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RESEARCH ARTICLE

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Social behavioural epistemology and the scientific community

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Abstract. The progress of science is influenced substantially by social behaviour of and social interactions within the scientific community. Similar to innovations in primate groups, the social acceptance of an innovation depends not only upon the relevance of the innovation but also on the social dominance and connectedness of the innovator. There are a number of parallels between many well-known phenomena in behavioural evolution and various behavioural traits observed in the scientific community. It would be useful, therefore, to use principles of behavioural evolution as hypotheses to study the social behaviour of the scientific community. I argue in this paper that a systematic study of social behavioural epistemology is likely to boost the progress of science by addressing several prevalent biases and other problems in scientific communication and by facilitating appropriate acceptance/rejection of novel concepts.

Keywords. epistemology; paradigm shift; peer review bias; evolutionary psychology.

Introduction

One of the unique attributes of the human species is the ability and propensity to ask questions, understand the world around, generate and propagate knowledge. 'Science' constitutes the most important and most successful methodology of knowledge generation and propagation. Although there is a set of 'ideal' principles of science, it is handled by the human mind and therefore many aspects of human behaviour interact with principles of science in a complex way. I use the words 'social behavioural epistemology' (SBE) here to indicate any attempt to address the question of how the process of knowledge generation and diffusion is influenced by social behaviour of the knowledge handlers and beneficiaries, and which principles of social behaviour are relevant to this process. My focus in this article is to ask whether we will be able to understand science better by considering social behaviour of science handlers and, further, whether science can progress better by applying SBE. It is well accepted that certain driving principles behind science such as curiosity, collective intelligence and desire to have control over some phenomenon are innate to human nature. Here, I intend to address those aspects of human nature and human behaviour which prevent, interfere, or slow down the progress of science.

At an experiential level, most scientists understand and admit that human nature and human behaviour play a significant role in the progress of science. But this almost always remains anecdotal and subjective. With a few exceptions, these questions are rarely systematically studied by the mainstream scientific community and the studies are rarely used to improve the structure and working of the scientific community. Some studies address specific problems such as biases in the peer review process (Phillips 2011; Lee *et al.* 2013) but the theoretical basis, methodology and implications of these studies are limited. It is generally agreed that the peer review process, although essential to ensure quality of scientific publishing, faces a number of challenges. A variety of sources of biases and unfairness in the process are recognized, discussed and sometimes statistically tested (Peters and Ceci 1982; Walker *et al.* 2015; Trinquart *et al.* 2016). But why such biases exist is a behavioural question, which has largely remained unaddressed.

There are three main contributors to human behaviour, particularly in the context of science (i) evolved

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psychological mechanisms, (ii) cultural learning; and (iii) formal education. All of them are likely to have some relevance to the behaviour of the scientific community and I make an attempt here to list and exemplify at least some of the behavioural principles that are likely to affect both the review process prior to publication and the reception of a published paper by the scientific community particularly if it has results or arguments against the currently held paradigm.

In this article, I focus on two levels at which novel concepts and findings are resisted. One is at the peer review process and the other is the community's reactions to a published finding that contradicts an existing paradigm. In the first section, I illustrate with an example, how researchers react to nonconformist findings in the light of Popper and Kuhn's ideas. In the second section, I suggest which behavioural principles are likely explanations of the observed modal behaviours of researchers. Towards the end, I make suggestions on how the problem can be studied and mitigated.

Popper, Kuhn and type 2 diabetes

Karl Popper and Thomas Kuhn are icons of two contrasting but equally important components of the scientific process. Popper epitomizes the ideal structure of scientific investigation and theorization. The Kuhnian philosophy (Kuhn 1962) on the other hand says that, in reality, science does not take the ideal path. The behaviour of the scientific community matters in accepting or rejecting revolutionary thoughts, hypotheses as well as evidence. Being logically and mathematically sound and also being supported by evidence is not sufficient for a new principle to be accepted by the scientific community. If it implies a radical change in the existing way of thinking, the new principle faces utmost resistance. The process of such a 'paradigm shift' is often painful and takes a tortuous path. What is more interesting is Kuhn's description of a state he calls 'crisis' that precedes a paradigm shift. In a crisis state, several anomalies in the prevalent thinking have been demonstrated, but there are no norms as to when an existing theory is refuted by the scientific community and a new one replaces it. This contrasts Popper's utopian principles based on falsifiability.

