Peer Review and Innovation

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ABSTRACT: *Two important aspects of the relationship between peer review and innovation includes the acceptance of articles for publication in journals and the assessment of applications for grants for the funding of research work. While there are well-known examples of the rejection by journals of first choice of many papers that have radically changed the way we think about the world outside ourselves, such papers do get published eventually, however tortuous the process required. With grant applications the situation differs in that the refusal of a grant necessarily curtails the possible research that may be attempted. Here there are many reasons for conservatism and reservation as to the ability of a grant allocation process based on peer review to deliver truly innovative investigations. Other methods are needed; although such methods need not be applied across the board, they should constitute the methods whereby some 10-20% of the grant monies are assigned. The nomination of prizes for specific accomplishments is one way of achieving innovation although this presumes that investigators or institution already have available the money necessary to effect the innovations; otherwise it is a question of the selection and funding of particular individuals or institutions and requiring them to solve particular problems that are set in the broadest of terms.*

1. The issues involved

To make progress in dealing with the issues of peer review and innovation it is necessary to define both the terms used and the variety of contexts in which the activities occur. Who is a peer? Simple definitions of this word call for an 'equal' or a 'match'. Others are quite contrary and denote 'a nobleman' (anything but an equal – except of another nobleman). Perhaps there is irony in another connotation of the word 'peer' and that is to 'look at intensively', 'gaze', 'stare', or 'scrutinise'. If we are to be judged by a jury of our peers, we understand that they would be people who might be

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regarded as having the same or similar socio-economic/ethnic/age/gender characteristics. Such conditions are more often met in the breach than in the fulfilment for those who are chosen to be 'peers' are selected by obscure processes which seem to require that those picked are unlikely to 'rock the boat'.

Apart from their involvement in law-court juries, peers tend to be the workhorses of discretionary processes in the world of academia. We find them reviewing applications for grants and scholarships; effecting determinations about promotions; assessing research papers and scholarly works prior to publication; making evaluations about the relative excellence of research groups within universities; and setting the priorities for the distribution of research funds. These determining bodies do attract criticism, but when faced with the need to design an alternative system, those in power retort that 'although it is not a perfect system, it is the best that we can possibly have'. This paper will examine the contention that this less than perfect process decreases or depresses innovation, and that we should at least experiment on a small scale with alternative ways to make judgements and allocate funds.

Innovation, too, needs to be examined in more detail than the bald statement that innovation is that which is the font of creative ideas that lead to exploitable inventions and hence economic progress, wealth and well-being. Implicit in this term are the connotations of newness, change, novelty, mutation, permutation, modification or transformation. When a patent has been granted for an 'invention' one is sure that somewhere there has been an innovative step. However, over 90% of patents are not taken up or acted upon (even fewer than 10% are validated by being upheld in court). So the granting of a patent cannot be the only criterion for innovation. This is in contrast to those patented innovations that have been the breakthroughs of the 20th century: jet engines, electronics and computing, the reading and manipulation of genetic materials, hovercraft, antibiotics, vaccines, nuclear reactions, the petrol engine, plastics, lasers …

How large a change constitutes an innovation? If we look at the world at the level of atoms, each instant of time expresses a difference in the spatial and energetic characteristics in each of the atoms of the object world. Such changes are insufficient to justify the designation; innovation. When we engage in processes made up of a sequence of small steps, each step may not be seen as innovative, while the sum or resultant of all the small steps may constitute a major breakthrough or innovation, e.g., the myriad of discoveries which led to the cloning of the sheep, 'Dolly'; the painstaking work on a *Varicella Zoster* vaccine over 26 years which led to a licensed product in 1996; the transformation of the themionic valve through the transistor to the printed circuit over a period of 30-40 years. However, in addition to the small steps which actually lead to breakthroughs, there are many hundreds of equally small developments which finish up, unused and 'on the cutting room floor'. The association with repeated failure over many years often, though not always, characterises the innovation process. It is the determination, faith and resolve of the putative inventor which keeps him/her on track until the breakthrough is made.

We may also consider an alternative form of innovation based on the emergence of a scientifically substantiated new thought, idea or concept. While such novelty could well lead to practical innovations of a material nature, the other consequences of such a departure could be a different political system or alternative way of looking at the world and one's position in it. This kind of innovation which could arise following an instant of mental gymnastics or after many years of scholarship and investigative detective work may require a different type of environment to promote its emergence. Often such innovations are effected outside the work funded by peer-review-based project-grant-giving agencies. In this one could include the work which led to the discovery of the three-dimensional structure of DNA, the enunciation of a universal code for computers, the uncertainty principle, the impossibility of having a complete and self-consistent system and the theory of relativity.

