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Commentary

Behavioral Optimization in Scientific Publishing

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Peer-reviewed scientific publishing is critical for communicating important findings, interpretations, and theories in any branch of science. While the value of peer review is rarely doubted, much concern is being raised about the possible biases in the process. I argue here that most of the biases originate in the evolved innate tendency of every player to optimize one's own cost-benefit. Different players in the scientific publishing game have different cost-benefit optima. There are multiple conflicts between individual optima and collective goals. An analysis of the cost-benefit optima of every player in the scientific publishing game shows how and why biases originate. By continuing with the current publishing trend, the global distribution of the scientific community would be increasingly clustered. Publication biases by gender, ethnicity, reputation, confirmation, and conformity will be increasingly common, and revolutionary concepts will become increasingly difficult to publish. For a better future of science, it is necessary to design a publication system based on principles of human behaviour rather than on some ideological assumptions. If a system is designed in such a way that the conflicts between individual optima and the collective goal are minimized, if everyone cares only for his/her personal benefits, biases would get minimized automatically, and the progress towards the collective goal would be faster and smoother. Changing towards such a system might prove difficult unless a critical mass of authors takes an active role to revolutionize scientific publishing.

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Undesirable trends and bad incentives in science

In recent years, much concern has been raised about a number of alarming trends such as the reproducibility crisis, research misconduct, declining public trust, and publication bias in various branches of science (Ioannidis 2005, Voelkl and Würbel 2016, Elliott 2016, Hesselmann et al 2017, Milkowski et al 2018). One of the major drivers of these undesirable trends is said to be the bad incentives created among researchers by some elements in the academic systems (Stephan 2012, Smaldino and McElreath 2016, Zimring 2019, Radzvilas 2023). Predominant among the creators of bad incentives are the "publish or perish" policy, excessive reliance on bibliographic indices, and possible biases in the peer review and publication system. In the complex system of academia, several factors are interlinked and affect each other in multiple ways. This article mainly focuses on peer review biases, the underlying behavioural causes of these biases, and suggests a novel system towards potential solutions. There is no doubt that only mitigating peer review biases may not be sufficient to improve academia, and other factors will have to be addressed simultaneously.

Peer review and scientific publishing

Peer reviewing manuscripts is a recent norm in the history of science. The majority of journals started

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mandatory peer reviews mainly by the 1960s and 70s, although the concept has a long history and selected journals were practicing it. The original purpose of peer reviews was to complement the thinking of one research group by others in the field (Kelly et al 2014). The purpose of peer review often deteriorates to support a dichotomous editorial decision of rejecting or not rejecting a manuscript. Many flaws and limitations of the review process are recognized (Campanario 1998, Kelly et al 2014, Huber et al 2022), but most scholars seem to think that in spite of the problems, the review system generally serves a useful purpose for scientific publishing and cannot be spared or replaced (Campanario 1998). Owing to the confidentiality of the review process, whether the system really works in an unbiased way is difficult to test. While only the data on the frequency of acceptance has limitations in detecting bias (Squazzoni et al 2021), whenever there have been specific and well-designed experimental or statistical tests for detecting biases in the peer review system, biases were often detected (Campanario 1998; Phillips 2011; Tomkins *et al.* 2017; Haffar *et al.* 2019; Kuehn 2017; Lee *et al.* 2013, Huber et al 2022). Moreover, at times, peer reviews have deteriorated the quality of the paper by directly making the authors spin their statements (Lazarus et al 2016) or indirectly by their bias towards positive findings (Emerson et al 2010, Boutron and Ravaud 2018). These studies of peer review quality mainly look at and detect gender, ethnicity, and reputation biases. Potentially much more relevant to science are confirmation bias (Nickerson 1998) and conformity bias (Asch 1955) in the peer review process, but there are hardly any attempts to study them; again, the main hurdle is likely to be the availability of data. Conformity bias is shown to grow stronger as the importance of a judgment increases (Baron *et al.* 1996), and therefore, this bias is likely to be very strong in science. Conformity does not guarantee a sound outcome; in fact, it often suppresses logical and correct solutions by individuals (Fender & Stickney 2016). Conformity bias has a biological basis and cannot be said to result merely from the tendency to blindly follow the majority (Klucharev *et al.* 2009; Germar *et al.* 2016). Therefore, the community of researchers, however responsible and honest, cannot be expected to be free from this bias.

The response of the scientific community to any findings anomalous to the prevalent paradigm is described by Kuhn (1962). Peer review was not universally considered mandatory for publishing when Thomas Kuhn wrote "The Nature of Scientific Revolution" (1962). So, whether and how the peer review system may have affected the nature of scientific revolution is an open question. A nonconformist but sound and evidence-based concept can ultimately gain sufficient social support, but the process most probably begins with only a few individuals appreciating it (Allen 1975). In the peer review system, the editor typically invites 2 or 3 reviewers from the researcher community working in a field. Given that individuals appreciating sound but nonconformist ideas are rare, the probability that this small subsample of the population will have such individuals is very small. Therefore, by simple probability considerations, a piece of disruptive work has a small probability of getting published even after being communicated serially to multiple journals. Thus, the peer review system is likely to have increased rather than decreased the Kuhnian bias. Nonconformist findings and interpretations are numerically always small but often scientifically much more important than an average publication. Therefore, even if one finds that peer reviews are fair most of the time and are biased with a small probability, those biases could be disproportionately more important for the progress of science. Biases are potential hurdles in the progress of science on the one hand, but on the other, they are the cause of injustice to individual researchers (Chapman et al. 2019). Furthermore, there is evidence that peer review biases can drag research in the wrong direction (Lazarous et al. 2016, Yang et al. 2023, Kirchherr 2023) or be responsible in part for the slowdown of the progress of science (Chu and Evans 2021, Park et al. 2023). Therefore, for healthy science on the one hand and for justice to individual researchers on the other, the causes of peer review biases need to be analysed, the evolutionary psychology behind them understood (Watve 2017, Radzvilas et al. 2023), and appropriate steps taken to minimize them.

In this paper, I intend to analyse how cost-benefit optimization by every player in the scientific publishing game is likely to influence the outcomes. Further, I will also discuss the ways to address the concerns and build up an ideal new system that is likely to minimize the biases by reducing the conflict between individual optima and the collective goal of science.

Rationalization and human decision making

It has been known for over half a century that human decision-making is not a straightforward and sequential 'rational' thinking process. 'Rationalization' refers to a phenomenon where a decision has already been made subconsciously, and then the individual

concocts beliefs, principles, and justifications to rationalize the decision (Brehm 1956; Cushman 2019; Sharot *et al.* 2011). Rationalization is not simply an attempt to discover the causes behind a decision. It is often an attempt to construct or invent a new set of beliefs that are socially convenient. In soft rationalization, people only try to justify their action, but in hard rationalization, they make themselves believe that the stories they have concocted are true (Cushman 2019). Different sets of experiments from independent researchers have shown that once a decision is made, people tend to modify and often invent reasons to reduce cognitive dissonance, i.e., the contradiction, if any, between thinking and action (Harmon-Jones & Harmon-Jones 2007).

If these principles of human decision-making are so fundamental to human nature, we cannot continue to pretend that they do not apply to editorial decisions and reviewers' recommendations. We need to understand the inevitable human elements in scientific publishing. It is likely that the true reasons for a decision and the justification given for the decision have only a partial or no overlap. A decision-maker may not be consciously aware of all the reasons they made a particular decision. Therefore, the comments in a review report are likely to be a set of post-decision justifications, and there is a more complex subset of reasons responsible for the decision that never surfaces. Therefore, when we analyze peer review data, we cannot rely entirely on the written comments. It is necessary to look for statistical patterns that would reveal any other factors affecting decisions.

Research is a noble profession, and a researcher undergoes substantial training not only in research methods but also in research ethics. It is an assumption that there is a high level of honesty and commitment to science in the research community. Although this assumption may be largely true, there are two caveats. One is that even an honest mind is prone to biases that it is not consciously aware of. The second is that as the number of research organizations, along with the number of researchers and journals, increases globally, it may be impractical to rely on the assumption of honesty and integrity. We will, therefore, start by assuming that the elements of evolved human behaviour will be at work all the time and predict the possible effects of this on the possibility of biases in scientific publishing. Any deviation from this can arise out of a conscious commitment to the principles of science.

