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 benefits. 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, conformation and conformity will be increasingly common and revolutionary concepts 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 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 take an active role to revolutionize scientific publishing.

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Peer review and scientific publishing

Peer reviewing manuscripts is a recent norm in the history of science. Majority of journals started 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 has largely, if not entirely, deteriorated to support a dichotomous editorial decision of accepting or 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 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. Whenever there were specific and well-designed experimental or statistical tests for detecting biases in the peer review system, biases were invariably detected by every study (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 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, howsoever responsible and honest, cannot be expected to be free from this bias.

The response of the scientific community to any findings anomalous for 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 get 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 invites typically 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, peer review system is likely to have increased 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 psychology behind them understood (Watve 2017) and appropriate steps taken to minimize them.

In this paper I intend to analyse mainly how cost-benefit optimization by every player in the scientific publishing game is likely to influences 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 is known for over half a century that human decision making is not a straight forward 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 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. Not all reasons for a particular decision could be consciously known to the decision maker. 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 she herself is not consciously aware of. The second is that as the number of research organizations along with the number of researchers and the number of journals increase globally, it may be impractical to rely on the assumption of honesty and commitment entirely. 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 conscious commitment to the principles of science.

Cost-benefit optimization in human behaviour

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 about adaptation and innate behavioural tendencies (Parker & Smith 1990). Although there has been serious criticism on some aspects of optimality theory (Pierce & Ollason 1987) at a conceptual level, 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 context (Purshouse & McAlister 2013). Therefore, it would be reasonable to assume that even in scientific publishing, all actors do cost-benefit optimization in taking decisions, which may often be at a subconscious level.

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. Theoretically when the investment opportunities are limiting and not the investible amount, a difference model is appropriate and when the

investible amount and not the investment opportunities are limiting, a ratio model is appropriate (Watve *et al.* 2016; Watve & Ojas 2019, Shinde et al 2021). 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 chose 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 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 lay people is an important benefit sought after. A range of other benefits form a part of the system of working and the system of publishing. They include job prospects, tenure, power positions, successful publications, good citations of work, applause for a talk as well as direct monetary gains as pay scale, royalty 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.

- Editors: Editors are important decision makers in the publication process and although their decisions are guided by review reports, they can steer the process by selecting reviewers as well as taking the final decision. The cost they need to pay is mainly in terms of time, energy and intellectual inputs. The

benefits are variable depending upon the nature of the journal administration. In some systems the editors are employed as full time or part time editors and are paid and in a peer editor system editors are active researchers themselves. Some of the benefits may differ between the two but others are common. For any 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 become ratio optimizers and not difference optimizers. Minimizing the time and efforts in taking a decision is the best optimization strategy even at the cost of the accuracy of decision.

We 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 conservative editors may like to avoid it.

The other type of error, i.e. rejecting a 'good' paper has little punishment. I will avoid going into the definition of 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 does not have any reputation cost since the decision almost invariably remains confidential. Only the authors know about it 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 reducing it. There is a perceived positive correlation between the reputation of the institute from where 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 institutes, editor can certainly save time and energy cost 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 into 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. Manuscript from reputed authors or organizations cannot be accepted carelessly since accepting a 'bad' paper can carry serious penalty. The expected end result is that type A error (accepting a 'bad' manuscript) is less probable but type B error (rejecting a 'good' manuscript) can be extremely common. Experimental studies are compatible with this prediction. People are more likely to accept a manuscript from a reputed place for review 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, it 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 favour 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. The editor knows by experience more liberal versus more critical reviewers 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, editor's position is a power position and although it has a large associated cost, many researchers may be happy to be offered one.

- Reviewers: Reviewers are the most important players in the scientific publishing game. In the prevalent system of scientific publishing there is no direct benefit awarded for 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 the editors. Since reviewers remain anonymous most of the time, their reputation is rarely at stake. The cost incurred is in terms of time and efforts. Nevertheless, there are several indirect benefits to the reviewers. They can avail social and

political benefits such as building good relations with the editors who are likely to handle their manuscripts in 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 one in support. However, these benefits do not increase in proportion to the time and efforts spent in thorough reviewing. Therefore, for a reviewer maximization of 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 reputation is not at stake it is possible to do so and get away with it. With 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 on 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, in either case, reputation and impact factor like indices are extremely valuable for the 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 the trendy areas, the trend reinforces itself. This positive feedback process strengthens trends and waves and further blows up the importance of quantitative indices.

