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Article

The Academic Scientist's Commitment to Epistemic Responsibility

Bor Luen Tang

Department of Biochemistry, Yong Loo Lin School of Medicine, National University of Singapore, Singapore 117596, Singapore; bchtbl@nus.edu.sg

Abstract: Questionable research practices (QRPs) and research misconduct (RM) involving university scientists waste resources and erode public trust in science and academia. Theories put forth for the occurrence of these transgressions have ranged conceptually from that of errant individuals ("bad apple") to an environment/culture which is conducive for, if not promotive of, QRP/RM ("bad barrel"), or a combination of both. These ideas appear to provide explanations for lapses in epistemic responsibility and offer reasons for instances of transgression. Some have even argued that scientific conclusions need not be accurate, justified, or believed by their authors. I take the opposite view and instead argue that academic research should be carried out such that a scientist's fundamental commitment to their epistemic responsibility triumphs over all reasons and incentives to err. In other words, carrying out and publishing research in which the results are authentic and veracious to the best of the scientist's knowledge and ability should be a default state of mind, a preferred path of action, or a moral axiom. This is a notion that should permeate any courses on research ethics and integrity.

Keywords: epistemic responsibility; research misconduct; research ethics

1. Introduction

Prevailing data suggest a widespread occurrence of research misconduct (RM) and questionable research practices (QRPs) [1]. RM, as defined by the US Office of Research Integrity, involves acts of fabrication, falsification, and plagiarism of research results and publications [2], while the Council of Science Editors categorizes RM into the following areas: (a) the mistreatment of research subjects, (b) the falsification and fabrication of data, and (c) piracy and plagiarism. [1] QRP, on the other hand, involves "... a range of activities that intentionally or unintentionally distort data in favor of a researcher's own hypotheses—or omissions in reporting such practices—including selective inclusion of data, hypothesizing after the results are known (HARKing), and p-hacking" [2] [3]. These fraudulent practices are surprisingly common. A recent meta-analysis of reports revealed that 2.9% of researchers surveyed have committed an act of RM and 12.5% one of QRP. Furthermore, 15.5% of respondents have witnessed others committing RM, while 39.7% were aware of others having engaged in QRPs [4]. RM and QRPs contribute to falsehood and irreproducibility in research [5], incur a waste of public resources, and erode public trust in science and academia. The latter misgivings would likely be fueled by widely publicized cases of QRPs/RM involving renowned university academics in the past few months alone [6–8]. However, what is more worrying is that scientific misconduct has steadily risen over the past several years [9,10]. On the other hand, the phenomenon of research irreproducibility in research has appeared in all scientific fields of work [5], and this is at least partly due to QRP/RM.

Why do scientists engage in acts of QRP/RM and why are erected rules not enough to stop these acts? Some explanations offered over the years surround a combination of two major causes—errant individuals ("bad apples") with various reasons to deceive and

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a somewhat dysfunctional research environment ("bad barrel") that either facilitates or fails to detect/deter fraudulent acts (or both) [11,12]. Benjamin Sovacool identified three distinct narratives concerning scientific misconduct. Other than "individual impurity" and "institutional impropriety", he also recognized a third narrative of "structural crisis" that critiques the entire process of research itself [13]. Building on Sovacool's three narratives, Haven and van Woudenberg [14] constructed six theories to explain RM. Four of these theories concern the individual (rational choice, bad apple, general strain and prospect theories), while one organizational justice theory focuses on institutional factors, and one on new public management targets the system of science. These narratives and theories have all provided some degree of insight into the perpetration and occurrence of RM.

Recently, thoughts have moved from these fault-based reasons towards somewhat more conceptual explanations for QRPs/RM based on qualitative survey studies [15,16]. One refreshing conceptual explanation for the phenomenon of irreproducibility, presented in the form of hyper-ambitious characters prone to aberrant behaviors to move ahead of others in terms of achievements in research, was recently offered by Yasemin Erden [17]. Perhaps a common trait found in these hyper-ambitious individuals is a focus on churning out publications, even by cutting corners in research and even when results are not yet completely validated or reproduced. Scientific research entails a good deal of epistemic uncertainty. Although the veracity and accuracy of results and conclusions are highly valued in academic science, it remains unclear whether there should be a moral mandate for these to be strictly adhered to. Citing a case study from William H. Bragg's work on X-ray, Dang and Bright have indeed argued that "... scientific conclusions need not be accurate, justified, or believed by their authors" [18]. However, it would appear illogical and morally indefensible if scientists only want attributability but cannot or would not assume responsibility for what they publish. At the very least, a scientist should only publish an account of their work if she herself is certain of its validity and veracity. I would call such a baseline level of technical and moral commitment the fundamental epistemic responsibility of a scientist towards science and its other stakeholders, including colleagues and the public [19–21].

