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ARTICLE

To Be Scientific Is To Be Communist

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ABSTRACT

What differentiates scientific research from non-scientific inquiry? Philosophers addressing this question have typically been inspired by the exalted social place and intellectual achievements of science. They have hence tended to point to some epistemic virtue or methodological feature of science that sets it apart. Our discussion on the other hand is motivated by the case of commercial research, which we argue is distinct from (and often epistemically inferior to) academic research. We consider a deflationary view in which science refers to whatever is regarded as epistemically successful, but find that this does not leave room for the important notion of scientific error and fails to capture distinctive social elements of science. This leads us to the view that a demarcation criterion should be a widely upheld social norm without immediate epistemic connotations. Our tentative answer is the communist norm, which calls on scientists to share their work widely for public scrutiny and evaluation.

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1. Introduction

An academic researcher who sets a threshold for statistical significance on the basis of a careful balancing of the epistemic and social risks that could result from erroneously rejecting or failing to reject her null hypothesis seems to be doing something right, whereas a commercial researcher who sets a similar threshold based on whether rejecting or failing to reject the hypothesis would serve her company's interests better is doing something wrong. One of them, one is tempted to say, is engaging in properly scientific inquiry, whereas the other is simply mimicking such a practice for rhetorical effect. Wherein lies the difference?

When thinking about what, if anything, makes science distinctive as a form of inquiry it is tempting to look for an answer in its epistemic norms and practices. After all, it is the (perceived) epistemic success of science that is meant to be honored or protected by demarcating it. We think this is a mistake, as we will argue in sections 3 and 4. Briefly, our view is that while the epistemic success of science as a social enterprise is indeed worth honoring and protecting, individual contributions to science need not be particularly meritorious from an epistemic perspective. Therefore, if one is trying to make judgments at the level of individual contributions, any demarcation criterion closely tied to epistemic success is going to judge too many cases incorrectly. We look instead for a non-epistemic demarcation criterion, and we tentatively settle on the communist norm, which focuses attention on the way scientists share their work and allow it to be scrutinized and evaluated by others.

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Boundary problems rarely arise in isolation. Typically, the goal of demarcation (including in philosophy of science) is to honor or protect something perceived to be valuable, or to exclude or marginalize something perceived as bad (for a recent example, see Larsen et al. 2020). So it is here. As already alluded to, we think something problematic is going on when research is driven by commercial interests. To put our cards on the table up front, our main motivation in this paper is to highlight problems with commercial research. There is abundant evidence, reviewed below, that the incentives for commercial research produce a mode of inquiry that, while sharing many features of scientific research, is very different in its ethos, practices, and outcomes. Moreover, there is evidence that working scientists are aware of this gap and concerned about its consequences. We thus wish to draw attention to the important differences between commercial research and other forms of inquiry. We attempt to achieve this by drawing a line of demarcation that puts most academic research on the side of science and much commercial research on the other side. Thus, our aim is not to give a once-and-for-all answer to the demarcation problem, but rather to suggest a possible answer and argue for its usefulness for specific goals in specific contexts.

Section 2 provides some background on commercial research as well as philosophical perspectives and criticism of it. This motivates our return to the demarcation problem in section 3, where we review and critique attempts to use epistemic success as a demarcation criterion. In section 4 we establish, based on the preceding discussion, what we want out of a demarcation criterion, and suggest that the communist norm meets the desiderata. We conclude in section 5 with some theoretical and practical upshots.

2. Commercial Research and its Problems

Consider the research done in-house by pharmaceutical companies ('Big Pharma') and other commercial entities. The research done at such companies is (almost by definition) intended to be relevant to the company's operations. This means that conflicts of interest are ubiquitous. Pharmaceutical companies want the drugs they develop to be found effective and safe, so they can sell them for a profit. Tobacco companies hope for research findings that minimize the negative health effects of smoking. Construction companies prefer not to find habitats of endangered species that would delay or prevent their projects from getting approval. And so on.