  Journal administration frequently has its own media cells that issues press notes on selected published papers that can make news headlines. Selection of papers for media coverage is decided by

its potential public appeal and sentimental value which is more likely to further reinforce a trend. Trends benefit publishers and publishers in turn are interested in strengthening the trends. This vicious cycle is expected to draw the field away from scientific concerns and get more sentimental and populist.

- Readers: The cost-benefits of readers are rapidly changing with increase in online publishing. The reader or his/her organization had to subscribe to a journal earlier. 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. The 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 reader's screening cost per article. On the other hand, readers' methods of searching articles also changes the cost-benefits of 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 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 ideally the relevance of choice of journal should vanish. But surprisingly the importance of indices like the impact factor 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 perceptional 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 maximising 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 success ensured projects 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 the funders need to be ratio optimizers and not difference optimizers. 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 than by difference. Ideally a researcher's potential and quality should be judged by reading his published work. But there is huge cost in reading. This cost 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 proposer's publications and the proposal itself. In systems where the experts need to give justifications for their short listing, they need to read something at least to enable them 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/Institutes: Organizations that support researchers pay a huge cost in employing the researchers and providing necessary infrastructure. But this they do 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 choice of the researchers and also monitor their output. Most organizations have a system of evaluation of researchers at the time of selection as well as periodically through 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 h index would have rapidly become irrelevant to researcher

evaluation. But with 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 favourable benefit-cost ratio than 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 would continue to retain their importance because of behavioural reasons if not for scientific reasons. Since they alter the cost-benefits of evaluation favourably to the evaluator, it should be accepted that they will continue to be used although they are completely unscientific. Science will have to accept the increasingly unscientific component in the present academic structure.

- Researchers/Authors: Since reportable scientific findings are more likely to be limiting than the time required for writing, researchers are most likely to be difference optimizers. Researchers' investment per publication is maximum among all the players and includes time, energy, efforts, intellectual inputs and at times their own money. Online publishing and author charges have added to the costs to 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 royalty or indirect such as research grants crucially depend upon 'good' publications. Since most persons involved in deciding a researcher's career path cannot afford to pay the 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 the real merit may be given an 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 matter a lot, apart from doing high quality science the 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 being a trend setter. Out of the box ideas often meet with more reluctant response, scepticism or outright disbelief and it is a smarter strategy to avoid them. There appears to be an optimum level of novelty for a piece of work to receive appreciation. High level of novelty has a high cost and more unpredictable social benefit. Although the intellectual satisfaction as a benefit may be very high, individuals relying substantially on social 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.

As a part of the optimization strategy most researchers seek positions in more reputed institutes or universities. It is easier to publish from a more reputed institute since that is an important surrogate in editor and reviewers decisions. It is also easier to attract funding. Therefore attempt to get positions in an organization with better reputation is a part of strategic optimization for any researcher. Position in reputed institutes 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 that 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 which Thomas Kuhn (1962) had not foreseen. (iii) The third important consequence 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. All fields of science do not need huge amount 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 cycles. 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 reputation of institutes 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 the 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 the light of such studies, designing more open and unbiased science publication policy 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.

A behaviour-based alternative system

The problems with the current system are often recognized, discussed and some alternative experimental systems are under trial. 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 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. The reviewers spend substantial efforts in imagining the author identity (Kuehn 2017) which may worsen the quality of review further. Furthermore the growing pre-print trend directly hampers the 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 the best be pretence, or a smokescreen.

On the other hand, a number of experiments are being performed on open peer reviews (Ross-Hellauer 2017, Else 2022, Mailman School of Public Health 2022) although not without a debate (Abbott 2023). In some journals reviewers' comments and author responses are made public, but only for accepted papers. By our cost-benefit analysis biases are more likely with the rejection decision and therefore making the reviews of rejected manuscripts public is more relevant. The alternative systems by Journals such as eLife or Qeios are welcome as experiments although they may be still far from achieving a minimum bias review system.

Although exploration and such experimentation on alternative review systems are welcome, they have limited success so far and haven't yet replaced main stream significantly. There are two possible reasons for the limited success of the alternative publishing systems. One is that they are not designed based on the principles of human behaviour. Systems with harmony in every player's cost-benefit optimum would work with minimum hick-ups and maximum fairness. But such systems haven't been explored as yet. The other being that it may be against the interest of the current power structure of science and therefore the established systems are reluctant to change (Abbott 2023). We can nevertheless think of whether we can design a system optimizing cost benefits of all parties and still reducing type A and type B errors so as to facilitate the progress of science.