In the Sections that follow, I shall briefly review the very realistic notion of the hyper-ambitious individual postulated by Erden and point out what might be weaknesses in Dang and Bright's arguments. I shall then counterpose my own view that a scientist's fundamental commitment to their epistemic responsibility should triumph over any reasons or incentives to err.

2. Hyper-Ambitious Individuals Flourishing in a Hyper-Competitive Academic Environment

Erden introduced the concept of 'hyper-ambition' in academia that could contribute towards irreproducibility, as hyper-ambitious individuals would cut corners or engage in questionable practices [17]. In doing so, she charted the imaginary career paths of two hypothetical individuals within the same academic environment, one hyper-ambitious while the other much less so. It appears that in an intensely competitive academic environment that values high levels of ambition, the former individual would have an edge (despite a higher likelihood to also go astray in terms of research integrity), and the latter might falter in terms of career progression. Codes of proper research conduct would apply to both, but for individuals who prioritize success above all, Erden thinks that they might simply find ways around these. Erden's description is very clearly reflective of what we actually see in academia [22], and her recommendation of implementing "... robust 'top down' measures that expect and accommodate a broader range of academic values, motivations, and tendencies, while challenging those that help to promote hyper-ambition ..." would be good advice to follow.

While I see no flaws in Erden's arguments and projections, I would nonetheless point out that there is a line that any academic, no matter how ambitious, must know not to ever cross. In fact, it would be advisable to stay well clear of the hazy gray zone associated with this line. This bottom line would reflect a sense of epistemic responsibility of a scientist in academia towards herself and all other stakeholders. This sense of epistemic responsibility must be strong enough to forbid any attempt to publish data/results that are inauthentic or otherwise falsified, for doing so would be crossing a point of no return which is not only morally impermissible but the products or consequences of which would also come back to bite. We are not short of examples of stellar careers thus tarnished, disrupted, or destroyed. I shall return to this point on epistemic responsibility later.

3. Dang and Bright's Take on Publishing Scientific Conclusions That "Need Not Be Factually Accurate, Appropriately Justified, or Believed"

Dang and Bright considered three classes of norms of assertion (factive, justification and belief norms), particularly in relevance to what the authors termed the "public avowals of science". The latter would include "... conclusions in papers in peer-reviewed journals, conference presentations, posters, online pre-prints, etc.", but not extra-scientific testimony or "public scientific testimony" (such as the IPCC reports) aimed primarily at policy makers [18]. The authors argued that public scientific avowals should not be held to any of the three classes of norms. In citing a hypothetical example, the authors illustrated that despite failing these norms, such assertions can still contribute to the epistemic success of science. They referred specifically to the case of William Henry Bragg (1852–1942; 1915 Nobel laureate in Physics)'s neutral material particle theory of γ and X-rays, which was presented and defended in a series of papers from 1907 to 1912. Dang and Bright recounted that, on balance, Bragg's theory was neither factually supported nor backed up by contemporary evidence, and furthermore Bragg himself did not even quite believe in his own theory. The authors thus concluded that "... scientific public avowals frequently do not and need not satisfy those norms of assertion that have been discussed in the analytic epistemology literature", and that "Public avowals in science ought to be governed by a different norm" [18] (p. 8200).

I shall indulge in what I perceive as two possible weaknesses of Dang and Bright's arguments. The first being the nature of their cited case study of Bragg, which makes such cases exceptions rather than the norm. Bragg was, by and large, postulating yet unproven hypotheses in his attempts to formulate a theory on the nature of γ and X-rays, rather than publishing data/results that might be in support of a hypothesis, or falsifying one. The proposals of his hypotheses and the ensuing debates and discussions could all appear as peer reviewed publications, and this type of exposition of scientific ideas is more common in certain areas of science (such as theoretical physics) than others. When dealing with a hypothesis, it would be intuitively clear that its propositions might neither be factual nor yet fully justified. In accordance with Popperian falsification [23], a hypothesis is falsifiable if it can be logically contradicted by an empirical test. Hypotheses might eventually culminate into a more robust scientific theory that could account for observational and experimental results and could be used to infer further or other effects, which can then be verified (or disproved) by further empirical evidence. Until then, there should be no reason to "believe" in a hypothesis, or otherwise. In other words, an exposition of hypotheses might indeed not satisfy any of the three norms of assertion. It should also be noted that hypothesis expositions do usually come with a default sense and label which indicate that these are inconclusive in nature. There might be caveats, as well as boundary conditions, beyond which the hypothesis could not be relied upon to explain observations and results. This inconclusiveness indeed awaits further confirmation or refutation by more empirical data.