Cost-benefit optimization in human behavior

Optimality theory is an important element of behavioural ecology and evolutionary psychology, which assumes that a strategy that optimizes the cost-benefits gets selected. Optimization models improve our understanding of adaptation and innate behavioural tendencies (Parker & Smith 1990). Although there has been serious criticism of some aspects of optimality theory (Pierce & Ollason 1987) at a conceptual level, the theory of optimization of strategies continues to be useful to address behavioural questions (Rahnev & Denison 2018). There is considerable debate over the application of optimality to humans (Driscoll 2009; Rahnev & Denison 2018); nevertheless, behavioural optimization models have been used to explain human behaviour in nutritional (Nettle *et al.* 2017), ecological (Watve *et al.* 2016), and social contexts (Purshouse & McAlister 2013). Therefore, it would be reasonable to assume that even in scientific publishing, all actors do cost-benefit optimization in making decisions, which may often be at a subconscious level.

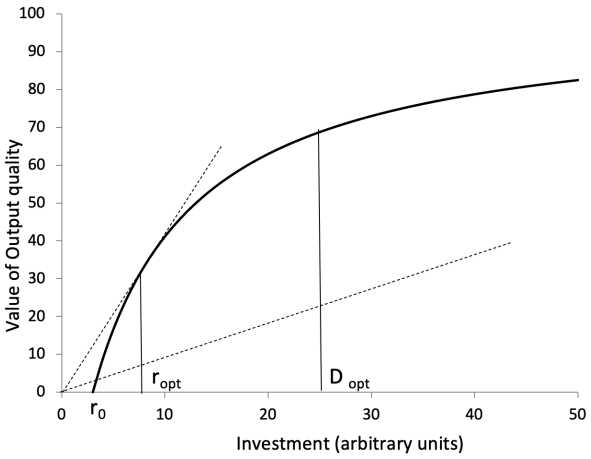

Figure 1. The ratio versus difference optimum: (adopted with modifications from Shinde et al 2022). When the returns on investment follow the law of diminishing returns with an initial cost C_0, the ratio optimum (r_{opt}) is obtained where a tangent from the origin touches the curve. The difference optimum (d_{opt}) lies where the slope of the curve equals unity or parallels the break-even line. For any profitable deal, the ratio optimum always lies to the left of the difference optimum, i.e., a ratio optimizer tries to minimize investment, and a difference optimizer tries to maximize output.

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A recent insight into optimization is that while optimizing investment into one unit at a time, under certain contexts, people tend to maximize the ratio of returns to the investment, and in certain other contexts, the difference between returns and investment (Figure 1). Theoretically, when the investment opportunities are limiting but the investible amount is not, a difference model is appropriate, and when the investible amount, and not the investment opportunities, is limiting, a ratio model is appropriate (Watve *et al.* 2016; Watve & Ojas 2019, Shinde et al 2022). A difference optimization model maximizes the benefit per investment opportunity and, therefore, when investment opportunities are limiting, this is the model of choice. On the other hand, the ratio model maximizes the benefit per unit investment, and therefore, when the investable amount is limiting, a ratio optimum should be used. Watve & Ojas (2019) and Shinde et al (2021) argued that people intuitively use these rules appropriately. I will assume here that different players in the scientific publishing game have an innate knowledge about these rules and they subconsciously choose the right model in the right context.

For maximizing a ratio, reducing the denominator is a more effective strategy than increasing the numerator. Therefore, a ratio optimizer is keener on cutting the costs. A difference optimizer is more interested in increasing the output even if it needs greater inputs, as long as the increase in inputs is not greater than the increase in output. Since the two optimization strategies often have diametrically opposite effects on behaviour, it is necessary to examine whether each of the players in the scientific publishing game is a ratio optimizer or a difference optimizer.

While for a researcher, the costs of publishing are the inputs in terms of time, energy, and intellectual intensity, the benefits are more varied. A scientist's mind should and does perceive intellectual benefits as important. Being able to solve a problem, being able to raise a novel question, designing an experiment, getting expected results, or being able to interpret surprise results are all intellectual benefits a researcher would certainly seek. But this is not at the exclusion of other costs and benefits. Apart from the intellectual costs and benefits, reputation within the research community as well as among laypeople is an important benefit sought after by researchers. A range of other benefits forms a part of the system of research and publishing. They include job prospects, tenure, power positions, successful publications, good citations of publications, applause for a talk, as well as direct monetary gains such as pay scale, royalties, and others. Different individuals give different weightings to the different benefits, but generally, in the field of science, reputation appears to be among the top-rated ones, and it also influences many of the other benefits. It is difficult to make any quantitative models with the complex and multidimensional currency structure, but it is certainly possible to make a set of qualitative inferences and predictions about the cost-benefit optimization strategy of every player. The inferences of the cost-benefit analysis stated below should be taken as testable hypotheses. Testing them empirically is certainly possible if peer review data are made public.

- Editors: Editors are important decision-makers in the publication process, and although their decisions are guided by review reports, they are the ultimate decision-makers (Etkin et al 2017). We can view the cost-benefits of editors at two levels. There are certain cost-benefits of accepting an editorial responsibility, and there are cost-benefits of every manuscript handled.

  While accepting an editor's responsibility, a researcher commits time, energy, and intellectual inputs. The benefits are variable depending on the nature of the journal administration. In some systems, the editors are employed as full-time or part-time editors and are paid. In contrast, in a peer editor system, editors are active researchers themselves but may not be paid separately for being an editor. Some of the benefits may differ between the two, but others are common. The editor knows by experience which reviewers are more liberal versus more critical and thereby has substantial control over the probability of a manuscript getting accepted or rejected, and they can play this card diplomatically. As a result, an editor's position is a power position, and although it has a large associated cost, many researchers may be happy to be offered one.

  At the level of manuscripts handled, the cost-benefit optimization strategies depend on the journal and the community context. For a new journal or a highly specialized journal, the number of submissions can be small, and in that case, the editors are more likely to be difference optimizers. In contrast, for any established journal with high repute, the number of manuscripts communicated is always large, and therefore the editor's time is the limiting factor and not the number of communications. With the global increase in the number of researchers, this situation is faced by almost every journal. As a result, editors are

expected to become ratio optimizers and not difference optimizers. Minimizing the time and effort in making a decision is the best optimization strategy, even at the cost of the accuracy of the decision.

I assume that there is a price to be paid for a wrong decision in terms of loss of reputation (Tancock 2018). However, this price is highly asymmetric. There are two types of wrong decisions, and the price to be paid is widely different. If a 'bad' paper is accepted and published, it can cause serious damage to the journal's reputation and thereby to the editor's reputation too. Therefore, extreme care is needed before accepting a paper. However, the 'bad manuscripts' category may include not only the ones with problems or shortcomings in the scientific quality of the paper but also the ones politically incorrect or going against the mainstream thinking in a field. Publishing some findings against established star researchers in a field may irk them. The influence of star researchers on publications in the field is well demonstrated by the significant change in the pattern of publications that follows the death of a star (Azoulay *et al.* 2016). An obscure author challenging one or more giants in the field is a high-risk situation for the journal, and avoiding it helps the editor minimize cost.