Kuhn stopped at observing how the scientific community behaves when faced with a crisis in any field of science. He did not seem to have analysed the origins and mechanisms of this behaviour. Perhaps this question can be better addressed by a behavioural biologist. Two limitations of Kuhn's approach can be readily appreciated. First, that almost all his examples come from the history of physics. It is likely that while facing a crisis, a biologists' community might differ grossly or subtly in its behaviour from a physicists' community. But currently there are no data across different disciplines. The second limitation is that he made his inferences based on examples from the history of science. Much detail of reality is lost over time and a historical study is limited by whatever is available in writing. Studying the present has the advantage that the researcher can witness the process, interact with the stakeholders and also attempt to predict the future and test the predictions. Instead of a retrospective analysis of history it would be much more enlightening if a crisis followed by paradigm shift could be studied prospectively. The difficulty in this approach is that most paradigm shifts are realized much after they have happened. By Kuhnian principles itself, it might be difficult to clearly predict a paradigm shift so that it can be studied in the process. Nevertheless trying to predict a paradigm shift from the symptoms of crisis would make Kuhn's analysis empirically testable and is, therefore, of great value to the philosophy of science. If the behaviour of people across disciplines have common underlying elements, and if one finds predictability in paradigm shifts, the usefulness of Kuhn's principles would increase substantially.

I will take an example from present day biomedicine so that we have the advantage of witnessing the process, as well as see whether Kuhn's observations turn out to be true in the biomedicine research community as well. The example I have chosen is the classical theory of type 2 diabetes mellitus (T2DM) which exists as a text book theory for over five decades. For the convenience of a non-biomedicine reader, I will briefly summarize the current textbook understanding of T2DM. A series of five statements that briefly capture the classical theory of T2DM (Watve 2013) is (i) a positive energy balance caused by modern diet and lifestyle leads to obesity, (ii) obesity gives rise to insulin resistance (lower than normal response of insulin dependent tissues to insulin), (iii) the body tries to compensate for insulin resistance by producing more insulin, resulting in a hyperinsulinemic normoglycemic (prediabetic) state, (iv) in the long run the insulin producing beta cells of the pancreas become dysfunctional owing to exhaustion or some other cause and produce inadequate quantities of insulin; this inability to compensate insulin resistance leads to hyperglycemia, and (v) chronically raised blood glucose causes a number of complications.

A number of experiments have challenged the classical picture. I will briefly relate only three out of the many sets of challenging experiments. The statement (iii) above that hyperinsulinaemia compensates insulin resistance is based only on correlational evidence. Whenever insulin resistance was demonstrated in a normoglycemic individual, it was accompanied by high levels of insulin. This has been labelled a horse and cart paradox (Shanik *et al.* 2008; Watve 2013). A simple experimental solution to a horse and cart paradox is to remove the horse and see whether the cart still moves and vice-versa. When insulin receptors from muscle tissue (which is responsible for about 70% of insulin dependent glucose uptake) were knocked out in order to ensure primary insulin resistance, rise in insulin levels was not observed (Kim *et al.* 2000). On the other hand insulinomas that produce large quantities of insulin are always accompanied by insulin resistance and removal of the tumour rapidly brings down insulin resistance (Gin *et al.* 1987, 1998). These experiments along with much other evidence (Shanik *et al.* 2008; Corkey 2012; Pories and Dohm 2012; Watve 2013; Mehran *et al.* 2014) indicate that hyperinsulineamia is not a compensatory response to insulin resistance, insulin resistance might be a compensatory response to hyperinsulineamia. If hyperinsulineamia is not a compensatory response then failure of compensation cannot be the reason for hyperglycemia of T2DM.