The above consideration cannot, however, be equally apply to publications whose contents are primarily empirical data/results. It would not make much sense if these were not factual, or for conclusions inferred from the data/results contained within to be neither self-evident nor interpretationally justified. It would also be somewhat illogical if the scientists who published these data/results did not believe in the authenticity of their own work. Unless, of course, we are actually looking at instances of fraud or misconduct.

I will illustrate my points above with a (hypothetical) example. Scientists (S) 1 and 2 have published, respectively, two papers (P1 and P2) with the following related punchlines:

> "It is hypothesized that a composite compound X could become a superconductor at room temperature when doped with hydrogen", P1

> "Evidence is provided for the composite compound X exhibiting superconductivity at room temperature when X is doped with hydrogen", P2

S1 explicated a hypothesis in P1, and it is clear that this is neither yet a fact nor has extensive empirical justifications. The hypothesis might be based on theoretical calculations, but detailed structural insight is still lacking, and the room-temperature superconductivity of X has yet to be experimentally verified. S1 explicates and publishes P1 as a scientific idea open to debate, and she need not believe in P1.

P2, on the other hand, primarily contains a set of data and results that factually attest to the claim that hydrogen-doped X is a room-temperature superconductor. The data and results should be authentic, reliable, and able to be replicated, thus justifying the above claim. S2 published these data/results, making a claim for an important finding. As such, she must believe in the validity and authenticity of these data/results. In fact, such justifications and beliefs are crucial for the research to be useful and for others to follow and to make further advances. This would be the epistemic responsibility the scientist must fulfill in publishing her results.

Dang and Bright's arguments might thus reasonably apply to hypotheses, but perhaps not so much to empirical results/data. In suggesting that lax norms in asserting science could be beneficial, and that "... fail norms we hold assertions to can still be important to the epistemic success of science" [18] (p. 8192), the authors' position attested to examples in history in which scientific breakthroughs arose and important ideas were consolidated through serendipitous, bold, and at times chaotic scientific activities. Afterall, Bragg did eventually win a Nobel prize for his work on X-ray crystallography, and the discovery of X-ray, with all of the theoretical and experimental work associated with it, constitutes a monumental advance in physics. In this regard, however, the history of science has also recorded a related counterexample, in which a much-heralded finding had turned out to be farcically false.

It was the discovery of X-rays (as well as the nature of radioactivity and that of cathode rays or electrons) that had led to speculations that there must be other forms of yet-to-be-discovered radiation. N-ray was first described by René Blondlot, an accomplished experimental physicist at the University of Nancy [24]. Over a period of several years, many of Blondlot's contemporaries published hundreds of articles claiming the detection of N-rays (together with a small number who failed to do so) and their various properties (including their emission by living organisms and transmission over a physical medium such as wires). Frustrated with his inability to detect the new radiation, Robert Wood traveled to Blondlot's laboratory and found that the N-rays could still be detected by those demonstrating despite Wood having stealthily removed key experimental components or changed samples. Wood wrote in his report to *Nature* that he was "... left with a very firm conviction that the few experimenters who have obtained positive results have been in some way deluded" [25]. Blondlot had apparently continued to believe in the existence of N-rays although he might have been misled by a trusted technician [26]. However, the same might not have been the case for the others who had jumped on the bandwagon and claimed to have detected their existence.

It should also be noted that, even in the utterance of hypotheses and theories, it is important that these are explicitly qualified as such, and the associated inconclusiveness or reservations should also be spelled out clearly. In other words, the hypothetical nature of any assertions or avowals should be clearly labeled, so as to be in line with the proper assumption of epistemic responsibility.