More generally, because of the different context and goals, commercial research is subject to different norms than academic research. Commercial researchers may choose not to share the results of their work with researchers outside the company for a variety of reasons: to preserve an advantage in a race to develop a new product, to keep secret a production process, or to prevent information that would put the company or its product in a bad light from coming out (DeAngelis 2003). Even where there is some business incentive to engage in more open research practices, entrenched cultural practices and business models can still lead pharmaceutical companies to engage in closed, secretive, research practices (Nakagaki, Aber, and Fetterhoff 2012). In fact, commercial researchers have been known to actively work to prevent publicly funded research being made available to all (Hilgartner 2012; Heller and Eisenberg 1998, 700).

In contrast, academic researchers have strong incentives to share their results widely even in very competitive environments. Commercial researchers may see themselves as involved in a zero-sum game with competitors, whereas academic researchers are always collaborating (on the sum total of human knowledge) at the same time as they may be competing (to contribute particular bits to that sum), as Hull (1988) highlighted. Commercial researchers may explicitly and self-consciously work towards a desired finding or goal (McGarity and Wagner 2008; Oreskes and Conway 2010). Thus they may abandon lines of research that appear to yield undesirable results and have no reason to avoid practices such as *p*-hacking and data dredging, which are frowned upon in academic research (Simmons, Nelson, and Simonsohn 2011).

To be clear, we do not intend to suggest that the incentives for academic researchers are beyond reproach. The pressure to publish and establish a name for oneself can make *p*-hacking, data

dredging, and other questionable research practices attractive to academic researchers as well (Bruner 2013; Bright 2017; Heesen 2018, forthcoming). In distinguishing commercial and academic research we instead make the following two more narrow claims. First, the incentive to share results widely is an essential cornerstone of academic research (Strevens 2017; Heesen 2017), whereas the incentive to share is at best incidental to commercial research. Second, as we will now emphasize with examples, the goals, context, and norms of commercial research lead to behavior that is plainly misleading and counterproductive when viewed from the perspective of the lofty epistemic goals of science (Becker-Brüser 2010; Fernández Pinto 2020). While some of these behaviors occur in academic research as well, the evidence we will now review suggests that they are much more frequent and more serious in their consequences in the commercial context.

By selectively sharing research results, companies may sow doubt or misleadingly suggest a lack of consensus about findings they know to be well-established (e.g. the health impact of smoking and sugar or the effects of fossil fuel use on the climate). Selective sharing biases the research record, with potentially serious negative consequences for meta-analysis, public opinion, and public policy (McGarity and Wagner 2008; Doucet and Sismondo 2008; Elliott 2016; Holman and Elliott 2018; Weatherall, O'Connor, and Bruner 2020). Beyond selective sharing, companies may use a range of other methods to manipulate the (perceived) research record in light of their commercial interests, including data fabrication or falsification (Doucet and Sismondo 2008; Holman and Elliott 2018), manipulating research designs to engineer favorable outcomes (Lexchin et al. 2003; McGarity and Wagner 2008; Doucet and Sismondo 2008; Elliott and McKaughan 2009; Lundh et al. 2018; Fabbri et al. 2018), rhetorically spinning unfavorable outcomes into favorable ones (Doucet and Sismondo 2008; Elliott 2016), selectively funding external researchers (Elliott and McKaughan 2009; Elliott 2016; Holman and Bruner 2017; Fernández Pinto 2017; Holman and Elliott 2018), manipulating scientific concepts (e.g. disease classifications) (González-Moreno, Saborido, and Teira 2015; Elliott 2016), intimidation (McGarity and Wagner 2008; Elliott 2016; Fernández Pinto 2017), and lobbying (Christensen 2008; Elliott 2016; Fernández Pinto 2017). Especially in combination, these methods can and have been used to create confusion and stall public action on important issues for many years (Oreskes and Conway 2010).

This is more than a minor design flaw, and given the profits at stake along with industry's ability to effectively lobby and hire the best lawyers it is not something that can be addressed with a clever legal framework. Since companies' ultimate goals relate to their self-interest (usually understood in financial terms), whereas epistemic goals are instrumental to that, the epistemically counterproductive practices outlined above will routinely be perfectly rational from a commercial perspective. As long as it remains in their interest to do so, companies will continue to mislead, creating an epistemic arms race (Holman 2015; Holman and Geislar 2018; O'Connor and Weatherall 2019). For these reasons, it would be misleading to group together commercial and academic research under a single header ('science').