In another class of experiments done in rodents as well as humans, insulin production was experimentally suppressed in an insulin resistant hyperinsulinemic state. According to the classical theory this should lead to hyperglycemia. In all the insulin suppression experiments, immediately after lowering insulin levels, insulin resistance reduced on its own and glucose remained normal (Alemzadeh *et al.* 2002; Lustig *et al.* 2003; Velasquez-Mieyer *et al.* 2003; Alemzadeh and Tushaus 2004; Schreuder *et al.* 2005; Lustig *et al.* 2006; Hwang *et al.* 2007; Alemzadeh *et al.* 2008). This was true across independent experiments by different research groups using different agents to suppress insulin production and different model systems. On the other hand, studies on insulin degrading enzyme mutants consistently maintained a high level of insulin but did not show a decreased glucose level, some studies actually showing higher levels of glucose (Farris *et al.* 2003). These experiments clearly falsify statement (d) of the above sequence.

Many large scale clinical trials tried to achieve aggressive normalization of glucose levels and expected substantial reduction in the risk of complications. The findings were that some of the complications and total mortality actually increased on normalization of glucose (Cefalu and Watson 2008). A few other complications showed a marginal decrease. So far there has been no demonstration that bringing back glucose levels to normal can eliminate diabetic complications.

These are only a few examples from the large body of experimental results and epidemiological patterns which falsify or at least challenge the classical theory of type 2 diabetes. All the experiments have been independently reproduced by different research groups with converging results. Papers publishing these results did not receive any criticism of the experimental designs or inferential logic. The anomalies exposed by these studies remained unresolved. However, barring a few exceptions (Corkey 2012; Pories and Dohm 2012) that challenge some of the assumptions, the paradigm itself is neither refuted nor questioned. The burden of anomalies on the current paradigm combined with the reluctance to recognize it characterizes a 'crisis' state described by Kuhn. Therefore I feel confident in predicting that a paradigm shift is overdue in the field of T2DM.

But how does the diabetes researcher community react to the 'crisis'? How many of them feel that there is a crisis? If they do not feel that there is a crisis, how do they perceive the anomalies? We did an online survey in which over 4000 diabetes researchers all over the world were requested by email to participate in the study. The questionnaire consisted of quoting a set of published papers giving falsifying evidence and thereby challenging the classical T2DM paradigm. The questionnaire asked the respondents whether they were aware of this work and what their responses to it were (see electronic supplementary material at http://www.ias.ac.in/jgenet/ for the questionnaire used). 5% of the requested researchers responded. To our amazement, most respondents were simply unaware that such experiments had been done in spite of the papers being published in peer reviewed journals with good readership in the field. After being requested to go through these papers, 37% accepted that there was some anomaly but only 3% felt that there was a need to address the anomaly; 18% denied that there was any anomaly and another 18% thought that it could be explained in the current paradigm (figure 1). The remaining gave a confused and self-contradictory response. On asking whether the anomalies demand some revision of the theory, 37% said yes but only 11% thought that a paradigm shift was needed. Interestingly, of those who did not feel a need for change, nobody felt a need to challenge the experiments or seek sound alternative inferences. They rather preferred to ignore them. In literature on T2DM research papers publishing anomalies such as the insulin suppression experiments are among the least cited. It is interesting to note that rather than finding flaws in the experimental design, trying to test reproducibility of the results or examining whether alternative interpretations are possible, the research community has preferred to ignore the contributions giving inconvenient evidence.

With the above data, and one to one conversations with many diabetes researchers, a number of excuses for overlooking contradictory findings were exposed. (i) The most common response was to ignore and/or pretend to be unaware of the published study. (ii) If made to read and react, the preferred reaction was to say that something must be wrong with these experiments. Very rarely was this accompanied by a careful look to see what could be wrong. (iii) A further minority tried to make an interpretation in compliance with the existing paradigm even if it involved far stretched explanations (figure 2). (iv) Surreptitious skipping between subhypotheses was a more sophisticated response. It is a prevalent belief that obesity induces insulin resistance. A number of hypotheses have been proposed for the mechanism by which this happens (reviewed by Watve 2013). However every mechanism suggested has one or more flaws. The defenders of obesity

