Can economic approaches to science do away with epistemic virtue?

Duygu Uygun Tunc & Duncan Pritchard

Abstract

Economic approaches to science underline the social structure of science as the chief explanatory factor in its collective epistemic success, and typically endorse a common conclusion, namely that individual virtue is neither necessary nor sufficient for science to be successful. We analyze a central example, the invisible hand argument, in reference to a case of collective epistemic failure, namely the credibility crisis. While divergent motivations might also serve the collective goals of science, our analysis shows that the presence of a significant proportion of virtuous scientists in a scientific community is a necessary condition for its success.

Introduction

Do scientists have to be epistemically virtuous in order for science to be successful?[1] This question as to the role of epistemic virtue in science can be embedded in a broader debate on how the goals of epistemically virtuous communities and epistemically virtuous individuals are related. We can identify two positions as marking the extreme ends of this debate. At one end we can place the view that individual epistemic virtue is necessary and sufficient for collective epistemic virtue. Call this position *virtue traditionalism*. At the other end, we can place the recently much more popular position that individual virtue is neither necessary nor sufficient for collective virtue, and that individual vice can possibly contribute to the collective success of science. Call this position *virtue radicalism*. This latter position has been defended by many in the philosophy of science to explain the success of science in terms of its social structure only; most notably through the argument from the invisible hand (Hull 1978; 1988; Kitcher 1993; Strevens 2011; Wray 2000). It maintains that the success of science is due to the incentive structures of its credit economy. Thanks to these economic structures in science, scientists can serve the collective epistemic good by pursuing non-epistemic goals like

---

[1] We take epistemic virtue to consist in a cognitive competence that is exercised with proper epistemic motivations and reliably produce good epistemic outputs such as truth or knowledge. By proper epistemic motivations we mean those that manifest an intrinsic or primary "concern with truth" (or other epistemic goods such as accuracy or validity).

career advancement, esteem or recognition instead of genuinely epistemic aims such as truth or knowledge.

While acknowledging (*contra* virtue traditionalism) that divergent motivations and behaviors might also serve the collective goals of science, in this paper we argue (*contra* virtue radicalism) that the presence of a significant proportion of epistemically virtuous scientists in a scientific community is a necessary condition for collective epistemic virtue. This is because the rules of credit allocation in science cannot be established, maintained, or reformed in a way that reliably serves collective epistemic ends of science without there being a significant proportion of epistemically virtuous scientists. Thus, top-down structural explanations are at best partial explanations. Individual epistemic virtue is a key variable that must be included in any systemic explanation of scientific success that manifests external validity, which must be relatively more complex and dynamic.

Argument from the invisible hand

What we call the economic argument for virtue-radicalism constitutes a bold attempt towards a "disenchantment" of science. Before roughly the second half of the 20th century, the commonplace conception of the ideal scientist was a champion of epistemic virtue. This picture was slowly replaced by a picture of the scientist as an entrepreneur. Scientists do not contribute to the advancement of scientific knowledge because they intrinsically endorse certain epistemic norms and values more than other people, but because doing so is in their personal practical interest. Strevens (2011) puts the motive for the disenchantment in the economic argument quite explicitly when he says: "It is hardly possible to believe any more, if it ever was, that scientists are made of […] better stuff than the rest of us…They are intellectual entrepreneurs."

We see one of the most common forms of the economic argument for virtue-radicalism in the philosophy of science in the various invisible hand arguments for the success of science (e.g., Hull 1978; 1988; 1997; Kitcher 1993; Strevens 2011; Wray 2000). The general structure of invisible hand arguments is that a decentralized social process results in an outcome that is not intended by the participating agents (Ylikoski 1995). Quoting from Wray (2000), an invisible hand argument describes a particular outcome "as an unintended consequence of the intentional behavior of a number of individuals. The individuals have one end in mind, and act accordingly; but their concerted efforts give rise to a consequence that was no part of their intentions." Applied to science, an invisible hand argument for the success of science purports that scientists primarily seek credit, not knowledge, and in doing so they advance scientific knowledge. In Hull's formulation, the credit economy of science is so organized that the collective outcome of each scientist just pursuing their individual interest is that the aims of science are served. "Scientists want credit and [...] science is so structured that this desire leads to increased knowledge of the empirical world" (Hull 1988, 357), so that "some of the behavior that appears to be most improper actually facilitates the manifest goals of science…those who make the greatest contributions just as frequently behave the most deplorably" (*ibid*., 32). Thus, the credit economy of science gives rise to collective virtue as if it is intentionally designed to promote the advancement of knowledge.