There are a number of ways one might distinguish commercial research from academic research. One is to identify individual practices and norms that differ, and criticize these where appropriate. There is a rapidly expanding body of research that does this (see Holman and Elliott [2018, section 3] for a bibliography). Another is to return to an old problem in the philosophy of science: the demarcation problem. Just as Popper used his answer to the demarcation problem to criticize Freudian psychoanalysis and astrological theories as pseudo-scientific, we may give an answer to the problem that lets us criticize commercial research as pseudo-scientific.

Our challenge, then, is to find an independently plausible answer to the demarcation problem that lets us do this. Note that this does not require the line between commercial and academic research to be fully strict and determinate. While we have presented the distinction as a strict binary for ease of presentation in this section, we are aware that research may be funded by public-private partnerships, that academic research may be commercialized, and so on. At the same time, the distinction between the products of commercial research labs and university labs that have half an

eye on future commercial applications should be relatively clear. Our discussion below is sensitive to the graded nature of this distinction.

3. Science as a Success Term

One view that has tempted some thinkers, usually in a deflationary mode, is to say that 'science' is a success term. The idea here is that what makes something scientific is just that it succeeds in meeting whatever epistemic criteria we think are pertinent to the present situation. We may go even further in an emotivist direction, and say that to call something 'scientific' is just a way of giving near contentless praise to it, a way of saying 'yay!' with a peculiarly epistemic mode. Susan Haack has expressed this idea as follows:

> [O]ften – perhaps more often than not — 'science' and its cognates are used honorifically: advertisers urge us get our clothes cleaner with new, scientific, Wizzo, teachers of critical thinking urge us to reason scientifically, to use the scientific method, expert witnesses are believed on the grounds what they offer is scientific evidence, astrology, water-divining, homeopathy or chiropractic or acupuncture are dismissed as pseudo-science, skeptical of this or that claim, we complain that it lacks a scientific explanation, or demand scientific proof. And so on. 'Scientific' has come to be an all-purpose term of epistemic praise, meaning 'strong, reliable, *good*'. (1999, 188)

Larry Laudan seems to endorse the view when he writes:

> If we would stand up and be counted on the side of reason, we ought to drop terms like 'pseudo-science' and 'unscientific' from our vocabulary; they are just hollow phrases which do only emotive work for us. (1983, 125)

Somewhat cheeky though such views are, what makes them tempting is a combination of two observations. First, the project of finding a clear methodological demarcation criterion has not seemed to bear much fruit. There are some who still believe it can be borne out (Pigliucci and Boudry 2013), but for the most part philosophers of science have abandoned the project, believing it will not be fruitful (Laudan 1983). Second, there is a sense that to be scientific is to be especially good or trustworthy that seems to drive many everyday uses of 'scientific' as an adjective. It seems to indicate meeting some standard that the speaker believes ought to have been met. The science-as-a-success-term analysis is motivated by these two points and captures them by directly appealing to the second observation, while explaining the first by saying that the positive connotations of 'scientific' are not tied to any particular methodological or epistemic achievement or standard, only the general point that something epistemically good has been achieved.

Despite these virtues, such views miss out on core features of scientific inquiry. For one, they miss out on the necessity of failure, mediocrity, and half-baked guesses, effectively making 'scientific error' an oxymoron. Popper said that science proceeds by bold conjecture and refutation. One does not have to buy into Popper's full theory of scientific method to see the point that scientific error is important, and that we should avoid excluding all epistemically bad work from the remit of the scientific.

A few quick examples illustrate this point. We certainly would not want to call, e.g. Newtonian physics or phlogiston theories unscientific merely because more epistemically successful theories exist. One might object that these theories were 'successful' in some to-be-specified sense at the time they were proposed, but the next example shows the point generalizes.

Consider 'null results', i.e. scientific studies or experiments that fail to find (strong) evidence for some theory or hypothesis of interest. In contemporary science, this typically occurs as a result of a failure to reject a relevant null hypothesis on the basis of the data collected. Null results are notoriously difficult to publish, though there has recently been a push to reverse this (Ioannidis 2006). While there has been discussion over the relative value of publishing null results, to our knowledge no one has claimed that null results are unscientific in virtue of failing to find evidence for the guiding theory or hypothesis.