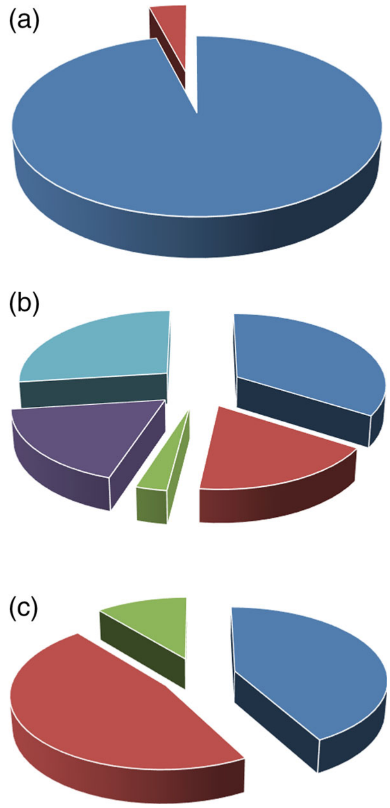

Figure 1. Responses of a sample of diabetes researchers to published experimental work exposing anomalies in the prevalent theory. (a) Before starting with the questionnaire, 96% respondents (in blue) were not aware of the published work exposing anomalies. (b) Responses after pointing out the experiments. Note that only 34% (in blue) acknowledge that there is anomaly, 18% (in red) try to explain it in the current paradigm, 27% (in cyan) have self-contradictory views, 3% (in green) acknowledge the need to address it, while 18% (in violet) deny the anomaly. (c) Only 11% (in green) think that a paradigm shift is needed whereas 47% (in red) feel that there is something missing but are not sure, and 42% (in blue) think that the current situation is satisfactory.

induced insulin resistance theory make use of the alternative hypotheses as per convenience of time. Whenever one hypothesis is confronted by a serious problem, another one is used for support. When that faces serious trouble, a third one is used and often when the third also fails to stand, the first one is called again to the rescue. (v) If this strategy also does not work, complexity of the system is used as an excuse to subvert logical explanation. The rarest response to anomalies we witnessed in this survey and personal interactions was to address the flaws or anomalies head-on. A head-on approach could consist of either challenging the experimental design or inference, being prepared to test their reproducibility, or challenging the existing paradigm itself. All the three were uncommon responses in the sample. These observations are compatible with Kuhn's description of researchers' behaviour in crisis, although the proportions might be different. The modal response in the biologist community that we sampled was to ignore them or pretend not to be aware. Kuhn's description of crisis talks more about the tendency to interpret the anomalies within the current paradigm. If the survey is considered representatives of biomedicine researchers, it is possible that on facing anomalies, biologists differ from physicists in the preferred response.

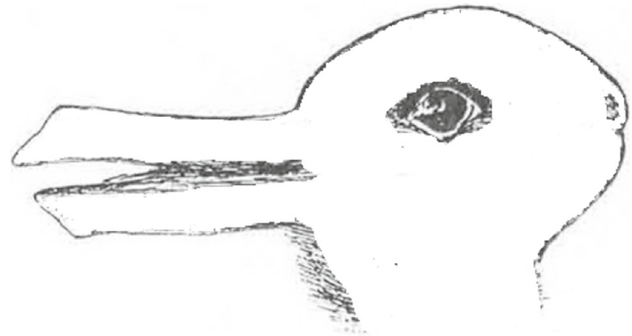

Figure 2. The duck–rabbit analogy. The picture can be interpreted either as a duck or rabbit. If an individual who subscribes to the duck theory happens to examine the 'beak' and finds it very soft and pliable, the first response is not to tell anyone and pretend that you never knew the beak was soft. But if that was not possible the next option is to infer that 'it is a duck with an unusually soft beak' rather than doubting that it is duck.