The social structure of science that creates this invisible hand mechanism is characterized as a "winner-take-all" credit market (Hull 1997), which is due to what is commonly called the *priority rule* in science (Merton 1957). Strevens (2003) describes the priority rule in terms of two main characteristics: "rewards to scientists are allocated solely on the basis of actual achievement, rather than […] on the basis of effort or talent invested" and "no discovery of a fact or a procedure but the first counts as an actual achievement."

Another characteristic of the social structure of science, and for many a result of the priority rule (Hull 1997; Heesen 2017), is the system of mutual citation. The scientific institution incentivizes originality, speed and impact, since credit goes only and completely to those who are the first to discover something, and to the extent that they can make others accept and use their research. Scientists want their research to be accepted in order to get credit for it, and they can maximize the changes of their being accepted by using well-established work of others, thus by citing them. The competition for credit is also the very mechanism by which science polices itself or corrects its own record. Having to use the findings of others also makes the scientists interested in their reliability, because it affects their prospects for credit. As a result, scientists are incentivized to be sensitive for any potential errors in others' research, thereby realizing scientific self-correction.

The crux of these arguments is that thanks to the social structure of science, in this case the particulars about the credit economy, epistemic virtue and selfish credit-seeking are behaviorally or consequentially indistinguishable. Let's call this to the *behavioral indistinguishability thesis*. The social structure of science is miraculously successful in producing collective epistemic goods in the same way as a truth-seeking motivation would do. This is clearly suggested by Hull (1997), who says "Scientists need not be saints to contribute to science…the really neat thing about the reward system in science is that it is so organized that, by and large, more self-serving motivations tend to have the same effect as more altruistic motivations."

The invisible hand argument rules out epistemic virtue as a viable explanation of why scientists do what they typically do. While it does not advance the empirical claim that scientists generally do not have epistemic motivations but only practical motivations, it does advance the theoretical claim that the seemingly virtuous practices of scientists can be explained with practical motivations only and thus any reference to epistemic motivations is an unnecessary commitment for an explanatory account. We think that scientists are truth-seekers by looking at their behavior, but the most parsimonious account of their behavior is that they pursue their own practical interests just as much as anybody else. Thus, the social structure of science and the practical motivations of scientists are sufficient to explain the success of science and individual epistemic virtue is redundant.

An anomaly for the model: the credibility crisis

The invisible hand mechanism is invoked as a causal model to explain the success of science. But a serious flaw in its justification is that its proponents only look at science at its most successful and abstract out some social interaction patterns to explain this success. A good test for the model would involve, however, also looking at less impressive cases to see whether they feature the same or different incentive structures.

The credibility crisis (Vazire 2019; IJzerman et al. 2020) and increased attention on the effect of the present incentive structures on research quality and reliability reveals a critical anomaly for the model. The same reward system not only fails to work as the proponents of the invisible hand describe, but also correlates with highly undesirable epistemic outcomes such as unreplicable findings, low methodological standards, publication bias, and consequently an unacceptably high rate of false or misleading scientific claims in the literature (Ionnadis 2005; Camerer et al. 2018; Open Science Collaboration 2015; Button et al 2013). The root causes of this credibility crisis have been widely traced to a range of established but faulty research practices (Bakker, Van Dijk & Wicherts 2012; Fraser et. al. 2018; John, Loewenstein & Prelec 2012; Simmons, Nelson & Simonsohn 2011). These 'questionable research practices' (QRPs), as they are commonly called, make up a wide spectrum, some clearly bordering on research misconduct. In general terms, QRPs refer to the use of wrong methods, wrong use of right methods, misinterpretation of results, selective reporting of results or studies, and drawing of unjustified conclusions (Altman 1994). The use of QRPs lead to seriously bad epistemic effects, because published results generate false impressions about the reliability and importance of empirical findings.