We think this is more than an edge case. It is simply essential for science as we presently practice it that we allow for a lot of work that does not succeed, indeed that we judge (now or later) to be

poorly conceived and improperly carried out. It has even been argued that the norms governing publication of individual papers have evolved specifically to permit this sort of epistemic failure (Dang and Bright 2021, section 5). Any demarcation criterion that rules a study unscientific because it fails is not fit for purpose. In this way, the science-as-a-success-term analysis counts in too little.

Perhaps one could finesse the relevant views so that refuted scientific theories or studies that fail to find evidence for a proposed theory could still be counted as successful in some sense. A distinction between epistemically bad practices and virtuous alternatives that happen to generate falsities might be helpful here. However, it is not obvious how to proceed while remaining within the category of answers to the demarcation problem (traditional or new) that aim to describe a near contentless form of epistemic praise.

At any rate, we think there is another problem: contentless epistemic praise views count in too much. People are epistemically successful all the time in all kinds of contexts that we would not normally describe as scientific. If I form basic perceptual beliefs, e.g. about the color of the objects in this room, I will typically achieve quite a high rate of epistemic success. This is true even relative to high standards regarding the amount of luck that is involved, the degree of certainty I achieve, etc. (as in the more demanding philosophical analyses of 'knowledge'). Still, it would be unusual at best to call this scientific knowledge (Pigliucci 2021, 206] makes a similar point in a different context).

Finally, the science-as-a-success-term analysis fails at the pragmatic purpose of demarcation. As noted in the introduction, the hope is to use a demarcation criterion as a means of critiquing work which fails to meet the standard. The description 'things which are not epistemically good' is too thin for this purpose: any attempt to critique work as non-scientific is just going to be a restatement of the first-order dispute. Freudian psychologists (Popper's favorite target) are given no reason to agree that their work is not scientific if one means by this simply that it is not good – they think it is good, and they may have theories as to why you think otherwise.

Nonetheless, we should take on board the lessons of the two observations that motivated this line of thinking about demarcation problems. First, science is too varied and many-faceted to be exhaustively characterized by any peculiar or distinctive methodological or epistemic achievement. Thus, an answer to the demarcation problem cannot only rely on the specifics of methodology. Second, we do often use 'scientific' or describing something as a 'science' as a means of indicating some sort of normative success. The question remains how one can square these without the errors just identified.

4. The Significance of Communism

Being scientific or being part of science means something like successfully participating in the socio-cultural activity of scientific inquiry. The appearance of circularity here is because at root there is an ostensive element to this. To get started, we must point to a set of paradigm institutionalized activities and say 'those are the scientific ones'. Others then count in just in case they are taking part in the same broad cultural activity as the initial ostensively picked out activities.

Ideally, of course, scientists would be epistemically successful. That is, not only would they participate in the socio-cultural activity, but by so doing they would help advance human knowledge. But, as we have argued, that cannot be a requirement for counting as scientific. We therefore explore the possibility that there are social norms of a non-epistemic sort that scientists may successfully uphold and that unify to some extent the pertinent community.

What do we mean by norms of a non-epistemic sort? Here we have in mind behaviors that a relevant community does or might perceive as normative (for a general definition of social norms, see Bicchieri 2006), but that do not thereby guarantee the achievement of knowledge, or true belief, or the avoidance of error (in other words, norms that do not directly exemplify one or more epistemic virtues, in the sense of Montmarquet 1987; or Turri, Alfano, and Greco 2019). For instance, the norm of shaking hands upon first meeting someone is such a non-epistemic norm, since presumably our cognitive achievements are unaffected by this.