How do uneducated persons treat evidence falsifying their current beliefs? Anthropologists describe several occasions in which hunter gatherers use inferential logic following Popperian principles (Lee and DeVore 1976; Flavell 1977). For example, a small band of Kalahari bushmen hunters finds antelope hoof marks early in the morning which look very fresh. Thinking that the animals must be close by, they start following the trail. However after a while they suddenly decide to give up the chase. When later asked by the anthropologists they say that they saw tiny foot prints of a field rat overrunning the antelope marks. Since these rats are nocturnal, the antelope marks must be yesterdays. They could not be quite fresh and therefore it was worthless following the trail. This is a Popperian process of scientific logic. They made an observation which gave rise to a plausible hypothesis. They followed the hypothesis until there was falsifying evidence. The moment they found falsifying evidence, they abandoned the hypothesis and stopped all actions founded on the hypothesis. Many such examples demonstrate that hunter gatherers have the ability and propensity to follow scientific logic. Against this background, it is even more important to investigate what prevents the highly trained biomedicine research community to forget the Popperian principles and refuse to give up a hypothesis in spite of finding reproducible falsifying evidence.

Unscientific behaviour of the scientific community has indeed been recognized and criticized repeatedly. For example 'Semmelweis reflex' is a phrase in use that refers to our reflex-like tendency to reject new evidence or new knowledge because it contradicts our established norms, beliefs or paradigms. Ignaz Semmelweis was a Hungarian obstetrician of the pregerm-theory era of medicine. Even in the absence of germ theory he recognized the importance of sterilization and disinfection in surgery and advocated it to other surgeons and obstetricians. However, not only were his ideas rejected, but he was also ultimately beaten to death. There is substantial literature on Semmelweis reflex in general and in the field of medicine in particular. The French philosopher Voltaire, e.g. comments 'It is dangerous to be right in matters on which the established authorities are wrong' (Voltaire 1752). Psychiatrist Thomas Szasz remarks 'It can be dangerous to be wrong, but, to be right, when society regards the majority's falsehood as truth, could be fatal. In the past, such basic false truths were religious in nature. In the modern world, they are medical' (Szasz 2010). One of the main behavioural elements in rejecting a new idea is that it amounts to admitting that 'I was wrong'. This too is noted much prior to Kuhn by Max Plank, who remarked 'A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it'. Azoulay *et al.* (2015) gave data that appears to support Planks's remark. They showed using published work that in several fields of science, studies that support alternative thinking are published only after the death of an elite scientist in the field, and they are often published in low impact factor journals.

The reactions revealed by our survey were in the context of papers that were already published in peer reviewed journals. Even more serious resistance is likely to be faced by authors for publishing their work if the results expose some anomaly in currently accepted theories. The difficulty anticonformist results might face in getting published is anybody's guess since data on rejected papers are not generally available (Luty *et al.* 2009).

Possible behavioural principles behind the observed responses

Biases in the publishing and communication processes are increasingly being recognized by the researcher community. A number of alterations in the review process including double blinding and open peer review have been tried to reduce the bias with various levels of successes and failures (Kravitz *et al.* 2010; Kravitz and Baker 2011). However there is inadequate discussion on the behavioural principles behind the reviewer bias. Occasionally it has been recognized that many of the biases are intrinsic and subconscious (Bargh and Williams 2006; Pinholster 2016) and even the most honest reviewer may not be free of biases. I will attempt below, a few hypotheses about the origins of the biases in review as well as in reception of a study after publication.

I am listing here some of the well-known phenomena of ethology that have parallels to the behaviour of the scientific community. Finding a parallel is not a proof of the same principle being at work. Currently they can only be treated as hypotheses, but they are certainly testable if data about the review processes of many journals (including rejected manuscripts) are made public and well-designed experiments with appropriate blinding are performed.