The invisible hand idea, or at least some of its major premises, consequently came under attack in the aftermath of the credibility crisis. In the meta-science literature there are several arguments underlining the damage the existing incentive structures do to science. The causes of the widespread use of QRPs have widely been traced back to the institutional incentives that favor them (Chambers 2017; Nosek et. al. 2012; Munafo et. al. 2017; Simmons, Nelson & Simonsohn 2011; Smaldino & McElreath 2016). According to these analyses, the institutional incentive structures are not well-aligned with the aims of science but actually undermine them. Competition for priority leads to unreliable research processes (Tiokhin et al. 2021a). Playing the credit-maximizing game strategically requires collecting publishable results most efficiently, which consists in doing research in a way that inflates effects and increases rates of false positives in the literature (Bakker et al. 2012). From a broader angle, valuation of high competition, emphasis on novelty, productivity and originality actually prevent the realization of science's aims because they make the proliferation of bad methodological practices inevitable. Smaldino and McEreath (2016) call this the "natural selection of bad science." Among philosophers, Heesen (2018) demonstrated through a rational choice model that the incentive structures of the credit economy of science praised by the proponents of the invisible hand idea makes low replicability of findings inevitable, because it "incentivizes scientists to focus on speed and impact" at the expense of replicability. Romero (2017) argues resonantly that the priority-based credit system of science creates a tension between the values of novelty and replicability. Bright (2021) adds that the credit incentive even abets and encourages fraud.

Moreover, the system of mutual citation does not seem to serve the self-correcting function that the invisible hand explanation takes it to do. Citation networks are shown to be able to create unjustified epistemic authority through distortions (Greenberg, 2009). Let alone being a more or less reliable indicator of scientific quality, citations do not seem to be correlate with scientific quality in any useful way. For instance, citations cannot be used to distinguish between studies that replicated and those that failed to replicate (Arslan & Eleftheriadou 2019; Schafmeister 2021), not even between articles that are retracted and those still in the literature (Candal-Pedreira et al., 2020). Moreover, retracted papers seem to perform better than others in science communication and dissemination (Peng et al. 2022). As the interesting case of a phantom reference shows, sometimes a high number citations do not even indicate that the article, the journal or the authors exist (Harzing & Kroonenberg 2017).

Epistemic virtue and credit-seeking are behaviorally distinct

Behavior that manifests epistemic virtue and behavior that is motivated by credit market incentives are indeed behaviorally or consequentially distinguishable under the same incentive structures. Arguably the reason why they seemed indistinguishable to the proponents of the invisible hand argument is their disregard for circumstantial variables due to their exclusive attention to the most successful examples of science.[2] A virtuous epistemic character would diverge from others regarding behavioral and epistemic outcomes under different conditions affecting the social-institutional context of research.

Let's imagine three kinds of scientist, which we can label the *lawful good*, the *chaotic good* and the *chaotic neutral*. The lawful good scientist might desire unveiling nature's secrets, advancing human knowledge, or developing the best models to represent the world. Or they can have a complex *credit-for-knowledge* motivation, where the credit-motive is the distal and the truth-seeking motive is the proximate cause of behavior. In any case, a credit-motivation is not part of the explanation of why they choose method X over method Y, how they interpret the implications of the results for the hypotheses under test, how they go about error-control and use researcher degrees of freedom (Simons, Nelson & Simonsohn 2011), why they decide to publish or file-drawer a study, how they assess own competence and methodological strategy, and so on. If all available methods lack sufficient reliability (e.g., construct validity or measurement invariance), they suspend judgment on certain research questions until further methodological advances are made or engage in the development of new ones. In short, the incentive structures do not explain their research choices, although they may still explain their choice of research topics or the decision to enter or leave the scientific profession.

For the chaotic good scientist truth is a significant concern but not the only or the primary concern. Their actions are motivated also, or ultimately, by prestige, reputation, academic promotion or some other such aim. Consequently, this scientist uses methods that are standard in that field, and tends to deal with ambiguities and degrees of freedom in the research process in way that increases publishability rather than rigor. They avoid publishing false or unjustified scientific claims, will not fake research, but they have a higher risk of self-serving bias in scientific reasoning. The context

---

[2] It is important to note here that the notion of a credit incentive involves that something else than knowledge or purely epistemic performance is rewarded, typically an outcome or product that may correlate (or not) with knowledge.

dependency of their scientific behavior comes to the fore in that their preferred methods will be reliable only to the extent that the publication standards in the field are reliable. Moreover, they are less inclined to do replications, search for error or other flaws in published research or do rigorous peer-review. They are less inclined to collaborate with others (as the potential credit is distributed) or invest in auxiliary or second-order competences, which are not often or directly rewarded (e.g., highly specialized skills, skills for error-control, knowledge of measurement theory, calibration know-how).