The other type of error, i.e., rejecting a 'good' paper, has little punishment. I will avoid going into the definition of a good paper but assume for the sake of argument that there are good papers that, in the absence of bias, the journal would be happy to publish. Rejecting a 'good' paper could potentially have an opportunity cost, but if the quality of a paper is increasingly being decided by where it gets published, this cost becomes negligible. Also, the information that this journal rejected a potentially important paper does not become public, and therefore it does not have any reputation cost. Only the authors know of the rejection, and they themselves are quite unlikely to talk about it since rejection is perceived as damaging to their own reputation. Therefore, even if a rejection is unjustified, the editor does not have to pay any cost for the wrong decision. In journals with high reputation, a large proportion of papers are rejected without reviewing. This is because the time required to review such papers is treated as non-productive time, and the cost-benefit optimization demands that non-productive time should be minimized. In order to reduce non-productive time, the editor needs to make a quick judgment of the quality of the paper. Since reading a manuscript has a high cost, a number of surrogates help reduce it. There is a perceived positive correlation between the reputation of the institution from which the manuscript comes and the quality of the paper. Although good papers can potentially come from obscure places and at times bad papers from reputed institutions, editors can certainly save time and energy costs by applying probability rules. A probability-based decision is good from the cost-benefit optimization point of view since quick and careless rejections save costs, and a wrong rejection does not result in any penalty. Therefore, quick rejection without reading the manuscript is a good strategy if it comes from unknown and non-reputed authors, organizations, or countries. The reverse is not true. Manuscripts from reputed authors or organizations cannot be accepted carelessly since accepting a 'bad' paper can carry a serious penalty. The expected end result is that a type A error (accepting a 'bad' manuscript) is less probable, but a type B error (rejecting a 'good' manuscript) can be extremely common. Experimental studies are compatible with this prediction. Reviewers are more likely to accept a manuscript from a reputed place, and acceptance is more likely when they know it comes from a reputed place in comparison with a blinded control (Tomkins *et al.* 2017, Huber et al 2022). For journals that have a high rejection ratio, this implies that if a manuscript comes from a less-reputed organization or country, it is most likely to be rejected without a careful read. This can be tested easily if editorial decision data are made available.

The cost-benefit optimization applies to the choice of reviewers as well. Since in the current system reviewers have little direct benefit in reviewing a paper, the editors often have to ask a favor from busy researchers. The reviewers are likely to be happy with certain manuscripts, particularly the ones that support or uphold their hypotheses with new data. Accordingly, the editors may gain a social benefit by pleasing highly influential scientists by sending them such manuscripts. As a corollary, any nonconformist manuscript is more likely to go to 'lesser' scientists, if at all reviewed.

- Reviewers: In the prevalent system of scientific publishing, there is no direct benefit awarded to a reviewer, and it is presumed to be a sincere duty of any researcher in the interest of science. Busy researchers are often reluctant to accept review requests, and even when they accept one, it is a low-priority task. Therefore, getting reviewers for every manuscript is often a headache for editors. Since reviewers remain anonymous most of the time, their

reputation is rarely at stake. The cost incurred is in terms of time and effort. Nevertheless, there are several indirect benefits to the reviewers. They can avail themselves of social and political benefits such as building good relations with the editors who are likely to handle their manuscripts in the future. They have access to new research in their field before publication (but with the increasing popularity of preprint archives, in some fields, this benefit is fading away fast). Perhaps more important than all this is that they can suppress evidence against their own points of view and promote evidence in support. However, these benefits do not increase in proportion to the time and effort spent in thorough reviewing. Therefore, for a reviewer, maximization of the benefit-cost ratio is best done by minimizing the denominator.

A very effective way to decrease the denominator is to transfer the responsibility to a junior researcher, post-doc, PhD student, or even a project assistant in the lab. Since the reviewer's reputation is not at stake, it is possible to do so and get away with it. With the increasing volume of scientific publishing, reviewers are under huge demand, but there is surprisingly little incentive for as well as quality check on reviewing. Indeed, many reviewers do it in the interest of science, and it is a significant contributor to the quality of scientific publishing, but careless reviewing may not be as uncommon as believed since it maximizes the benefit-cost ratio.

The rationalization principle applies most extensively and appropriately to reviewers. There are a number of possible reasons why a reviewer would like to recommend acceptance or rejection. However, not all of them can be stated in the review report. Moreover, the reviewer may not even know all the reasons behind his/her decision. Nevertheless, impressive rationalization needs to be done. How the reviewers decide to recommend acceptance, revision, or rejection and how they rationalize their decision is a fascinating subject of psychology, but currently, owing to the confidentiality of the review process, little data are available.

- Journal administration: Journals are owned either by Scientific Societies or by Publishers. Although the commercial interests may be different from scholarly interests, in either case, reputation and impact-factor-like indices are extremely valuable for journal administration. The journal publisher may not interfere in the day-to-day editorial process, but they decide the scope and policy of the journal and appoint the editors. The scope and policies can influence the impact-factor-like indices substantially. Since themes in trend are more likely to attract more citations and thereby increase the impact factor, they are keen to cover such areas. As more journals give preference to trendy areas, the trend reinforces itself. This positive feedback process is expected to strengthen trends and further inflate the importance of quantitative indices.

  Journal administrations frequently have their own marketing units that issue press notes on selected published papers that can make news headlines. Selection of papers for media coverage is decided by their potential public appeal and sentimental value, which is more likely to further reinforce trends. Trends benefit publishers, and publishers are therefore interested in strengthening the trends. This vicious cycle is expected to draw the field away from scientific concerns and become more sentimental and populist.
- Readers: The cost-benefits for readers are rapidly changing with the increase in online publishing. In previous decades, the reader or his/her organization had to subscribe to a journal. Now, there is an increasing trend to charge the authors and give free online access to the reader. As a result, the optimization strategy of the reader has drifted from difference to ratio optimization. The reader has to choose articles of real interest from a huge pile of publications. This has certain reflections in the writing style as well. Titles are becoming shorter and more attractive, abstracts have to make all major findings explicit, and in addition to technical abstracts, highlights and graphical abstracts are reducing readers' screening time cost per article. Readers' methods of searching for articles also change the cost-benefits for authors and thereby influence what career-seeking young researchers would like to do.

  Online searches have a dual effect on publication access. In the old system, certain journals were widely subscribed to and others were not. Therefore, it was more important to publish in a widely subscribed journal. Today, almost every journal is searchable online, and therefore the relevance of the prestige of a journal should vanish, but surprisingly, the importance of journal prestige is on the rise instead of declining. One of the reasons is likely to be in the cost-benefit optimization of the reader. Since the reader has to choose among a very large set of available literature, he/she may exert a surrogate choice based on impact-factor-like indices. A substantial proportion of readers of research papers are researchers themselves. Although for them,

accessing and reading may become independent of impact factors or journal reputation, when they cite papers in their manuscripts, they tend to think that citing high-impact journals is likely to increase the perceived value of their manuscript. This is also a testable hypothesis, and access to review data would help the analysis. This is another potential positive feedback process in the system that is expected to affect the social structure of the researcher community.

- Funders: Research funding for basic science mainly comes from government or non-government funding agencies. The funding agencies are investors who would be interested in maximizing the returns on investment. The returns are measured in terms of publications, patents, and commercialized processes if applicable. Since the rate of success is important for them, they are likely to prefer projects that are assured of success and avoid novelty and risk. There are two standard ways of ensuring this. One is to rely on the reputation of the investigator and his institution, and the other is to see whether the path ahead is clearly defined so that the chances of success are good. The flip side of this optimization strategy is that novel, controversial, and risky ideas are less likely to be supported, since new ideas often come from young researchers who are yet to establish a reputation, and also new ideas often have an uncertain path of progress. The cost-benefit optimization of funding agencies certainly stands in conflict with novel ideas and potentially revolutionary research paths.

  Again, since the number of funding proposals is large, funders need to be ratio optimizers and not difference optimizers in evaluating proposals. They need to make judgments with minimum evaluation inputs. Further, they need to rely on a set of experts. Having a large number of experts involves greater management cost. A small set of experts may not be sufficient to have in-depth knowledge of all the super-specializations that the funding requests come from. Here again, the optimization would go by ratio rather than by difference. Ideally, a researcher's potential and quality should be judged by reading his published work, but there is a huge cost in reading, which can be cut down substantially by the use of impact-factor-like indices. Here again, probability-based decisions are most cost-effective, and therefore there is no need to thoroughly read a proposer's publications and the proposal itself. In systems where the experts need to give justifications for their shortlisting, they need to read something at least to enable them to find justifications for their decisions. Here too, the cost-benefits of acceptance and rejection are highly asymmetric, so while positive decisions need to be more careful, negative decisions can be careless, and there is little penalty for a false negative.
- Universities/Institutions: Organizations that support researchers pay a huge cost in employing researchers and providing necessary infrastructure, but they do this simultaneously with a large number of researchers working in different fields. This is a high overhead ratio optimization scenario, which necessitates that they are careful in their choice of researchers and also monitor their output. Most organizations have systems of evaluation for researchers at the time of selection as well as periodically throughout their career. However, a problem lies with the criteria used for evaluation. Ideally, evaluation of scientific work can only be done by detailed reading of the published scientific work of a researcher. However, evaluators may not have the necessary expertise for it and, moreover, do not have the necessary time to do so. As a result, they need to rely on surrogates. The number of publications along with impact-factor-like indices can provide a quantifiable surrogate index, which can effectively save the reading cost. This is the main reason why indices like impact factor and h-index have gained importance in the evaluation of a researcher (Chapman et al. 2019). If researchers were being evaluated by reading their work, impact factors and the h-index would have rapidly become irrelevant to researcher evaluation, but with the increasing numerical volume of the scientific community, and also with increasing technical specialization, it is becoming increasingly impossible to evaluate a researcher by reading his/her published work. Evaluating someone's science without reading it is pseudoscience, but it has a highly favorable benefit-cost ratio compared to rigorous science. Therefore, it is inevitable that pseudoscience will replace rigorous science rapidly. The impact-factor-like indices have faced serious criticisms on several grounds (Bohannon 2016; Callaway 2016; McKiernan *et al.* 2019), but they will continue to retain their importance because of behavioral reasons, if not for scientific reasons. Since they alter the cost-benefits of evaluation favorably to the evaluator, it should be accepted that they will continue to be used although they are completely unscientific. Science will have to accept this unscientific component in the present academic structure because it saves cost dramatically.