It could of course be argued that, in the real world, there are many past and present instances or examples in which public avowals or assertions in scientific publications were made with poor adherence to the epistemic norms of assertion. While the cases of X- and N-rays are both historical, more contemporary examples can also be readily identified. Several such examples arose during the peak of the ongoing COVID-19 pandemic, when bold claims of efficacy and benefits against the disease based on scarce or unreliable data were made by researchers on drugs such as hydroxyquinoline [3] and ivermectin [4]. Another set of contemporary examples would be the much-hyped announcements of several unsubstantiated claims of new materials (such as LK-99 and N-doped Luthetium hydride) that exhibit room-temperature superconductivity [6,27,28]. Dang and Bright's argument that these assertions need not be factually accurate, appropriately justified, or believed by the authors would either suggest that these are simply normative acts, or an attempt to make acts like such a norm in the scientific realm. However, there is of course a difference between what "is/was" and what "ought to be". Therein lies the second weakness of the authors' arguments—a naturalistic fallacy of some sort. Even if there exist a number of scientists who make assertions with lax epistemic norms, this alone does not make such acts right or acceptable. I would argue that such acts are epistemically irresponsible, and in betrayal of the epistemic trust [29,30] placed upon scientists by their colleagues and the public.

4. Epistemic Trust and Epistemic Responsibility

Epistemic trust in science and scientists stems from an epistemic reliance on these as knowledge and information providers. Many of the scientific findings and advances could only be made by scientists with specific skills, expertise, and training. However, a reliance on what could be provided alone does not necessarily lead to trust. In his analysis of epistemic trust in science, Torsten Wilholt concluded that "... trusting someone in her capacity as an information provider also involves a reliance on her having the right attitude towards the possible consequences of her epistemic work" [31]. Gürol Irzik and Faik Kurtulmus expounded on four necessary conditions for epistemic public trust in science. Accordingly, with M being a member of the public investing warranted trust in scientists (S) as providers of information, which is a proposition (P):

> "M has warranted epistemic trust in S as a provider of P only if (1) S believes that P and honestly (that is, truthfully, accurately, and wholly) communicates it to M either directly or indirectly, (2) M takes the fact that S believes and has communicated that P to be a (strong but defeasible) reason to believe that P, (3) P is the output of reliable scientific research carried out by S, and (4) M relies on S because she has good reasons to believe that P is the output of such research and that S has communicated P honestly" [30].

Therefore, for a scientist to be trustworthy, her scientific output (data/results) should be a product of reliable scientific research, which she must herself believe in and communicate honestly, with her assuming full responsibility for the output.

Trust by the public begets trustworthiness [32] on the part of scientists. Given its achievements and influence in the advancement of human technological civilization, there can be little doubt that science is a privileged profession [33]. As professionals, scientists should thus act with a keen sense of responsibility [34]. These would include a broad brush of social/civic responsibilities [35–37], but what is of fundamental importance is one that is intrinsic to the process of scientific research itself, or a commitment to performing good science [38]. In knowledge acquisition, there is an apparent responsibility to not just know, but also to know well enough. Epistemologist Lorraine Code had, for example, championed a "responsibilist" approach and qualifies the philosophical view of epistemic responsibility as an obligation to "know well" [19]. This notion would be aptly applicable to science and scientists. When scientists make claims, they have a burden of proof to justify and substantiate these claims [39].

To be epistemically responsible thus entails both knowing what needs to be carried out to be both technically adequate and deontologically apt. The technicalities of how a commitment towards epistemic responsibility in science should be fulfilled have been elaborated in various textbooks and guidelines on responsible conduct in research [40,41], as well as various research integrity statements, principles [5], and codes [6]. In my view, such a sense of fundamental commitment towards epistemic responsibility should ideally be deeply ingrained in all scientists. In other words, being a scientist means one must strive, first and foremost, to fulfill this responsibility, i.e., refraining from publishing data/results that are false or misleading, clearly stating propositions that are hypothetical in nature (together with their caveats and reservations), and proactively aiding or facilitating replication or confirmation of the results by others. This sense of responsibility should triumph over any hyper-ambition [17] to excel or be successful and should be steadfastly held despite various uncertainties [16]. In other words, scientists should and could still commit towards being epistemically responsible in carrying out and publishing their works, regardless of any intrinsic obsession with success or extrinsic challenges against their wishes to be successful.