Lastly, the chaotic neutral scientist does not have any epistemic motivation, but only a simple *credit-for-credit* motivation. They pursue good epistemic ends only if doing so brings more credit. When there are low-hanging fruits (low-risk high-gain research) they pick them up, but they would be more reluctant to pursue conservative research strategies or to contribute to incrementally progressing fields or projects. They are not responsive to norms of scientific inquiry if these are inadequately enforced through scientific quality control mechanisms and sanctions. They respond only to a gain-loss calculus involving the prospects for credit, the costs of producing publishable and citable research outcomes, and the risks of not passing through scientific quality-control and gate-keeping. The chaotic neutral scientist has a much higher risk of engaging in QRPs or even outright fraud. If fraud becomes even slightly more profitable than conforming to the standards of research, such as when the prospects of getting flawed research published and potentially cited are reasonably high (due to scientific quality and error control mechanisms being ineffective), and the risks of sanctions for misbehavior are low (due to the vagueness of standards or difficulty of establishing misbehavior), the chaotic neutral scientist easily becomes a free-rider.

Contextual variables can distinguish the chaotic neutral from the chaotic good scientist whenever the scientific quality control mechanisms are inefficient. They can also distinguish the chaotic good and the lawful good scientist, who share a concern for truth. Different circumstances place different demands on the intellectual character of scientists regarding what it takes to contribute to the advancement of scientific knowledge reliably and responsibly. It is easier to be epistemically virtuous in some circumstances than others, and in some circumstances epistemic virtue requires extraordinary qualities. If a scientific field already has well-established and objectively reliable scientific standards, protocols and methods, has reached a certain theoretical maturity, is on a progressive track, and has plenty of resources, there will not be any significant difference in the epistemic quality of the inquiry procedures and outputs of the lawful good and chaotic good scientists. But if the prevailing norms and standards are weak, the dominant theory has started to accumulate anomalies, or there is a scarcity of research funding and academic jobs, there can emerge a gap between the demands of epistemic rationality and practical rationality. Then we can expect the lawful and chaotic good scientists to diverge behaviorally. In a context such as the credibility crisis, on top of dramatically low scientific standards, we also see serious contextual pressure that creates incentives for epistemically bad research practices such as high competition for scarce funding, too few employment or promotion options together with temptations such as a high demand for flashy results combined with low deterrence for questionable research practices or scientific fraud. In such circumstances, making a reliable and credible contribution to the advancement of scientific knowledge requires a higher level of epistemic virtue.

Lastly, if the behavioral indistinguishability argument is correct, then the research practices of the lawful good scientist are also those that are the most rational in economic terms. However, as we illustrated, this may easily be false. Many practices that contribute to the success of science often must explained in terms of individual epistemic virtue. The invisible hand mechanism can rarely promote some goods that are clearly in the collective epistemic interest, such as research transparency and methodological rigor. Well-conducted peer-review contributes to the success of science by filtering flawed research, but it is rarely in anyone's individual interest to do it well. Sharing one's data and code allows others to assess the reliability of research, but it is in a researcher's best interest not to do so. Increasing methodological rigor often comes with serious extra costs (Forscher et al. 2020). Consequently, researchers who make all the potentially relevant aspects of their research procedure publicly available and reproducible for others to freely use, who make the extra effort to maximize the accessibility and usability of their data and code by proper labeling, indexing and so forth, are not behaving in a maximally rational manner if modelled as credit-seekers.