- Researchers/Authors: Since reportable scientific findings are more likely to be limiting than the time required for writing and publishing, researchers are most likely to be difference optimizers when choosing what to research. Researchers' investment per publication is the maximum among all the players and includes time, energy, effort, intellectual inputs, and at times their own money. Online publishing and author charges have added to the costs for the author. The benefits they expect from a publication include credit, reputation, intellectual property rights, as well as contribution to quantitative evaluation criteria which may decide their salaries, tenure, or job security. Sometimes, getting direct monetary benefits such as patent royalties or indirect benefits such as an increased likelihood of research grants crucially depends upon prestigious publications. Since most persons involved in deciding a researcher's career path cannot afford to pay the time cost of reading his/her publications, good research achievement predominantly means being high on the indices (Chapman 2019). Often, some level of selection is made at a preliminary level before it goes to an expert committee. At this level, the indices predominate, although at the committee level, at times, the real merit may be given some importance. This is also a type A and type B error problem, and some deserving candidates may get eliminated without reading their work.
  Since the surrogates for quality matter a lot, apart from doing high-quality science, researchers need to have several other concerns in order to maximize the quantifiable indices (Chapman 2019). Work on trendy themes gets quicker citations; therefore, it pays more to be a trend follower than to be a trendsetter. Out-of-the-box ideas often meet with more reluctant responses, skepticism, or outright disbelief, and it is a more advantageous strategy to avoid them. There appears to be an optimum level of novelty for a piece of work to receive appreciation. A high level of novelty has a high cost and more unpredictable benefits. Although the intellectual satisfaction as a benefit may be very high, individuals relying substantially on career benefits may find it useful to keep the novelty low to moderate. It might be particularly important to avoid confrontation with the currently dominant school of thought in the field and the interests of the dominant personalities in the field. Thus, cost-benefit optimization is expected to reduce novelty, discourage disruptive ideas, and expand on existing paradigms more often.
  As part of the optimization strategy, most researchers seek positions in more reputed institutions or universities. It is easier to publish from a more reputed institution since that is an important surrogate in editor and reviewer decisions. It is also easier to attract funding. Therefore, an attempt to get a position in an organization with a better reputation is a part of strategic optimization for any researcher. A position in a reputed institution and publication in high-impact journals exhibit a positive feedback vicious cycle. Thus, apart from doing high-quality research, researchers need to employ many other strategies in order to optimize the cost-benefits of publishing their research.

Effects of cost-benefit optimization on the quality of science and social justice among the researcher community

Assuming that every player tries to optimize his/her own cost-benefit, a number of consequences for the quality of science and the rate of progress are inevitable. (i) In the current system, low-quality science is unlikely to get accepted in journals of high repute, but good-quality science from lesser-known researchers is equally unlikely to get accepted there. The possible effects of type B error being more likely than type A may not be visible since instances of type B error always remain hidden. In the absence of data, it is impossible to know how much loss science suffers due to type B error, but it certainly is unjust to good researchers who are less connected to the power centres of scientific publishing. (ii) The second possible effect of the current cost-benefit structure is that novelty is likely to be increasingly discouraged. This is already a detectable trend in science (Park et al. 2023, Yang et al. 2023, Kirchherr 2023), and peer review and other biases are likely to be important causal factors. Novel ideas are often high-risk ideas, and they are more difficult to get funded as well as published. Even more difficult is theoretical or experimental work that goes against a currently prevalent paradigm. Paradigm shift is difficult in science owing to many reasons (Kuhn 1962, Watve 2017). Many of the reasons existed before peer review, but peer reviews have created an additional hurdle for potential scientific revolutions that Thomas Kuhn (1962) had not foreseen. (iii) The third important consequence of the current system of peer review is the inequality of science organizations. It is not very difficult to practice good science in a third-world

country or a university with minimum facilities, or even on a citizen science forum. Not every field of science needs huge amounts of funding, and certain types of work can be pursued with high scientific quality in any corner of the world. However, owing to the type B factor, it would be difficult to publish high-quality work in reputed journals from less reputed places. As a result, good researchers would strive hard to get into organizations of high repute. These organizations can afford to invest substantially in attracting good researchers. Moreover, it is easier to get better publications from such organizations, which works in a positive feedback cycle. Such vicious cycles are expected to make the field increasingly imbalanced (Wapman et al. 2022), and it becomes difficult to spread good-quality science throughout the globe. The imbalance in science has many political consequences, and therefore dominant political forces would try to protect the imbalance, but it is in the interest of science and humanity that science should spread globally in a more equitable fashion. Although the importance of the reputation of institutions is and should remain relevant, it should not escalate to monopolizing. It should not be impossible to do and publish good science from less reputed places, but with the current structure of science publishing, the cost-benefit optimization of editors and reviewers is bound to increase the imbalance.

It is necessary, therefore, to study and analyze the systems of scientific publishing and design better systems in the interest of unbiased and globally accessible science. The first step should be making peer review data available for research so that a number of hypotheses, only some of which are made explicit in this article, become testable. In light of such studies, designing more open and unbiased science publication policies will be possible. A foundation for designing unbiased scientific publishing systems needs to be the principles of human behaviour. Without understanding and incorporating the principles of human behaviour, a system based only on good intentions and only an appeal to all players to be responsible (Chapman et al 2019) is unlikely to work in a fair and just manner. Peer review data is also an important resource for research training, career development, developing healthy ethical standards, and so on. Keeping this important resource hidden is a great loss to science. Even cost-benefit decisions are improved by better access to information. Bounded rationality, with inadequate information or experience, can lead to irrational or disastrous decisions. Therefore, the first step in improving academic systems should be to make all review data public. This will help us build alternative systems with a better understanding of the past and the present. It is possible to suggest designs of alternative systems that would potentially reduce biases, which I attempt below. The attempt is limited by the inaccessibility of data to learn from; nevertheless, based on whatever is known and inferred, alternative systems can be designed and experimented on.

A behaviour-based alternative system

The problems with the current system are often recognized and discussed, and some alternative experimental systems are under trial (Bravo 2019, Radzvilas et al 2023). Some journals have practiced double-blind or even triple-blind reviews. The success of blinding is extremely limited because the human mind is not evolved for impersonal judgments. If the author's identity is masked, the reviewer's mind spontaneously starts guessing the source. In fact, studies have shown that masking fails to hide the identity of the authors quite often and thereby blinding is a failure. Reviewers spend substantial effort in imagining the author identity (Kuehn 2017), which may worsen the quality of the review. Furthermore, the growing pre-print trend directly hampers attempts to hide author identity. Therefore, double-blind peer review fails to improve the review quality (Justice et al 1998, Cho et al 1998, Goldbeck-Wood 1999, Brown 2006, O'Connor et al 2017) and can at best be a pretense or a smokescreen.