A good starting point in thinking about scientific social norms is the set of Mertonian CUDOS norms: communism, universalism, disinterestedness, and organized skepticism (Merton 1942). Why are these specific norms a good starting point? First, because they have been well studied and scientists still indicate high levels of allegiance to them. According to a survey by Anderson et al. (2010), when asked about their normative force (i.e. whether scientists should conform to them), each of the CUDOS norms is supported by over 75% of scientists. Norms expressing opposing principles ('counternorms') receive no more than about 25% support (Anderson et al. 2010, figure 1). In other surveys, expressed support for the communist norm in particular ranges up to 95% (Louis, Jones, and Campbell 2002; Macfarlane and Cheng 2008). Moreover, a recent survey of 633 South Korean scientists found not only that communism was the Mertonian norm they thought most respected in practice (Kim and Kim 2018, 9), but also that younger scientists' replies showed evidence of 'mounting tension between the commercialisation of academic science and the norm of communalism' (Kim and Kim 2018, 19). Our focus on scientific communism and its relationship to commercial research may thus reflect the concerns of working scientists.

A second reason for taking the CUDOS norms as a starting point is that they are relatively field-independent. Other purported social norms in science (e.g. do not claim to have discovered something unless you get data allowing you to reject a relevant null hypothesis at a specific significance level, see Benjamin et al. [2018] and Malinsky [2015, 927]) are often quite local. These do not mark out science, but rather the norms of some particular field. The CUDOS norms have a much better claim on being norms of science as such.

What is the content of these norms? Communism says that one must make one's work available to others for free, not try to maintain proprietary rights to it, and treat it as always properly open to the evaluation of the scientific community. Universalism enjoins one to evaluate ideas independently of one's opinion of the person who put it forward. Disinterestedness requires one to evaluate ideas based on evidential or cognitive considerations, rather than based on one's preferences or interests. Finally, according to organized skepticism, one must systematically test claims and remain open to the possibility of their future falsification. This includes never simply taking things on faith or as beyond the pale of dispute.

We now submit that out of these four social norms, the communist norm in particular is a good place to start in deciding whether or not a claim is scientific. Why do we single out communism, rather than taking one of the other three norms as our putative demarcation criterion, or, say, the combination of all four CUDOS norms? Firstly, and most importantly, the communist norm lends itself quite well to the particular goal we have set for ourselves in pursuing a demarcation criterion: critiquing commercial research. We have argued above that it is both normal and appropriate to use demarcation to critique particular practices, and we will argue below that communism lets us do this.

We do not claim that the other CUDOS norms, or a combination of them, could not be effectively used as demarcation criteria in pursuit of these or other goals. Moreover, there may well be other norms than the CUDOS norms which apply to all of science and which may be fruitfully used in contexts of demarcation. For instance, emerging norms around participating in 'certified amplification' processes could perhaps do similar work (Lee forthcoming). But as we find that we can achieve our goals using only communism, we keep things simple by restricting our attention to it.

The second reason for singling out communism relates to our observation at the start of this section that science is a shared socio-cultural activity. Anyone who is not making their work available to the broader scientific community is not taking part in science as a shared cultural activity. And anyone who does make their work available and has at least some minimal pretense to taking the community's evaluation seriously is taking part in a shared cultural activity. Using the communist norm as a demarcation criterion of scientific activity straightforwardly makes sense of these observations. The other three CUDOS norms do not in the same way help us identify the relevant community.

So the view we will explore is that the communist norm may be used to decide whether a given claim is scientific. It is scientific, we say, if it is made appropriately available to the scientific community and proprietary rights are not claimed in any way that interferes with fellow researchers

accessing, using, or evaluating it. Acknowledging that this is not a strict binary, we can say that a claim is more scientific the more it lives up to this norm. We do not claim that the above quick and dirty process of elimination constitutes much of an argument in favor of our view. Instead, the proof is in the pudding: how does the communist norm fare as a demarcation criterion and how does it align with the goals we have set up in the foregoing?

This view gets some points of demarcation right that seem relatively uncontroversial. Alchemists maintaining secret journals and refusing to share esoteric arcane knowledge are not typically felt to be part of a scientific community. Indeed, it was considered an important cultural battle by people we now think of as leaders of the scientific revolution that this practice of secrecy be overcome (Principe 1992; Macrakis 2010). Or consider cranks, who are all too keen on sharing their work, but make it clear by their indifference to critique and counterargument that they do not think the community has any right to evaluate their achievements. Our view correctly assesses alchemists and cranks as engaged in activities that in some ways resemble scientific research but fall short of full participation.