"The rules of the game" are dynamic

Wray (2000) offered a criticism of Hull's virtue radicalism that nonetheless shares most of the premises of the invisible hand argument. He maintains that an invisible hand explanation has to be combined with a hidden hand explanation. The scientific institutions are intentionally designed in a way that scientists' pursuit of credit contributes (unintentionally) to scientific knowledge generation. Thus, we cannot completely deny an explanatory role to individual epistemic virtue, and we must significantly qualify the virtue-radicalist conclusion. Wray assumes as Hull does that scientists are primarily motivated by credit, such that the success of science is not due to their intentional pursuit of truth. Nonetheless, a concern for truth is not completely out of the picture, because "many of the practices and institutions constitutive of science are intentionally designed by scientists with an eye to realizing the very goals that Hull believes need to be explained by reference to an invisible hand mechanism" (Wray 2000). A hidden hand with properly epistemic intentions designs the rules of the game and the subsequent outcomes of the game can be explained purely in reference to the rules and the reward-seeking behavior of the players. It follows that if the institutional structure of science and the standards of scientific practice are initially designed competently and out of a concern for truth, collective epistemic virtue can be achieved even with only chaotic neutral scientists.

Wray's argument offers a significant counterpoint to the idea of the invisible hand by maintaining that the incentive structures do not self-organize into epistemically virtuous configurations; they are designed that way by epistemically virtuous people. But we can go much further. There is not only an initial design of scientific institutions with an eye to the advancement of scientific knowledge required, but regular "virtuous interventions" to the credit economy are also necessary in order to maintain the progressive character of a scientific community. This takes us still further away from the idea of an invisible hand.

The current reward system of science incentivizes QRPs and research misconduct and leads to the proliferation of bad methods, as metascientific studies amply demonstrated. External reward systems (credit as well as promotions, grants, awards) by their nature incentivize gaming or free-riding, and they do so to the extent that getting away with flawed or fake research is possible. Scientific quality control is far from being fail-safe. There are sanctions for misbehavior, but they are too low compared to those for misbehavior in other institutions, and misbehavior is too difficult to clearly identify. The process of peer-review falls dramatically short of being a reliable social mechanism for detecting error and fraud (Anderson et al. 2013; Shatz 2004; Smith 2006), and the need to reform it have been variously noted (see also Gibson 2007; Nosek & Bar-Anan 2012). With sufficient ill-intent and skill one can possibly pass their research reports through any quality control process. This epistemic vulnerability is not a problem when most scientists are indeed trustworthy in their intentions and most scientific error is honest error (Resnik & Stewart 2012). If there were no external rewards in science, it would probably attract only scientists with such intentions. But since external reward systems create incentives to exploit the epistemic vulnerability of the system, they also invite divergent motives. The scientific institutions are also not originally designed to fit the challenges of the industrial scale science we have today, which amplify the epistemic vulnerability of the system. So, the current combination of external reward systems with inherently vulnerable filtering and gatekeeping mechanisms makes it necessary that the rules of the game (e.g., publication standards) are constantly monitored, modified or calibrated with a view to decreasing incentives for free-riding and increasing incentives for research quality and rigor in the credit economy. On top of this, science also goes through constant social and technological changes, such as the transition from individual research to large collaborations or advances in research infrastructure, which requires appropriate modifications to the norms and standards of credit allocation. All this implies that the practices and institutions of science cannot be designed once and for all in a way that effectuates collective epistemic success, as Wray contends—the incentive structures of science must change and evolve in ways that cannot be pre-determined by initial design. Thus, a system which centrally features external rewards needs virtuous interventions by people who would in effect act outside of the credit economy in terms of their motivations, who would act as guardians of scientific goals and values. Their motivations should be exogeneous to the system, because there is no additional, specifically designed credit allocation mechanism to create a strategic or instrumental interest in truth in them. It is clearly possible to have a collectively virtuous scientific community that involves many chaotic neutral scientists, as the hidden hand argument has it, as long as the individual interests of these scientists are aligned with implementing epistemically good scientific practices. But maintaining a critical ratio of lawful good scientists is necessary to preserve the epistemic reliability of the structure of scientific institutions. The dominance of chaotic neutrals in the population of scientists could increasingly corrupt scientific institutions and reduce their capacity to safeguard collective scientific goals. In the worst case, we can end up with a scientific community that cannot even get to change its perverse incentive structures because there are not enough scientists who non-instrumentally value the aims of science among the ranks of executives at scientific institutions and science policy makers.