- One behavioural principle relevant to the review process is the sequence in which the human mind works while making a decision. The popular perception that humans take decisions by conscious thinking was challenged by many experiments. Two celebrated experiments by Libet (1985) and Bateson *et al.* (2006) demonstrated that action decisions are taken before or independent of conscious thinking. Libet (1985) showed that simple motor actions are initiated a few hundred milliseconds before a person becomes consciously aware of the urge to act. Thus, the decision to act was taken before the conscious thought of acting. The second experiment demonstrated that honesty or cheating is affected by the presence of pictures prominently showing eyes, although at a conscious level everyone knew that it was only a picture and nobody was really watching them act (Bateson *et al.* 2006). This demonstrates that the eye stimulus affected behaviour independent of conscious knowledge. In line of these experiments an argument emerged that consciousness is an epiphenomenon, one that may accompany a decision but does not cause the decision. The extreme view that conscious thinking has no role in decision making is debated (Baumeister *et al.* 2011) but it is clear that much of the decision making happens at a subconscious level. These experiments have possible implications for the review process. It is likely that during a peer review process the decision to accept or reject a manuscript is taken by the reviewer's mind before consciously perceiving the reasons for the decision. A number of factors contribute to the decision including the place from where the manuscript comes, reviewer's previous experience with the authors if any, authors' reputation, financial and ideological conflicts of interest and whether the manuscripts supports or opposes the prevalent paradigm or the reviewer's beliefs (Lee *et al.* 2013). The review report does not make any of these reasons explicit. A different set of justifications is then sought to support the acceptance or rejection. The 'justifications' are projected and pretended to be the 'reasons' for rejection. While the justifications are made explicit in the review reports, the reasons remain implicit. Some such implicit biases are revealed by statistical analysis of review reports (Kravitz *et al.* 2010) but much remain hidden due to confidentiality of most of the data. This does not mean that reviewers are dishonest. It is in human nature that a substantial part of the decision making process happens at the subconscious level, which is true for even the most conscientious reviewer. Therefore greater care is needed to remove the implicit biases in the process.

- Innovation and innovation diffusion in primates: Innovation in many primate species has been studied extensively and shown to be tightly intertwined with social structure. In chimpanzees, low-ranking individuals innovate more often, but these innovations are less likely to spread in the society. Acts by dominant or more socially connected individuals are copied more frequently than those by subordinates, younger or less connected individuals (Kendal *et al.* 2015; Hopper 2016). Thus the spread of an innovation is decided more by social factors than by the usefulness of the innovation alone. This pattern appears to be at least partly inherited by the scientific community. This is supported by the statistics of publications of innovative concepts which suggest that dominant personalities are often the obstacle in publication of novel ideas and the demise of celebrities in specific fields is often followed by rise of novel ideas in that field from a different set of researchers (Azoulay *et al.* 2015).
- Xenophobia: Most species of group living animals show fear, dislike or aggression towards an individual that does not belong to their social group. Xenophobia of evolutionary origin is implicated in explaining war and violence in humans (Thayler 2015) where ethnicity, country or religion is the group identifier. However, at a more subtle level, there can be many different group identifiers and accordingly a variety of ways of responding to one's group identity. In scientific literature it appears to work in a more subtle way and rather than aggression, omission seems to be the most common response. Classical psychology literature, for example, rarely cites evolutionary psychology papers although they might be talking about the same issue. In areas where there are two schools of thoughts, researchers of one school avoid citing work from other school. In the debate about the role of salt in hypertension, Trinquart *et al.* (2016) illustrate with data the selective connectivity of researchers in the field.
- Cost-benefit optimization: Optimization of cost benefit is an important concept in evolution of behavioural strategies and optimization models are used in modelling both animal and human behaviour (Foley 1985; Parker and Maynard 1990). Optimization in the context of complex and stochastic challenges by organisms with primitive brains (Ellers *et al.* 2000) suggests that much of the optimization calculations could be independent of conscious thinking.

It is highly likely therefore that a researcher does a subconscious cost-benefit calculation while taking any decision such as accepting or rejecting one's own observation, experimental result or hypothesis as well as reacting to others' ideas or findings. The currency used for estimating cost benefits is complex but certainly includes social costs, reputation, acceptability by others, funding, competition and rivalry among researchers, tenure and promotions among other things. The weighting given to the different dimensions may change from person to person. The important thing to realize is that such cost-benefit calculation is an integral part of any decision making process. Standard publication process does recognize certain components of the cost benefits such as financial conflicts of interests and journals in some fields make it mandatory to declare conflicts of interests. However, it needs to be realized that there are other more subtle conflicts of interest that may affect the decision making process.