On the other hand, a number of researchers subscribe to the open science movement (Siew 2017), and a number of experiments are being performed on open peer reviews (Ross-Hellauer 2017, Else 2022, Mailman School of Public Health 2022, Radzvilas et al 2023), although not without debate (Abbott 2023). As yet, only a minority of researchers appear to subscribe to the alternatives. In some journals, reviewers' comments and author responses are made public, but that is only for accepted papers. Biases are more likely with the rejection decision, and therefore making the reviews of rejected manuscripts public is more relevant. The alternative peer review systems by journals such as eLife or Qeios are welcome as experiments, although they may still be far from achieving a bias-free review system.

Since good quality reviews are crucial to scientific publishing, the cost-benefit optimization of the reviewer should be the first consideration in designing an alternative publishing system. To optimize the cost-benefit proposition for reviewers, it is necessary to give sufficient incentive for reviewing as well as allow

reviewers to build a reputation for good quality reviews. This can be achieved by making the review reports public with optional anonymity. However, currently, journals that do so only publish review reports of accepted papers. Rejection recommendations can still be accompanied by hurried conclusions and irresponsible comments, and reviewers can get away with it without affecting their reputation. Anonymity should be optional for the reviewers, but if reviewers are ready to disclose their names, the published reviews should be considered a valid form of publication that can be credited to the reviewer. Reviewers should be able to enrich their CVs with published review reports, which should be given some importance by their evaluators at any level. Anonymous reviews cannot be included in evaluation for obvious reasons. The choice of being anonymous would then lie with the reviewer. There could be specific conditions under which the reviewer may not like to disclose his/her name, but commonly, disclosing it would be beneficial for the reviewer. Inclusion of reviews in evaluation would provide a substantial incentive for reviewing, but at the same time, the publication of review reports would impose a reputation cost for bad quality reviews. Since all reviews are made public, bad quality reviews will threaten the reputation of a journal as well. Thereby, bad quality reviewers are unlikely to get further review requests from editors. This carrot and stick approach can fundamentally alter the cost-benefits for reviewers, motivating them towards greater effort, greater quality, and timely input.

Open or public peer reviews may not be free of problems, and some potential problems have been pointed out (Etkin et al 2017). Every model of peer review is bound to have some pros and cons, but the first concern of science needs to be a commitment to the principles of science. Public availability of raw data for independent analysis and cross-questioning is a primary requirement of today's science. Keeping anything hidden and unavailable for independent analysis is unscientific, and there is increasing consensus about data transparency. By this principle, confidential peer reviews are unscientific and need to be abandoned right away, even if the alternatives come at some extra cost or implementational difficulties. Therefore, open peer reviews will have to be the norm in the near future. The debate can be only about the form and the modalities by which peer reviews can be made public.

It is easy to visualize the publication of review reports for an accepted paper. It is less obvious how to publish reviews of rejected manuscripts. There are two solutions to the problem. One is that reviews could be posted on pre-print servers independent of acceptance or rejection. Some pre-print servers currently accept posting review reports and authors' responses even for rejected manuscripts, but they do not allow disclosing the journal name. A single step change in this policy would allow making all reviews public. This will substantially improve review quality, since bad reviews would bring bad repute to the journals. Making review comments public can reduce the asymmetry in the penalty for wrong decisions. Rejecting a good paper would also have some reputation cost in the long run, although not as large and immediate as that for publishing a bad paper.

Another potential solution is more radical, and that is to change the acceptance and rejection system. Only a few journals have eliminated the acceptance-rejection decision after review. eLife proposed to publish all papers under review along with review reports, but it still has desk rejection, and all possible biases associated with desk rejection will remain unchanged. Qeios does not make dichotomous decisions, but such a system is likely to end up with a lot of junk getting published. I have a novel suggestion that can avoid both problems effectively. In this system, the editor does not make an acceptance or rejection decision, but based on the reviewers' comments, the editor assigns a grade to the paper, say going from 0 to 1, which would be predominantly published at the top of the paper itself. Then the ball is in the authors' court. The authors decide whether to publish the paper in the given journal with the comments obtained and the given grade or to retract and resubmit to the same or a different journal hoping for better comments and a higher grade. If the comments expose major flaws in the work, the authors will get exposed by publishing along with the comments. They should better revise their work. The bibliographic listing of the paper should include the grade along with the volume, page numbers, etc. In such a system, the product of the journal reputation index and the grade obtained by the paper should be used in the author's profile instead of the journal impact factor alone. So publishing in a highly reputed journal with a near-zero grade may be bad for the authors, and they may decide to rework and resubmit. This would prevent a lot of junk from being published. It is likely that the reviewers' comments and the grade are unfair. In such a case, the authors should be free to choose to publish even with a low grade if they are confident about their work and think that the comments and the grades are unfair. Since everything is transparent, the reader is free to judge the fairness of the reviewers. This would allow publication and

dissemination of scientific work uninhibitedly, but at the same time with comments and grades from expert reviewers from the field published. So readers would have access to both the publication and its score, and they are free to form their own opinion. At the same time, since comments are also published, the responsibility of reviewers would be much higher, improving the review quality itself. In any case, readers would be the ultimate evaluators. It is likely that some work might be ahead of its time and therefore not appreciated by reviewers at the time of publication. Today, such work often does not get published. In the new system, it would certainly get published, maybe with less positive comments, but since it gets published, it would become accessible, and its importance would be realized when the right time arrives. In the current system, reviewers' views can stop publication, and readers do not have the freedom to differ from the reviewers. This freedom will be brought on board if the ultimate choice of publishing or not publishing lies with the authors. They just have to understand that their paper will get published along with the comments and grade offered.

A possible objection to such a system is that reviewers may be reluctant to review since their responsibility increases. Alternatively, they might be less critical and hesitate to point out flaws in the manuscript. Studies on open peer reviews have already shown that open peer reviews do not increase the reluctance of reviewers (Rooyen 1999, Bravo 2019). Another potential objection could be that the volume of literature being published is already huge, and the reader has no time to read more. Leaving the ultimate judgment to the reader will not work under these circumstances. Considering the principles of human behaviour, this objection is unwarranted. Experimental psychology has demonstrated that just having the possibility of others watching you is sufficient to alter human behaviour (Bateson et al 2006). It is not necessary to actually be watched. The possibility that whatever you write is being made public is sufficient to make the reviewers more responsible for the quality of the review report. It is not necessary that many readers read them. Another undesirable possibility is that reviewers will hesitate to point out flaws and deficiencies in manuscripts and only write positive comments to improve their CV. However, since their comments are also getting published, they have a responsibility here. If they happen to endorse a paper that is obviously flawed, they become a party to the flaws, and that is public. So being critical and rigorous but open-minded and polite will prove to be a good strategy for the reviewer.

If authors decide to retract the paper from a given journal owing to a low score or adverse comments, they should be free to publish the review reports either on pre-print servers or along with publishing their paper somewhere else. There is no need to hide the name of the rejecting journal. If the authors declare that this paper was given a zero score by the editors of the previous journal, and the paper happens to receive some importance later, the decision of the editors of the first journal will be exposed. This increases the responsibility of editors while grading as well.

A practical problem that reputed journals might face is that they will be flooded with manuscripts, and it would be impossible to review them all. To prevent this, the primary editorial judgment and decision to return the manuscript should be retained as it is today. However, this decision will have to be made carefully with appropriate justification, which the authors may make public if they wish. A number of journals have standard pre-drafted letters for rejections at this stage. This creates some tricky situations. In a true story of a manuscript communicated, the authors had shown that a fundamental assumption behind the prevalent mainstream theory in a field was wrong, which could have been sufficient to topple the theory itself. This manuscript was returned on the grounds of "not having sufficient novelty." This implied that it was already known that the prevalent theory was wrong. The authors then asked the editor whether they could quote this comment in support of their central argument that the theory was wrong. The editor then admitted that certain sentences are routinely written for manuscripts that are returned without review. Editors generally get away with this type of negligence because the editorial correspondence does not become public. There are more examples where the justifications given for returning a manuscript without review can put the editors in trouble if the authors cross-question the justification (Watve 2020). If accessibility of all editorial correspondence becomes the norm, even rejection decisions will have to be made with responsibility.

How this system would handle biases and other currently faced difficulties

The three main features of the suggested system in decreasing order of importance are

i. Transparency and public accessibility of the reviews

ii. Recognizable reward credited for reviewing, and for consent to disclose the reviewer's name
iii. Authors ultimately make the publication decision.

