-unified, and far too widely distributed, to carry out any sort of coordinated and intentional actions; moreover, even where trends do emerge within this community, they are rarely under the rational control of any individual or collective agent. So the community, understood as an agent in its own right is probably not the right place to look in attempts to change the norms and expectations that give rise to fraudulent action. That said, there is another sense in which the scientific community can be held accountable for the emergence and prevalence of fraud. The members of the scientific community are causally responsible, *qua* members of the scientific community, for the norms that govern attributions of priority within the scientific credit economy. No individual can determine which norms are accepted by the community. And no individual can change problematic norms where they become stable. But when scientists act together as members of the scientific community, they can begin to shift the salience of expectations to act in particular ways. And over time this can lead to a shift in the way that the scientific community as a whole operates.

Social norms do not float free from the expectations of the individuals in a community. But at the same time, the expectations that the individuals within a community have depend on the patterns of behavior that are observed within interconnected networks of social actors. As members of a norm-governed community, scientists tend to share numerous values, at least in the minimal sense that they each see themselves, and they each see one another, as possessing the standing to demand compliance with scientific norms. And this tends to be true even where expectations regarding norm compliance are not grounded in joint commitments or shared intentions (cf., Hedahl & Huebner 2018). As a result, the stability and prevalence of the norms that allow fraud to emerge can be properly attributed to the networks of social actors who constitute the scientific community. Moreover, since the presence of fraud depends on the presence of norms and incentives that are inherent in the network structure of the scientific credit economy, ameliorative strategies will require a shared commitment among them members of that community to transform the content and the salience of the norms governing scientific practice. So when we claim that the community as a whole is accountable for fraud, what we mean to suggest is that the proper target of praise and blame in the case of fraud is the interconnected network of scientists, who act in accordance with their roles as community members, and who shape the salience of scientific and credit-seeking norms.

Only the community as a whole has the power to bring about the reforms that could increase or decrease the prevalence of fraud in the non-ideal world. There may not be an agent which constitutes the scientific community, but effective anti-fraud action can

only happen through change that works upon this diffuse network of interlinked scientists, enforcing norms and constituting credit through their praise and esteem, and thus generating different expectations about risks and reward. But this must occur through the actions of individual scientists, who are working to reshape the credit seeking economy. According to the theory outlined in Section 1, scientific fraud tends to arise as a result of the incentive structure that scientists operate within, as well as the efficacy of the social norms that scientists are supposedly subject to. When fraudulent claims are entered into the scientific literature there is always some individual or identifiable group to whom the claim is properly attributable. But no individual or group that is smaller than the whole community has the power to modify the credit system, or to shape the norms that will increase or decrease the prevalence of fraud in the non-ideal world that we inhabit. As such, we contend that only the community as a whole can see to it that there is less fraud; that the scientific community as a whole must actively work to minimize scientific fraud, and it collectively should be praised or blamed for arranging itself in ways that yield greater or lesser amounts of fraud.

As to how this responsibility should be discharged, we think the scientific community is broadly on the right track already. We should bring credit incentives into line with obeying epistemic norms of science, foster respect for those norms, and avoid anomie. As such, educational institutions should do their best to inculcate and encourage a sense of honesty and allegiance to the epistemic norms of science (Du Bois 1898). Initiatives such as encouraging pre-registration to ensure fraudulent research practices do not yield accredited publications ought to be supported (Veer & Giner-Sorolla 2016).

Moreover, robust uses of checking mechanisms via replication (OSC 2015), and attempts at multi-method triangulation (Munafò & Davey-Smith 2018), should be used to ferret out false claims that have already been entered into the literature, making it less likely that scientists will gain lasting priority credit for claims that are false. Together, these practices will help disincentivize fraud. Additionally, mechanisms should be put in place to ensure that taking part in such practices is rewarded in the credit economy. This might involve favored access to funding, rewards, or publication venues for studies of the required form. Finally, where there is evidence of malpractice, ensuring that there are procedures in place for retraction and punishment will further disincentivize fraudulent behavior. This may involve making individuals or groups answer for the instances of fraud they commit. But individual instances of fraud are always embedded in larger social patterns; and the agent that is answerable for regulating these patterns is the scientific community that is constituted by networks of interacting individuals and institutions.