'Streetlight effect' is a good example of cost-benefit optimization with the wrong currency. A conversation 'Did You Lose the Keys Here?' 'No, But the Light Is Much Better Here' captures the phenomenon of 'search under the lamp' or 'streetlight effect'. I once asked a senior R. and D. director of a reputed pharma company, why practically nobody was focussing on brain and neuronal mechanisms in glucose regulation with reference to type 2 diabetes. He replied very honestly that he was very much aware that the brain has a central role in glucose regulation. But he confessed that there are a large number of problems in drug discovery for a brain target. Peripheral targets are relatively easier and they could get quicker results with peripheral targets. These researchers were certainly aware that they were not targeting the root cause of the disease, but that was not a grave concern since the benefit was not measured in terms of long term relief to patients. The benefit was being measured in terms of getting a few quick patents and getting a new drug into the market.

It is an extremely important question, therefore, as to how working scientists calculate their cost benefits. Benefits can be measured differently in different fields of research, in pure versus applied science as well as in public versus private funded research. Although the tendency as well as ability to optimize cost benefits might be innate to animal and human minds, attaching weights to different types of costs and benefits can potentially be modified by culture and education and this can have a large impact on the progress of science.

- Territoriality and territorial settlement: A number of animal species show territorial behaviour in which they defend a territory from other conspecifics. The evolution of territoriality (Maynard 1974) and behaviours associated with it (Potts and Lewis 2014; Giuggioli *et al.* 2011) are complex. For example there is a 'prior residency effect' which influences the distribution of territories substantially (Lindeman and Grant 2012). Individuals appear to settle their territories based on a complex set of social and behavioural parameters and settling territories can reduce physical aggression substantially.

There are a number of parallels between the areas of research and comfort zones of researchers, and territories of animals with respect to the behaviours associated with it. Resisting unfamiliar intruders in one's territory as well as settling territories to avoid disputes is something that commonly happens in the field of research. While the behavioural aspects of territoriality and territorial settlements are well studied in several species, behaviour of researchers with respect to their areas of work has received little research inputs and is an interesting hypothesis and a potential challenge to evolutionary psychology.

Just as animals avoid entering others' territories and are resisted if they do so, researchers form narrow comfort zones and avoid getting into other fields. For a molecule such as insulin which has a wide diversity of roles to play including development, growth, protein synthesis, energy homeostasis, cognition and behaviour, every research group studies only a narrow range of functions of the molecule. As a result, a comprehensive picture of the molecule never emerges and alternative possibilities, such as an altered cognitive requirement affecting insulin expression, which in turn alters glucose regulation, are never perceived.

- Displacement behaviour: When an individual is highly motivated towards an act but something else prevents that act, the individual carries out an apparently unrelated act. Displacement activity is classically demonstrated in animals as well as humans (Ingram 1960; Anselme 2008). A parallel to displacement behaviour in the scientific community is that when confronted by a strong and evidence based anticonformist argument, a typical response is to avoid confronting the argument directly and criticize some less relevant sideliner and reject the importance of the argument based on it. For example, when the muscle specific insulin receptor knock-outs were shown to have normal levels of fasting insulin as well as fasting glucose, the direct implication of the experiment was falsification of the compensatory hyperinsulinemia and inadequate compensation hypotheses. However, since accepting falsification would have seriously challenged the prevalent theory, a smart displacement reaction was to say that since it did not lead to prediabetes or type 2 diabetes, it was not a good model system for studying diabetes and therefore could be ignored.
- Concorde fallacy: If one has already invested a lot on an endeavour then there is a reluctance to give up the endeavour even after realizing that it is not beneficial. This is said to be an irrational behaviour but there are many examples of it in animal as well as human behaviour (Curio 1987; Arkes and Ayton 1999). The term Concorde fallacy refers to the Concorde aircrafts that were not economically viable, but which British and French airlines continued to maintain on the grounds that they had already invested a huge amount of money in them. Whether this behaviour is always irrational and maladaptive is debated and the possible reasons for continuing with the 'sunk cost' are the advantages of information gained or reputational concerns (McAfee *et al.* 2008). Irrespective of how and why it evolved, this tendency appears to be common in human nature. Accordingly, it is extremely difficult to give up on a theory having followed it for a long time and having made a lifetime investment in it. The reluctance to get convinced by a new argument as described by Plank appears to be an example of the Concorde fallacy.