5. Caveats, Objections and Rejoinders

It should be qualified here that I am not exactly arguing for or advocating the value-free ideal in science, a notion that might have been considered contemporaneously impractical or even obsolete [42,43], as non-epistemic values do appear to influence scientists in all stages of research. However, it has been proposed that forsaking the value-free ideal completely would be undesirable [44–47]. Menon and Stegenga thought that "scientists should act as if science should be value-free, . . . even if a purely value-free science is undesirable, this value-free ideal is desirable to pursue" [46]. Sikorski, on the other hand, argued that ". . . acceptance of non-epistemic values . . . necessitates legitimizing certain problematic scientific practices", including QRPs that would cause irreproducibility [47]. Betz has argued that value-laden decisions in science can be systematically avoided by making uncertainties explicit and carefully articulating findings [44], while Djørup and colleagues also think that "scientists can reduce the influence from non-epistemic values, inter alia, by employing debiasing techniques and other scientific procedures . . ." [45]. Thus, a scientist's fundamental commitment towards their epistemic responsibility should be in place regardless of the nature of values considered. A recent survey conducted by Ambrosj and colleagues with 24 holders of grants from the European Research Council has indeed shown that ". . . while interviewees recognized that science is not completely value-free (awareness), they still seemed to hold on to the so-called value-free ideal of science as a professional norm to minimize bias" [48].

This notion of epistemic responsibility overlaps with those expounded by others in discussing intellectual or epistemic virtues and vices in science and academia [49–52]. Being epistemically responsible in science would logically be considered an epistemic virtue. However, whereas all epistemic virtues are good to have and would contribute towards performing good science, epistemic responsibility demarcates the baseline professional behavior of an academic scientist. Crossing this line would thus signify an irreconcilable departure from a fundamental norm, or a violation of a moral axiom, in the practice of science.

Two likely criticisms against the notion of epistemic responsibility commitment outlined above shall now be considered. First, is a steadfast commitment towards epistemic responsibility by scientists actually good for science? Might it not result, perhaps, in the worst-case scenario, an ultra-reserved state of affairs where scientists are fearful of making any assertions in case of a backlash of being labeled as irresponsible? Would this not then stifle breakthroughs in research or reduce the number of scientific findings of a disruptive, game-changing, or paradigm-shifting nature? Science is intrinsically fallible, and any science is always a work in progress. Thus, despite the upmost caution and the best of intentions, errors and mistakes might be made along the way, but would these instances be confused with lapses in the commitment to epistemic responsibility?

I would think that the above concern is largely unfounded. Science cannot and should not be regarded as a deliverable-based vocation, but rather an adventure in discovery. As such, scientists should always have the urge to explore the unknown and thrive on opportunities to overcome obstacles and challenges, not to find and tread along the paths of least resistance to reach publishing goals. Mistakes are always allowed in the reporting of science provided that these are honest mistakes made with no intent to disguise or deceive.

Secondly, given the apparent phenomenon that success is often tied to ambition in academia, as well rampant QRPs and rising cases of RM, would a call for a commitment towards epistemic responsibility not simply be empty idealism? I would think not, as there are indeed ways of pre-empting and overcoming lapses in, or tendencies against, such commitments. Educating the young or next generation of scientists with the right approach and attitude towards doing science would be important [53–56], and faculty mentorship by senior members with the right experience, mentality and outlook in scientific research would also be helpful. Erden's suggestion of robust administrative measures that reward a broad range of academic values and undertakings but thwart any advantage of being hyper-ambitious [17] would be a good starting point for formulating and crafting policies of this nature. Meanwhile, all educational courses on research ethics and integrity should make it a point to emphasize a scientist's commitment towards epistemic responsibility. The practice of open science [7] will also increase transparency and visibility of the scientific process to all and potentially enhance a commitment towards epistemic responsibility.

6. Epilogue

The Sections above elaborated on the importance of a fundamental commitment towards epistemic responsibility from scientists, and for this responsibility to be fulfilled regardless of one's personal desire/ambition and however stressful the research environment may be. In a world challenged by social–political upheavals, pressing environmental issues, and other uncertainties, Laurie Zoloth has called for researchers to cultivate the classic values of veracity, courage, humility, and fidelity in their research [57]. A commitment towards fulfilling epistemic responsibilities would be a fundamental ethical attribute in the practice of science and is of critical importance for science to remain a beacon of sensibility and knowledge in our uncertain, fast-changing world.

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Notes

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