Conclusion

The structure of the credit economy arguably serves to increase rate and speed of scientific discovery, and to steer the attention of scientists to high-impact ideas. Without a competitive reward system, it is possible that scientists would pursue only whatever they deem worthy to know, and as efficiently as they deem appropriate. All these factors have a role in scientific progress, but it is a mistake to think that these serve collective epistemic interests unconditionally. As efficient and speedy scientific progress is in the collective epistemic interest, as is reliable knowledge generation and error elimination—but these are not the kind of aims that can be achieved via external rewards. On the contrary, any competitive reward system is by itself a risk factor from the perspective of reliability and credibility because it incentivizes unreliable research practices and misconduct. Thus, it is wrong to think that the same social mechanisms can facilitate fast, impactful and reliable outputs at the same time. Furthermore, the rules of the scientific game are not set in stone. They must also be re-interpreted, evaluated and updated in the face of novel challenges to scientific progress, because they are not valued in themselves but for their instrumental role in bringing about good collective epistemic outcomes. Epistemically virtuous scientists are necessary to navigate situations of axiological uncertainty or crisis, where the scientific community must collectively reason to redefine rules, standards, goals and values in accordance with epistemic rationality. For all these reasons, the scientific institution, like any other institution, needs a significant proportion of individuals who value epistemic goods independently of the given structure of the scientific credit economy.

References

Altman, D. G. (1994). The scandal of poor medical research. *BMJ*, *308*(6924), 283-284.

Anderson, M. S., Shaw, M. A., Steneck, N. H., Konkle, E. and Kamata, T. (2013). Research Integrity and Misconduct in the Academic Profession. In M. B. Paulsen (ed.), *Higher Education: Handbook of Theory and Research*, Vol. 28, Dordrecht: Springer, pp. 217–61.

Arslan, R. C., Eleftheriadou I. I. (2019). 'Revised: Are studies that replicate cited more?', Accessed: 28.08.2022. Available at: https://rubenarslan.github.io/posts/2019-01-02-are-studies-that-replicate-cited-more/

Bakker, M., Van Dijk, A., & Wicherts, J. M. (2012). The rules of the game called psychological science. *Perspectives on Psychological Science*, *7*, 543-554.

Bright, L. K. (2021). Why Do Scientists Lie? *Royal Institute of Philosophy Supplements*, *89*, 117–129. https://doi.org/10.1017/S1358246121000102

Button, K. S., Ioannidis, J. P., Mokrysz, C., Nosek, B. A., Flint, J., Robinson, E. S., & Munafò, M. R. (2013). Power failure: why small sample size undermines the reliability of neuroscience. *Nature Reviews Neuroscience*, *14*, 365-376. https://doi.org/10.1038/nrn3475

Camerer, C. F., Dreber, A., Forsell, E., Ho, T. H., Huber, J., Johannesson, M., ... *&* Wu, H. (2016). Evaluating replicability of laboratory experiments in economics. *Science*, *351*, 1433-1436. https://doi.org/10.1126/science.aaf0918

Candal-Pedreira, C., Ruano-Ravina, A., Fernández, E., Ramos, J., Campos-Varela, I., *&* Pérez-Ríos, M. (2020). 'Does retraction after misconduct have an impact on citations? A pre–post study.' *BMJ Global Health*, *5*(11), e003719. https://doi.org/10.1136/bmjgh-2020-003719

Chambers, C. (2017). *The seven deadly sins of psychology: A manifesto for reforming the culture of scientific practice*. Princeton, NJ: Princeton University Press.

Gibson, T. A. (2007). 'Post-publication Review Could Aid Skills and Quality', *Nature* 448, 408.

Greenberg, S. A. (2009). 'How citation distortions create unfounded authority: analysis of a citation network.' *BMJ*, 339, b2680.

Harzing, A-W., *&* Kroonenberg, P. (2017). 'The mystery of the phantom reference.' Accessed: 28.08.2022. Available at: https://harzing.com/publications/white-papers/the-mystery-of-the-phantom-reference

Heesen, R. (2018). Why the Reward Structure of Science Makes Reproducibility Problems Inevitable: *The Journal of Philosophy*, *115*(12), 661–674. https://doi.org/10.5840/jphil20181151239

Hull, D. L. (1978). Altruism in science: A sociobiological model of co-operative behaviour among scientists. *Animal Behaviour*, 26, 685–697. https://doi.org/10.1016/0003-3472(78)90135-5

Hull, D. L. (1988). *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science.* Chicago, IL: University of Chicago Press.