Can studying SEB be useful?

Human behaviour is a complex interaction of the innate, culturally acquired and consciously shaped components. Apart from the possible evolved behavioural propensities many flaws in the behaviour of the scientific community arise due to problems in formal education. For example the current emphasis in school education on having a correct answer to every question, treating textbooks almost as religious books are some of the basic flaws. Certain field specific tendencies such as 'innumeracy' (Paulos 2001) in the field of biomedicine are products of flawed education systems.

In reality, different determinants of human behaviour interact in such a way that their effects cannot be clearly separated. I indicated above that cost-benefit optimization is deep rooted in behaviour, however, the currency for measuring costs and benefits can be influenced by cultural and educational development of an individual. The three certainly interact and can contextually override each other. Therefore what is innate is not necessarily inevitable. It is possible to control, suppress, channel or modify innate behaviours but in order to do so we need to understand the evolved behavioural propensities.

Psychologists have studied groups of people on winter-over expedition to Antarctica. Absence of sunshine, isolation and confinement has a number of physiological effects on the human body and mind that adversely affect behaviour and psychological health (Palinkas 2003; De'camps and Rosnet 2005). It is interesting to note that the effects are a mix of environment induced physiological effects and cultural, social and cognitive effects. People from different cultures report different symptoms perhaps owing to different cultural backgrounds (Palinkas *et al.* 2004). Further, being aware of the altered physiology and thereby making conscious efforts to provide social support to each other is shown to bring in substantial relief. This indicates that the physiological effects, although having an evolved biological foundation can be substantially softened by conscious social efforts. This has an important lesson for SBE.

One response to the suggestion that some of the biases in review processes could be subconscious innate tendencies of the human mind (Pinholster 2016) was that if it is so, it cannot be helped! Such an argument can be used to justify the biases, making things worse. The Antarctica example illustrates that being aware of the problems actually helps resolve them even if they have innate biological origin. It would, therefore, be better to identify and study the social behavioural elements in the scientific process rather than sweeping them under the carpet. The possibility of evolutionary psychology playing a role in the scientific process needs to be treated as a hypothesis and tested rigorously. A potential problem in such studies is that the best possible data to test these hypotheses are not accessible. If the entire review process for all accepted and rejected manuscripts is made transparent at an appropriate stage of the publication process, it can lead to a great advancement in our understanding of the social psychology of the scientific community. Understanding the social behaviour of the scientific community would certainly be a potential revolution in science. There is substantial concern and some efforts in this direction (Kravitz and Baker 2011; Pinholster 2016; Tracz and Lawrence 2016) but its output is limited by lack of vision as well as by accessibility to data. As yet scientists 'studying the science of science' do not get sufficient social support from the community itself (Hu 2016). But in an era where a need for radical changes in scientific publishing is being felt (Pinholster 2016; Tracz and Lawrence 2016) a stage is certainly reached where we need to study the social behaviour of the scientist community and such an input holds a promise to revolutionise communication and social dynamics in science.

I would suggest a sequence of steps to achieve this goal:

- In the first place, data on reviews and editorial decisions (including rejections) by the majority of journals need to be made available to researchers interested in SBE. It is possible to bring the required transparency while maintaining anonymity of reviewers.
- Researchers need to focus on testing SBE hypotheses such as the ones suggested above using available data.
- Such studies can suggest a number of alterations and refinements to both science administration as well as science communication processes.
- Principles of SEB should form an intrinsic part of the training of every young researcher.
- A culture of social pressure as well as social support should emerge which would minimize the possible conflict between the ideals of scientific method and elements of human behaviour.

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