The optimization dichotomy would remain as it is in the sense that editors and reviewers will remain ratio optimizers and authors mainly difference optimizers, but the asymmetry in the cost-benefits of acceptance versus rejection decisions will reduce to a large extent, if not completely. Since all reviews and decisions are freely accessible, rejection decisions would also have to be made carefully. Reputation damage by a type B error would still not be as large as that by a type A error, but it will be some non-zero positive compared to the near zero of the current system. So the symmetry will not be perfect, but better than what it is now.

Will this alternative system for peer review threaten journal reputation or impact factors? This is an important question related to the prevalent culture and mindset of the community. Owing to the excessive importance given to bibliometric indices, publishing in a prestigious journal has become an endpoint in itself. This viewpoint is bound to change in the new system, although it may take some time. In the new system, publishing in a prestigious journal is not "winning." It is just a means to reach the reader. Therefore, the frenzy to publish in high-impact-factor journals is expected to decrease, if not vanish completely. Journals may feel threatened that the ultimate decision to publish will be made by the authors, but authors have little to gain by the journal name alone.

Journals may also feel threatened because this practice will affect their impact factors, and this possibility needs to be examined carefully. Impact factors are indirect indicators of journal quality. Ideally, a journal's quality should be mainly decided by the quality and rigor of its peer reviews. Until peer reviews were hidden, such indirect indicators were necessary. Once peer reviews become accessible, the journal quality can be directly assessed. Therefore, journals having healthy and rigorous peer reviews need not feel threatened. The journals manipulating bibliometric indices (Falagas, M.E., Alexiou 2008, Ioannidis and Thombs 2019, Juyal et al. 2019, Hickman et al. 2019) will certainly feel threatened. The indirect indicators cannot only be manipulated, they are also subject to processes similar to the Fisherian runaway process (Bartley 1994). Once the impact factor goes up, the chances of attracting better-quality papers increase, the chances of getting cited also are likely to go up, and the high index can be maintained by the positive feedback independent of review quality. In the long run, these indices are unlikely to reflect review quality, and therefore, as soon as the peer reviews are made public, other indices need to be abandoned or used with limited importance.

But even if we use the indices for saving the cost, there is another level at which journals can avoid losing their indices. In the suggested system, the journal editor assigns the grades. The grades will also reflect the quality standard of the journal. When authors use impact-factor-like indices in their profiles, their credit would be weighed by the grade given to each of the papers. Therefore, only publishing in a high-impact journal will not be equated to a triumphant achievement.

The grades can be used in a different way in calculating impact-factor-like indices for the journals. While calculating the average citations of papers published in a journal, averages weighted by the grades could be used. The citations of a paper with a grade of 1 will weigh 100 times more than one with a grade of 0.01. Owing to weighted averages, the journals can control their impact-factor-like quantitative indices by being careful in reviewing and grading manuscripts.

The ultimate global implication of the new system will be a relatively even dissemination of science throughout the globe and a reduction, if not elimination, of oligopoly. Some differences in the quality of different universities will remain, but the difference will be decided by their own quality of work. It would be possible to publish good work from any place. The unjustifiable difficulty of publishing good work from the third world would certainly reduce substantially. Then, good scientists would enjoy working in different places in the world and still do equally good work. Spreading science throughout the globe more or less equitably is likely to have desirable social and political consequences too, which should be the long-term goal of science. Today, one of the main hurdles in achieving this goal is the oligopoly in science publishing. Removing that is likely to be a major step in changing different dimensions of the world.

Of greater relevance is the availability of data on the science publishing process to meta-science researchers. Currently, whether there are biases in the publication system, the nature and extent of bias cannot be studied, mainly due to the confidentiality of the editorial process. In the new system, all steps can be transparent while protecting the rights to anonymity. As a result, all the data will be available to researchers in meta-science, history, and philosophy of science. When such data are available, our understanding of science would increase manyfold. Science does not grow by theorems, experiments, and data analysis alone. The behavior of science handlers is a large and inseparable component

of science. Our understanding of that will grow only when data about it is openly available.

Starting trouble in adopting the new system

If the system suggested above is to be accepted as a significant step in the right direction, it would still face difficulties in getting into practice. One possibility is that some editors would oppose the system since they have to be more responsible or they fear that their journal's reputation will suffer. It is notable that such opposition is not based on the fear of any undesirable outcome for science; it is based on the fear of losing an individual journal's reputation (Abbott 2023). This fear is unfounded since those who are already responsible are most likely to increase rather than decrease their reputation by an open peer review system. Therefore, committed editors are more likely to welcome the system, whereas opposition from careless editors is most likely.

Any journal adopting a new system might find it more difficult to get reviewers since reviewers have the right to reject the request, and the greater responsibility of the new system increases their reluctance. Prior studies haven't found increased reluctance among reviewers in open review systems (Rooyen 1999, Bravo 2019). Further, if institutions and universities accept the inclusion of published reviews in the evaluation of researchers, there will be sufficient incentive to accept open review invitations. However, this decision is not in the hands of journals that may think of starting the new system, and therefore there is likely to be a time delay before this can happen.

More likely is the opposition from the current political and power structure associated with science. The promise of a more equitable distribution of the benefits of science throughout the globe might be a threat to at least some of the power elements in the field. However, if supporters of fundamental science are strong enough, the power politics can be effectively countered.

While trying to predict the community's response to the suggested system, it needs to be recognized clearly that doing good quality science and having a successful career in science are two distinct and different objectives with some overlap but also some mutually contradictory elements. Bibliographic indices are not necessary for the pursuit of good science, but they have gained substantial importance in stereotyped research careers. The stereotyping of research careers can be potentially bad for the progress of science since, in the history of science, major breakthroughs have often come from outside mainstream academia. A very likely scenario is that individuals interested in science would welcome open peer reviews, but there will be resistance from elements contributing to stereotyped careers.

The major hurdle in the implementation of the proposed system is that it needs simultaneous change at several levels. The journals, science organizations, universities, and funding agencies all need to change eventually, and therefore the decision is not in the hands of a single agency. Who will begin and whether others will follow are the critical questions. In a very likely situation, an agency that begins the change will have to pay a higher cost for quite some time until the new system becomes the norm. There is a risk of getting isolated if other components are reluctant to change.

Nevertheless, there is one solution that authors can implement at their end. In the publishing game, authors against whom the biases work are at the receiving end. So, they should be the ones to take the lead in change, and they can certainly do so very effectively. Authors should start making all the review comments they receive public by posting them on preprint servers. At least some preprint servers have started accepting such posts. If authors are worried that this may affect the publication of their paper elsewhere, they can do it a little differently. That is, when any paper is accepted in any journal, they can publish the entire history of that paper, including previous rejections. If the paper has been rejected previously, the reviewers' and editors' comments recommending rejection, the comments during revision and acceptance, and the former and revised versions of the manuscript can all be made public by the authors themselves. This would expose unfair rejection decisions, if any. Then it is for the interested readers to judge whether the rejection was sufficiently responsible. If a critical mass of authors does this, editors and reviewers will understand that the cover of confidentiality is gone. Any irresponsible decision or bad review gets exposed. If decisions are getting exposed anyway, editors would have less reluctance to make the reviews public themselves. Once the chain begins, other changes will inevitably follow. Authors often think that rejection is unjustified. If they think so, they have no harm in making the entire correspondence public so that readers may ultimately be free to decide whether it was justified or not. If the authors are wrong, they will themselves get exposed. So authors cannot make unfair allegations against any journal. This step, if boldly taken by authors who think they have suffered some injustice, would facilitate the change. Ultimately, the greater the transparency, the

greater the benefit of the global science movement will be.

Conclusions

Since cost-benefit optimization is an innate human tendency, it will have its impact, consciously or subconsciously, on the peer review system as well. A number of biases can potentially arise through known principles of optimality theory. A system that minimizes conflicts between individual optima and community goals is a behaviorally sound system. Making academic systems behaviorally sound can potentially minimize research misconduct and other undesirable trends in the field of science. I suggest an alternative system of publishing research based on three principles, namely transparency of peer reviews, recognition of open review reports as a form of scholarly publication, and making the authors the ultimate decision-makers in publishing. Such a system alters the cost-benefit optima of different players in science publishing and is likely to minimize many of the flaws and biases. It is necessary to empirically test these concepts by experimenting with different models of science publishing and making the peer review data of all old and new models public for comparative analysis.