Another feature of our view, which we take to be a virtue, is that it is inclusive where it comes to academic disciplines. Insofar as they conform to the communist norm, the social sciences, the arts, and the humanities all count as science. On this point we side with the broad German conception of the Wissenschaften over the more narrow English sciences. We exclude only those parts of the arts and humanities with a penchant for secrecy or esotericism.

We now return to our primary case study: pharmaceutical research and commercial research more generally. Through the use of patent law and the frequent refusal to share work in publicly accessible journals (especially if the results do not favor commercial interests, see Oreskes and Conway 2010; Weatherall, O'Connor, and Bruner 2020), such research fails to be scientific by our lights. Evidently it has some of the trappings of science and intermingles with the scientific community proper. But the profit motive, considered as an overarching value pursued in commercial research, turns out to encourage secrecy and withholding of scientific information. Insofar as commercial research is not fully and openly shared for the scrutiny of the community, our view does not count it as scientific as it fails to live up to the communist norm. And this is a good thing, as we have argued in section 2 that grouping together commercial and academic research is misleading, effectively allowing industry-funded research to steal from the exalted epistemic reputation enjoyed by science properly so called.

Using the communist norm as a demarcation criterion thus succeeds for the particular purpose we identified in section 2. It also fits nicely with a view about scientific progress recently defended by Dellsén (2021). Given that this was our primary goal in this paper, we leave the discussion here. We have not given anything like a knockdown argument establishing the communist norm as the uniquely correct answer to the demarcation question. However, if we take science to be characterized by a social norm binding together the various activities taking place under its banner, and we further require that this norm does not denote or imply some form of methodological or epistemic success, then we think the communist norm is a plausible candidate.

5. Conclusion

For research to be scientific is indeed for it to succeed in living up to an important norm. But, contrary to previous accounts, that is not an epistemic norm. Rather, scientific research is that which successfully manages to be communist. By this we mean that it makes itself available as far as possible to the perusal, scrutiny, and critique of the broader scientific community.

We have argued for this on the grounds that it accords with the sense that 'scientific' is a success term, it broadly coheres with our judgments about what should count as scientific, and by noting that the communist norm is suitably general to play the demarcation role since it is a shared normative commitment across many scientific fields. This means that scientists are expected to conform to it, though it certainly does not mean that every scientist always does. But we have not claimed that everything which might be thought of as scientific research should count in. We have highlighted especially that commercial research, such as that carried out by pharmaceutical companies, does not

tend to regard the communist norm as a constraint, in fact it frequently and flagrantly violates it. We take it to be a further virtue of our view that it regards such research as not properly scientific.

We stress that we come to praise science, not to deflate it. Being scientific, which is to say participating in this community as a good communist, is a worthy activity. For no particular piece of work does the mere act of sharing it guarantee any good epistemic outcome, but by each pitching in and contributing our part we are helping advance a collective project which has the potential to increase our knowledge and better our estate. We do, therefore, think there is a good sense in which we might want research to aspire to being scientific in this way, even if it yields no immediate epistemic warrant.

We draw two practical conclusions from this. The first concerns the private commercial research which we have ruled unscientific. In our view, such research should be made to be more scientific by becoming more communist. Some might worry that, since companies engage in this secretive research for good commercial reasons, if we forced them to share we might get less of it (common wisdom being that allowing companies to profit from their inventions spurs innovation, though see Heller and Eisenberg 1998 for a dissenting view). However, this objection does not concern us so much: one could always nationalize the research and the companies producing it, changing their incentives entirely. This may well have additional benefits, including removing some of the perverse incentives and opportunities for corruption identified in Oreskes and Conway (2010) and Weatherall, O'Connor, and Bruner (2020).

Second, our argument gives support and impetus to the Open Science movement. By making science cheaply and widely available, by facilitating more openness about what is being done and why, and by making it possible for a greater variety of people to participate, Open Science initiatives embody the definitive feature of science as such. Open Science is a step towards the better realization of communism within scientific communities, which is just to say it represents science achieving its highest ideal.

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