Hull, D. L. (1997). What's Wrong with Invisible-Hand Explanations? *Philosophy of Science*, 6, S117–S126. https://doi.org/10.1086/392592

IJzerman, H., Lewis, N. A., Przybylski, A. K., Weinstein, N., DeBruine, L., Ritchie, S. J., ... *&* Anvari, F. (2020). Use caution when applying behavioural science to policy. *Nature Human Behaviour*, *4*(11), 1092-1094.

Ioannidis, J. P. (2005). Why most published research findings are false. *PLoS Medicine*, *2*, e124. https://doi.org/10.1371/journal.pmed.0020124

John, L. K., Loewenstein, G., & Prelec, D. (2012). Measuring the prevalence of questionable research practices with incentives for truth telling. *Psychological Science*, *23*, 524-532. https://doi.org/10.1177/0956797611430953

Kitcher, P. (1993). *The advancement of science: Science without legend, objectivity without illusions*. Oxford University Press.

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

Munafò, M. R., Nosek, B. A., Bishop, D. V. M., Button, K. S., Chambers, C. D., Percie du Sert, N., . . . Ioannidis, J. P. A. (2017). A manifesto for reproducible science. *Nature Human Behaviour, 1*, Article 0021. https://doi.org/10.1038/s41562-016-0021

Nosek, B. A. *&* Bar-Anan, Y. (2012). Scientific Utopia: I. Opening Scientific Communication. *Psychological Inquiry*, 23, 217-243, DOI: 10.1080/1047840X.2012.692215

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. https://doi.org/10.1177/1745691612459058

Open Science Collaboration. (2015). Estimating the reproducibility of psychological science. *Science, 349*. doi:10.1126/science.aac4716

Peng, H., Romero, D. M., *&* Horvát, E.-Á. (2022). Dynamics of cross-platform attention to retracted papers. *Proceedings of the National Academy of Sciences*, *119*(25), e2119086119. https://doi.org/10.1073/pnas.2119086119

Romero, F. (2017). Novelty versus Replicability: Virtues and Vices in the Reward System of Science. *Philosophy of Science*, *84*(5), 1031–1043. https://doi.org/10.1086/694005

Shatz, D. (2004). *Peer review: A critical inquiry*. Tottowa, NJ: Rowman & Littlefield.

Simmons, J. P., Nelson, L. D., *&* Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science*, *22*, 1359-1366.https://doi.org/10.1177/0956797611417632

Smith R. (2006). 'Peer review: a flawed process at the heart of science and journals.' J R Soc Med. , 99(4), 178-82. doi: 10.1258/jrsm.99.4.178.

Smith, J. R., Louis, W. R., Terry, D. J., Greenaway, K. H., Clarke, M. R., *&* Cheng, X. (2012). 'Congruent or conflicted? The impact of injunctive and descriptive norms on environmental intentions.' *Journal of environmental psychology* 32(4), 353-361.

Strevens, M. (2011). Economic Approaches to Understanding Scientific Norms. *Episteme*, *8*(2), 184–200. https://doi.org/10.3366/epi.2011.0015

Strevens, M. (2003). The Role of the Priority Rule in Science. *The Journal of Philosophy*, *100*(2), 55–79. https://doi.org/10.5840/jphil2003100224

Resnik, D. B., *&* Stewart Jr, C. N. (2012). 'Misconduct versus honest error and scientific disagreement.' *Accountability in research* 19(1), 56-63.

Smaldino, P. E., *&* McElreath, R. (2016). The natural selection of bad science. *Royal Society Open Science*, *3*(9), 160384. https://doi.org/10.1098/rsos.160384

Tiokhin, L., Yan, M., *&* Morgan, T. J. (2021a). 'Competition for priority harms the reliability of science, but reforms can help.' *Nature Human Behaviour* 5(7), 857-867.

Tiokhin, L., Panchanathan, K., Smaldino, P. E., *&* Lakens, D. (2021b). 'Shifting the level of selection in science.' https://doi.org/10.31222/osf.io/juwck

Vazire, S. (2019). Do we want to be credible or incredible? *APS Observer*, *33*.

Wray, K. B. (2000). Invisible Hands and the Success of Science. *Philosophy of Science*, *67*(1), 163–175. https://doi.org/10.1086/392767