A change in the system is difficult, and although it is most likely to be welcomed by good editors, open-minded reviewers, and researchers from institutions outside the power centers of science, it is likely to be opposed by careless editors, irresponsible reviewers, institutions trying to cut down costs of evaluation, agencies confusing science quality with career success, and by power structures in science. However, if authors take the initiative and insist on open peer reviews, other desirable changes are likely to follow as a consequence.

References

- Abbott A. (2023). Strife at eLife: inside a journal's quest to upend science publishing. *Nature*, *615*(7954), 780–781. https://doi.org/10.1038/d41586-023-00831-6
- Allen, V.L. (1975). Social support for nonconformity. *Advances in Experimental Social Psychology,* 8, 1-43.
- Asch, E.S. (1955). Opinions and Social Pressure. *Scientific American.***193** (5), 31–35.
- Azoulay, P., Fons-Rosen, C., &Zivin, J. S. G. (2016). Does Science Advance One Funeral at a Time? *American Economic Review* 109, (8)
- Baron, R. S., Vandello, J. A., and Brunsman, B. (1996). The Forgotten Variable in Conformity Research: Impact of Task Importance on Social Influence. *Journal of Personality and Social Psychology*, **71**(5), 915–927.
- Bartley M. M. (1994). Conflicts in Human Progress: Sexual Selection and the Fisherian "Runaway." *The British Journal for the History of Science*, *27*(2), 177–196. http://www.jstor.org/stable/4027434
- Bateson, M., Nettle, D., & Roberts, G. (2006). Cues of being watched enhance cooperation in a real-world setting. *Biology letters*, *2*(3), 412–414. https://doi.org/10.1098/rsbl.2006.0509
- Bautista, L. M., Martin, B., Martinez, L., and Mayo, C. (2001). Risk-sensitive foraging in coal tits. *Behaviour*, 138, 69–83.
- Bohannon, J. (2016). Hate journal impact factors? New study gives you one more reason. Science DOI: 10.1126/science.aag0643
- Boutron, I., & Ravaud, P. (2018). Misrepresentation and distortion of research in biomedical literature. *Proceedings of the National Academy of Sciences of the United States of America*, *115*(11), 2613–2619. https://doi.org/10.1073/pnas.1710755115
- Bravo, G., Grimaldo, F., López-Iñesta, E., Mehmani, B., & Squazzoni, F. (2019). The effect of publishing peer review reports on referee behavior in five scholarly journals. *Nature communications*, *10*(1), 322. https://doi.org/10.1038/s41467-018-08250-2
- Brehm, J. W. (1956). Post decision changes in the desirability of alternatives. *Journal of Abnormal and Social Psychology*, **52**(3), 384–389.
- Brown R. J. C. (2006). Double Anonymity and the Peer Review Process. *TheScientificWorldJournal*, *6*, 1274–1277. https://doi.org/10.1100/tsw.2006.228
- Callaway, E. (2016). Beat it, impact factor. Publishing elite turn against controversial metric. *Nature***535**, 210–211.
- Campanario, J. M.: (1998) Peer Review for Journals as it Stands Today. part 1 and 2. In: *Science Communication* 19(3) pp. 181–211 and 19(4) 277–306.
- Chapman CA et al. 2019 Games academics play and their consequences: how authorship, h-index and journal impact factors are shaping the future of academia. Proc. R. Soc. B 286: 20192047
- Cho, M. K., Justice, A. C., Winker, M. A., Berlin, J. A., Waeckerle, J. F., Callaham, M. L., & Rennie, D. (1998). Masking author identity in peer review: what factors influence masking success? PEER Investigators. *JAMA*, *280*(3), 243–245. https://doi.org/10.1001/jama.280.3.243
- Chu, J. S. G., & Evans, J. A. (2021). Slowed canonical progress in large fields of science. *Proceedings of the National Academy of Sciences of the United States of*

America, *118*(41), e2021636118. https://doi.org/10.1073/pnas.2021636118
- Cushman, F. (2019). Rationalization is rational. *Forthcoming in Behavioral and Brain Sciences*, 1–69. doi:10.1017/s0140525x19001730.
- Doniol-Valcroze, T., Lesage, V., Giard, J., & Michaud, R. (2011). Optimal foraging theory predicts diving and feeding strategies of the largest marine predator. *Behavioral Ecology*, **22**(4), 880–888.
- Driscoll, C. (2009). On our best behavior: optimality models in human behavioral ecology. *Studies in History and Philosophy of Science,* **40**(2):133-41.
- Elliott S. (2016). Bad Science: Cause and Consequence. *Journal of pharmaceutical sciences*, *105*(4), 1358–1361. https://doi.org/10.1016/j.xphs.2016.01.002
- Else H. (2022). eLife won't reject papers once they are under review - what researchers think. *Nature*, 10.1038/d41586-022-03534-6. Advance online publication. https://doi.org/10.1038/d41586-022-03534-6
- Emerson, G. B., Warme, W. J., Wolf, F. M., Heckman, J. D., Brand, R. A., & Leopold, S. S. (2010). Testing for the presence of positive-outcome bias in peer review: a randomized controlled trial. *Archives of internal medicine*, *170*(21), 1934–1939. https://doi.org/10.1001/archinternmed.2010.406
- Etkin A, Gaston T, Roberts J. (2017) Peer review: Reform and renewal in scientific publishing. Available at: 10.3998/mpub.9944026
- Falagas, M.E., Alexiou, V.G. The top-ten in journal impact factor manipulation. *Arch. Immunol. Ther. Exp.* **56**, 223–226 (2008). https://doi.org/10.1007/s00005-008-0024-5
- Fender, C. M., & Stickney, L. T. (2016). When two heads aren't better than one: conformity in a group activity.*Management Teaching Review*, **2**(1), 35–46.
- Germar, M., Albrecht, T., Voss, A., &Mojzisch, A. (2016). Social conformity is due to biased stimulus processing: Electrophysiological and diffusion analyses. *Social Cognitive and Affective Neuroscience*, **11**(9), 1449–1459.
- Goldbeck-Wood S. (1999). Evidence on peer review-scientific quality control or smokescreen?. *BMJ (Clinical research ed.)*, *318*(7175), 44–45. https://doi.org/10.1136/bmj.318.7175.44
- Ha, T. (2010). Optimality Conditions for Several Types of Efficient Solutions of Set-Valued Optimization Problems. In: Pardalos P., Rassias T., Khan A. (eds) Nonlinear Analysis and Variational Problems. *Springer Optimization and Its Applications*, **35**. Springer, New York, NY
- Haffar, S., Bazerbachi, F., andMurad, M. H. (2019). Peer Review Bias: A Critical Review. *Mayo Clinic Proceedings*, **94**(4), 670–676.
- Harmon-Jones, E., & Harmon-Jones, C. (2007). Cognitive Dissonance Theory After 50 Years of Development. *ZeitschriftFürSozialpsychologie*, **38**(1), 7–16.
- Hesselmann, F., Graf, V., Schmidt, M., & Reinhart, M. (2017). The visibility of scientific misconduct: A review of the literature on retracted journal articles. *Current sociology. La Sociologie contemporaine*, *65*(6), 814–845. https://doi.org/10.1177/0011392116663807
- Hickman C.F. (20190 Academic misconduct and criminal liability: Manipulating academic journal impact factors, *Science and Public Policy*, Volume 46, Issue 5, 661–667, https://doi.org/10.1093/scipol/scz019
- Huber, J., Inoua, S., Kerschbamer, R., König-Kersting, C., Palan, S., & Smith, V. L. (2022). Nobel and novice: Author prominence affects peer review. *Proceedings of the National Academy of Sciences of the United States of America*, *119*(41), e2205779119. https://doi.org/10.1073/pnas.2205779119
- Ioannidis J. P. (2005). Why most published research findings are false. *PLoS medicine*, *2*(8), e124. https://doi.org/10.1371/journal.pmed.0020124
- Ioannidis, J. P. A., & Thombs, B. D. (2019). A user's guide to inflated and manipulated impact factors. *European journal of clinical investigation*, *49*(9), e13151. https://doi.org/10.1111/eci.13151
- Juyal, D., Thawani, V., Sayana, A., & Pal, S. (2019). Impact factor: Mutation, manipulation, and distortion. *Journal of family medicine and primary care*, *8*(11), 3475–3479. https://doi.org/10.4103/jfmpc.jfmpc_515_19
- Kelly J., Sadeghieh T. and Adeli, K. (2014) Peer Review in Scientific Publications: Benefits, Critiques, & A Survival Guide. *J. Int. Fed Clin Chem*25(3): 227–243
- Kirchherr, J. Bullshit in the Sustainability and Transitions Literature: a Provocation. *Circ.Econ.Sust.* **3**, 167–172 (2023). https://doi.org/10.1007/s43615-022-00175-9
- Klucharev, V., Hytönen, K., Rijpkema, M., Smidts, A., and Fernández, G. (2009). Reinforcement Learning Signal Predicts Social Conformity. *Neuron*, **61**(1), 140–151.
- Kuehn, B. M. (2017). Rooting out bias. *ELife*, **6**, 1–3.
- Kuhn, T. S. (1962). The structure of scientific revolutions. *Chicago: University of Chicago Press*, ISBN: 0-226-45803-2
- Lazarus, C., Haneef, R., Ravaud, P., Hopewell, S., Altman, D. G., & Boutron, I. (2016). Peer reviewers identified spin in manuscripts of nonrandomized studies assessing therapeutic interventions, but