6. Works cited

Andersen, H. (2013). Epistemic dependence in contemporary science: practices and malpractices. In *Science After the Practice Turn in the Philosophy, History and Social Studies of Science* (pp. 161-173). Routledge.

Andersen, H. (2014). Co-author responsibility: Distinguishing between the moral and epistemic aspects of trust. *EMBO reports*, 15(9), 914-918.

Anekwe, T.D. (2010). Profits and plagiarism: The case of medical ghostwriting. *Bioethics*, 24, 267–272.

Bazargan-Forward, S. (2017). Complicity. In M. Jankovic & K. Ludwig (eds.), *The Routledge Handbook of Collective intentionality*. London: Routledge, 327-337.

Ben-Yehuda, N. (1986). Deviance in science—Towards a criminology of science. *British Journal of Criminology*, 26 (1): 1-27

Biddle, J., & Kukla, R. (2017). The geography of epistemic risk. In Elliott, K. C., & Richards, T. (eds.), *Exploring Inductive Risk. Exploring Inductive Risk: Case Studies of Values in Science*. New York: Oxford University Press, 215-237.

Braxton, J.M. (1993). Deviancy from the norms of science: The effects of anomie and alienation in the academic profession. *Research in Higher Education*, 34 (2): 213-228

Bright, L.K. (2017). On Fraud. *Philosophical Studies*, 174 (2): 291-310

Bright, L.K. (Forthcoming). "Du Bois' democratic defence of the value free ideal." *Synthese*: 1-19.

Bright, L.K., Dang, H., & Heesen, R. (2018). A role for judgment aggregation in coauthoring scientific papers. *Erkenntnis*, 83 (2): 231-252

Broad, W. & Wade, N. (1982). *Betrayers of the Truth*. Simon and Schuster

Bruner, J.P. (2013). Policing epistemic communities. *Episteme*, 10 (4): 403-416

Bruner, J.P. & O'Connor, C. (2017). Power, Bargaining, and Collaboration. In T. Boyer-Kassem, C. Mayo-Wilson, & M. Weisberg (eds.). *Scientific Collaboration and Collective Knowledge: New Essays*. Oxford University Press, 135-157.

Casadevall, A., & Fang, F.C. (2012). Reforming science: Methodological and cultural reforms. *Infection and Immunity*, 80 (3): 891–896.

Cole, J.R. and Cole. S. (1970). *Social Stratification in Science*. University of Chicago Press

Dang, H. (submitted). Epistemic Responsibility in Science.

Dasgupta, P. & David, P.A. (1994). Toward a new economics of science. *Research Policy* 23 (5): 487--521.

Douglas, H. (2004). "Prediction, explanation, and dioxin biochemistry: Science in public policy." *Foundations of Chemistry*, 6 (1): 49–63.

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

Du Bois, W.E.B. (1898). The study of the Negro problems. *The Annals of the American Academy of Political and Social Science*, 11(1), 1-23.

Elliott, C., & Landa, A. S. (2010). Commentary: What's wrong with ghostwriting?. *Bioethics*, 24(6), 284-286.

Fanelli, D. (2009). How Many Scientists Fabricate and Falsify Research? A Systematic Review and Meta-Analysis of Survey Data. *PLoS One,* 4(5), p.e573.

Fanelli, D., Costas, R., & Lariviere, V. (2015). Misconduct policies, academic culture and career stage, not gender or pressures to publish, affect scientific integrity. *PLoS ONE*, 10 (6): 1–18

Fallis, D. (2009). What is lying?. *The Journal of Philosophy*, 106 (1): 29--56.

Gelman, A., & Loken, E. (2013). The garden of forking paths: Why multiple comparisons can be a problem, even when there is no "fishing expedition" or "p-hacking" and the research hypothesis was posited ahead of time. *Department of Statistics, Columbia University*.

Healy, D., & Cattell, D. (2003). Interface between authorship, industry and science in the domain of therapeutics. *The British Journal of Psychiatry*, 183(1), 22-27.

Heesen, R. (2017a). Academic superstars: competent or lucky? *Synthese,* 194 (11): 4499--4518

Heesen, R. (2017b). Communism and the Incentive to Share in Science. *Philosophy of Science*, 84 (4): 698--716

Huebner, B., Kukla, R., & Winsberg, E. (2017). Making an author in radically collaborative research. In T. Boyer-Kassem, C. Mayo-Wilson, and M. Weisberg (eds.), *Scientific Collaboration and Collective Knowledge*. Oxford: Oxford University Press, 95-116.