Since good quality reviews are most crucial to scientific publishing, the cost-benefit optimization of the reviewer should be the first consideration in designing an alternative publishing system. For which it is necessary to give sufficient incentive for reviewing as well as allow reviewers to build 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 with 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 as 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 or not 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 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 of reviewers motivating them towards greater efforts, greater quality and timely inputs.

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 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 implementation difficulties. Therefore open peer reviews will have to be the norm in near future. The debate can be about the form and the modalities by which peer reviews can be made public.

It is easy to visualize publication of review reports for an accepted paper. How to publish reviews of rejected manuscripts? There are two solutions to the problem. One is that reviews would 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 as well as to the reviewers. 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 rejection system. Only a few journals have spared 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. 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

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 publication declares the grade right at the top. 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 author should be used instead of the journal impact factor alone. So publishing in a high reputed journal with a near zero grade may be bad for the authors and they may go for reworking and resubmitting. This would prevent a lot of junk from being published. It is likely that the reviewers' comments and the grade is unfair. In such a case the authors should be free to choose to publish even with a low grade score if they are confident about their work and think that the comments and the grades are unfair. Since everything is transparent the reader is open to judge the fairness of 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 have access to both 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. But 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 simply does not get published. In the new system it would certainly get published, may be with not so 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. Just that they have to understand that their paper will get published along with the comments and grade offered.

The possible objection for such a system is that already the volume of literature being published is 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 not very serious. Experimental psychology has demonstrated that just having the possibility that others could be watching you is sufficient to alter human behaviour (Bateson et al 2006). It is not necessary to be actually being watched. The possibility that whatever you write is being made public is sufficient to make the reviewers more responsible about the quality of review report. It is not necessary that the readers read them very frequently. 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 pulished, 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, rigorous but open minded and polite will only prove to be a good strategy for the reviewer.

If authors decide to retract the paper from a given journal owing to low grade 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 zero grade score by the editors of the previous journal, and the paper happens to receive some importance later, the decision of editors of 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 communicated manuscripts and it would be impossible to review all. To prevent this, 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 reviewer's name

iii. Authors to 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 also would have to be made carefully. Reputation damage by type B error would still not be as large as that by type A error, but it will be some non-zero positive as compared to the near zero of the current system. So the symmetry will not be perfect but better than what it was earlier.

The system aims to make the entire peer review process transparent but leaving the choice of anonymity with the reviewers and the choice of publishing or not publishing in the journal with the given grade and given comments with the authors. This will substantially reduce the biases and irresponsible behaviour of any player. Any residual irresponsible behaviour will get exposed on which readers can exert their ultimate judgment. Since publication is ultimately accessible for all authors, the gender and place of work bias will be removed from publication decision. It may still be there in the grading, but that too is expected to reduce since accountability in reviews and editorial decisions will increase.

Will it threaten the journal reputation or its impact factors? Journals may feel threatened by the feature that the ultimate decision to publish will be made by the authors. However, there are two possible solutions to this built in the system itself. One is that the journal editor assigns the grades. The other is that while calculating impact factor like indices the citation data would be weighed by the grade given to each of the papers. The calculation of indices would be a little more complex but the citation data of a paper with a grade of 1 will weigh 100 times more than one with a grade 0.01. Therefore the journals can control their impact factor like quantitative indices by being careful in reviewing and grading manuscripts. Simultaneously qualitatively the journal reputation will be decided by the readers based on review quality and grading judgment. This is precisely what a journal quality should mean.

The ultimate global implication of the new system will be a relatively even dissemination of science throughout the globe and 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 available 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 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 many fold. Science grows not by theorems, experiments and data analysis alone, human behaviour of science handlers is a large and inseparable component of science. Our understanding of that will grow only when data about it comes in the public domain.

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 editors would oppose the system since they have to be more responsible or they fear that their journal reputation will suffer (Abbott 2023). But it is obvious that editors who are already responsible would increase their reputation by open peer review system. 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. If institutions and universities accept inclusion of published reviews in the evaluation of researchers, there will be sufficient incentive to accept review invitations. But this decision is not in the hands of journals who may think of starting the new system.

More likely is the opposition from the current political and power structure associated with science. The promise of more equitable distribution of 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.

The major hurdle in implementation of all components of the proposed system is that it needs simultaneous change at several levels. The journals, science organizations, Universities, 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 is the critical question. 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 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, 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 understand that the cover of confidentiality is gone. Any irresponsible decision or bad review gets exposed. This kind of exposure is sufficient to reduce type B error. If decisions are getting exposed anyway, editors would have less reluctance to make the correspondence public themselves. Once the chain begins other changes will follow inevitably. 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 greater the transparency, greater will be the benefit of the global science movement.

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