their impact on spin in abstract conclusions was limited. *Journal of clinical epidemiology*, 77, 44–51. https://doi.org/10.1016/j.jclinepi.2016.04.012
- Lee, C. J., Sugimoto, C., Zhang, G., Cronin, B. (2013). Bias in peer review. *Journal of the American Society for information Science and Technology*, **64**(1):2–17.
- Mailman School of Public Health (2022) New journal seeks to reduce bias in scientific publishing. https://www.publichealth.columbia.edu/news/new-journal-seeks-reduce-bias-scientific-publishing
- McKiernan, E. C., Schimanski, L. A., Muñoz Nieves, C., Matthias, L., Niles, M. T., & Pablo Alperin, J. (2019). Use of the Journal Impact Factor in academic review, promotion, and tenure evaluations. *PeerJ Preprints*, 27638.
- Miłkowski, M., Hensel, W. M., & Hohol, M. (2018). Replicability or reproducibility? On the replication crisis in computational neuroscience and sharing only relevant detail. *Journal of computational neuroscience*, *45*(3), 163–172. https://doi.org/10.1007/s10827-018-0702-z
- Nettle, D., Andrews, C., & Bateson, M. (2017). Food insecurity as a driver of obesity in humans: The insurance hypothesis. *Behavioral and Brain Sciences*, **40**.
- Nickerson, R. S. (1998). Confirmation bias: A ubiquitous phenomenon in many guises. *Review of General Psychology*, **2**(2), 175–220.
- O'Connor, E. E., Cousar, M., Lentini, J. A., Castillo, M., Halm, K., & Zeffiro, T. A. (2017). Efficacy of Double-Blind Peer Review in an Imaging Subspecialty Journal. *AJNR. American journal of neuroradiology*, *38*(2), 230–235. https://doi.org/10.3174/ajnr.A5017
- Park, M., Leahey, E., & Funk, R. J. (2023). Papers and patents are becoming less disruptive over time. *Nature*, *613*(7942), 138–144. https://doi.org/10.1038/s41586-022-05543-x
- Parker, G. A., and Smith, J. M. (1990). Optimality theory in evolutionary biology. *Nature*, **348**(6296), 27–33.
- Phillips, J. S. (2011). Expert bias in peer review. *Current Medical Research and Opinion*, **27**(12), 2229–2233.
- Pierce, G., &Ollason, J. (1987) Eight Reasons Why Optimal Foraging Theory Is a Complete Waste of Time. *Oikos*, **49**(1), 111-117.
- Purshouse, R.C., and McAlister, J. (2013). Multi-objective Optimisation for Social Cost Benefit Analysis: An Allegory. In: Purshouse R.C., Fleming P.J., Fonseca C.M., Greco S., Shaw J. (eds) Evolutionary Multi-Criterion Optimization. EMO 2013. Lecture Notes in Computer Science, vol 7811. Springer, Berlin, Heidelberg.
- Radzvilas, M., De Pretis, F., Peden, W. *et al.* (2023) Incentives for Research Effort: An Evolutionary Model of Publication Markets with Double-Blind and Open Review. *Comput Econ* **61**, 1433–1476. https://doi.org/10.1007/s10614-022-10250-w
- Rahnev, D., Denison, R.N. (2018) Suboptimality in perceptual decision making.*Behavioral and Brain Sciences*,**41**, e223: 1–66.
- Sharot, T., Velasquez, C. M., and Dolan, R. J. (2011). UKPMC Funders Group Do decisions shape preference? Evidence from blind choice. *October*, **21**, 1231–1235.
- Shinde, S., Patwardhan, A. & Watve, M. (2022) The ratio versus difference optimization and its implications for optimality theory. *Evolution*, 76, 2272-2280. https://doi.org/10.1111/evo.14605
- Siew K. (2017) The open science movement: revolution is underway. Physiology news magazine, doi.org/10.36866/pn.107.24
- 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
- Squazzoni, F., Bravo, G., Farjam, M., Marusic, A., Mehmani, B., Willis, M., Birukou, A., Dondio, P., & Grimaldo, F. (2021). Peer review and gender bias: A study on 145 scholarly journals. *Science advances*, *7*(2), eabd0299. https://doi.org/10.1126/sciadv.abd0299
- Stephan, P. Perverse incentives. *Nature* **484**, 29–31 (2012). https://doi.org/10.1038/484029a
- Stephens, D. W., and Krebs, J. (1987) Foraging theory. *Princeton University Press*.
- Stevens, J. R. (2008). The Evolutionary Biology of Decision Making. Faculty *Publications, Department of Psychology*. http://digitalcommons.unl.edu/psychfacpub/523.
- Tancock, C. (2018). When reviewing goes wrong. The ugly side of peer review. *Elsevier connect* March 23, 2018.
- Thakar, J. D., Kunte, K., Chauhan, A. K., Watve, A. V., andWatve, M. G. (2003). Nectarless flowers: Ecological correlates and evolutionary stability. *Oecologia*, **136**(4), 565–570.
- Tomkins, A., Zhang, M., andHeavlin, W. D. (2017). Reviewer bias in single- versus double-blind peer review. *Proceedings of the National Academy of Sciences*, **114**(48), 12708–12713.
- Voelkl, B., & Würbel, H. (2016). Reproducibility Crisis: Are We Ignoring Reaction Norms?. *Trends in pharmacological sciences*, *37*(7), 509–510. https://doi.org/10.1016/j.tips.2016.05.003
- Wapman, K. H., Zhang, S., Clauset, A., & Larremore, D. B. (2022). Quantifying hierarchy and dynamics in

US faculty hiring and retention. *Nature*, *610*(7930), 120–127. https://doi.org/10.1038/s41586-022-05222-x
- Watve, M. (2017). Social behavioural epistemology and scientific publishing, *Journal of Genetics*, **96**(3), 525-533.
- Watve M. (2019)...but mangoes are out of syllabus. https://milindwatve.in/2019/11/25/but-mangoes-are-out-of-syllabus/
- Watve, M., Bayani, A., Ghosh, S. (2016). Crop damage by wild herbivores: Insights obtained from optimization models. *Current Science*, 111, 861-867.
- Watve, M., and Ojas, S. V. (2020). Difference, Division & Desi: How people's innate intuitive economics decides the outcome of an operation. *Economic and Political Weekly*, *55(8).*
- Yang, Y., Sánchez-Tójar, A., O'Dea, R. E., Noble, D. W. A., Koricheva, J., Jennions, M. D., Parker, T. H., Lagisz, M., & Nakagawa, S. (2023). Publication bias impacts on effect size, statistical power, and magnitude (Type M) and sign (Type S) errors in ecology and evolutionary biology. *BMC biology*, *21*(1), 71. https://doi.org/10.1186/s12915-022-01485-yZimring J. (2019) We are incentivizing bad science. Scientific American Oct 19, 2019.

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