John, L.K., Loewenstein, G., & Prelec, D. (2012). Measuring the prevalence of questionable research practices with incentives for truth telling. *Psychological science*, 23(5), 524-532.

Kitcher, P. (1990). The division of cognitive labor. *The Journal of Philosophy*, 87 (1): 5--22

Kukla, R. (2012). "Author TBD": Radical collaboration in contemporary biomedical research. *Philosophy of Science*, 79(5), 845-858.

Latour, B. & Woolgar, S. (1979). *Laboratory Life: The Social construction of Scientific Facts*. Princeton University Press.

Lee, C.J. (2013). The limited effectiveness of prestige as an intervention on the health of medical journal publications. *Episteme*, 10(4): 387--402.

Longino, H.E. (1990). *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton University Press

Martinson, B.C., Anderson, M.S., & De Vries, R. (2005). Scientists behaving badly. *Nature*, 435(7043), 737.

Mayo-Wilson, C. (2014). "Reliability of testimonial norms in scientific communities." *Synthese*, 191 (1): 55--78

McHenry, L.B. (2010). Of sophists and spin-doctors: Industry-sponsored ghostwriting and the crisis of academic medicine. *Mens Sana Monographs*, 8, 129–145.

Lenhard, J. & Winsberg, E. (2010). Holism, entrenchment, and the future of climate model pluralism. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 41(3), 253-262.

Merton, R.K. (1957). Priorities in scientific discovery: a chapter in the sociology of science. *American Sociological Review*, 22 (6): 635--659

Merton, R.K. (1968). The Matthew effect in science: The reward and communication systems of science are considered. *Science*, 159 (3810): 56-63

Moffatt, B., & Elliott, C. (2007). Ghost marketing: pharmaceutical companies and ghostwritten journal articles. *Perspectives in Biology and Medicine*, 50(1), 18-31.

Munafò, M.R. & Smith, G.D. (2018) Robust research needs many lines of evidence. *Nature,* 533(1): 399--401.

Nosek, B.A., Spies, J.R., & Motyl, M. (2012). Scientific Utopia II: Restructuring incentives and practices to promote truth over publishability. *Perspectives on Psychological Science*, 7(6): 615–631

Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. *Science*, 349(6251), aac4716.

Romero, F. (2016). Can the behavioural sciences self-correct? A social epistemic study. *Studies in the History and Philosophy of Science: Part A*, 60 (1): 55--69

Rubin, H. & O'Connor, C. (Forthcoming). Discrimination and collaboration in science. *Philosophy of Science*.

Sismondo, S. (2007). "Ghost management: How much of the medical literature is shaped behind the scenes by the pharmaceutical industry?" *PLoS Medicine*, 4(9):e286.

Sismondo, S. (2009). "Ghosts in the machine: Publication planning in the medical sciences." *Social Studies of Science*, 39 (2): 171–198.

Sismondo, S., & Doucet, M. (2010). Publication ethics and the ghost management of medical publication. *Bioethics*, 24(6), 273-283.

Sovacool, B.K. (2008). Exploring scientific misconduct: Isolated individuals, impure institutions, or the inevitable idiom of modern science? *Journal of Bioethical Inquiry*, 5(4): 271–282

Stephan, P.E. (1996). The Economics of Science. *Journal of Economic Literature*, 34 (3): 1199-1235

Strevens, M. (2006). The role of the Matthew effect in science. *Studies in History and Philosophy of Science Part A*, 37 (2): 159--170

van't Veer, A. E., & Giner-Sorolla, R. (2016). Pre-registration in social psychology—A discussion and suggested template. *Journal of Experimental Social Psychology*, 67, 2-12

Winsberg, E., Huebner, B., & Kukla, R. (2014). Accountability and values in radically collaborative research. *Studies in History and Philosophy of Science: Part A*, 46, 16-23.

Zollman, Kevin JS. 2018. "The Credit Economy and the Economic Rationality of Science" *Journal of Philosophy*, 115 (1), 5--33

Zuckerman, H. & Merton, R.K. (1971). Patterns of evaluation in science: Institutionalisation, structure and functions of the referee system. *Minerva*, 9 (1): 66--100

Zuckerman, H. (1984). Norms and deviant behavior in science. *Science, Technology, & Human Values*, 9 (1): 7-13