Do citation indices offer a way to evaluate the quality of innovation? Here, too, the system is less than perfect. Whether a particular paper is taken as the font of a new line of research work is dependent on the time the paper comes out in relation to the state of preparedness of the researchers for this development, the place where it is first propounded, the people who attach themselves as proponents of the paper's worthiness and the personal and social connections of the author. (When Darwin and Wallace's papers on evolution through natural selection were read back to back at the Linnean Society meeting of the 1st July, 1858, they generated little heat or attention: it was as a result of the publication of the *Origin of Species* in November, 1859 and following the unstinting work of Thomas Huxley that the matter of evolution by natural selection became a major social issue). Another way to consider innovation is to regard such works as those which challenge the accepted or conventional paradigm.[1]

2. Journal Peer Review

There is little doubt that the peer review of articles for publication in academic journals is fraught with problems. Thorough studies of this process show that after a rejection by one set of referees, it is not unusual that a second set of referees would find a paper acceptable for publication. (Clearly papers which are of poor quality will be rejected by most referees of most journals, but as the leading journals (*Science* and *Nature*) reject a high proportion of submitted papers there are other journals which pick up papers that carry major advances and innovations because of certain inherent weaknesses of the peer review process.) Referees who were not senior in their subject area produced more reliable and careful reviews than did eminent academics and double blind reviewing also produced more extensive and thoughtful comments. Papers which contained ideas that were contrary to the dominant paradigm tended to be rejected.[1,,2] Classical examples of this are: the rejections by the Royal Society of Great Britain of the paper offered by Edward Jenner in 1796 describing the process of vaccination of humans to protect them against the disease smallpox; the initial rejection of Kreb's paper describing the citric acid cycle; and the difficulty faced by Glick and colleagues in having their paper on the discovery of the separate activity of the B-lymphocytes accepted [some of these and other examples are provided in reference 13]. Most studies of this subject were able to sustain the idea that the refereeing process did improve the readability and quality of the articles.[3] These generalisations are sustained in the recent meta-review of the peer review process written by A. E. Stamps III.[4]

It is doubtful, however, that the difficulty of publishing a paper claiming some innovative idea, concept or model would be sufficient to prevent eventual publication unless the paper was of such a poor quality and contained obvious inconsistencies that virtually all referees would move for rejection. Persistence and perseverance and sending the often-rejected paper to a sufficiency of journals normally results in publication: even though such a process may take several years. Therefore, it would be difficult to claim that the idiosyncrasies of the academic publication process would inhibit innovative thinking or writing: such activities would take place in any case; it is their publication which is delayed. One consequence would be that the innovative article is most likely to appear in a second or third ranked journal. As such publications are less widely read (especially by newspaper reporters and commentators, who review the first rank journals as a matter of course) the dissemination of the novel idea is hindered and often completely buried. In this sense the peer review process has prevented the widespread broadcasting of the new idea with the result that its manifestation and implications may be lost for many years. However, with modern retrieval techniques of literature searching (using the internet, search facilities and 'hot links') and the increasing number of meta-analyses which are conducted, it is not unlikely that, providing the essence of the innovative idea is implicit in the title and abstract, the article would be 'fished out' from the sea of dross surrounding it, resulting in an elevation to its rightful place in the 'sun'.

It could be held that the pressure on academics to publish papers that are cited (another form of peer review) is often a determining influence on what is published. Papers which contain a method that becomes adopted by the field (an innovative departure) are a prime target for an ambitious academic. But papers that have innovative theoretical ideas are also well cited as such ideas are subjected to various tests and experimentation. There is, however, little correlation between what reviewers think about the quality of a paper (by a prediction of its citability) and the actual citations obtained,[1] which means that the targeting of a paper for its future citability is a dubious means of achieving such acclaim. So, as the performance of an academic is measured by successful and cited publications, this criterion drives the research process towards a 'me-tooism' approach, which is safe and likely to be cited by others working to a similar driving dogma in a similar or related subject area. This results in the small step change type of innovation which may, by happenstance, lead to a completely new paradigm, but which generally serves merely to expand the literature of the field.

Peer review of publications does not welcome, support or promote innovation but neither does it prevent it. Such novelty as does occur relies on the foresight and determination of the author. People in general are resistant to change and the introduction of that which is deemed foreign. As much innovation is strange at first sight, resistance to its promulgation may be considered natural. Innovative work survives because of its intrinsic merit: it succeeds as people become familiar with its advantages and prospects. It also emerges when the necessity to achieve a new goal has been clearly enunciated with accompanying funding.

3. Grant Applications

If innovative work is to be published (by whatever means) it must first have been effected. While most tertiary academic institutions of yesteryear were sufficiently well endowed or had large enough income streams to support the exploratory research of their staff (as part of the 'Dual Support System' as effected in the UK), the modern era requires that almost all research has to be financed from external monies which are acquired as a result of a competitive process based on the peer reviewed grant applications. This process is subject to more pressures than the peer review of journal articles because the grants which are awarded determine who can and cannot carry forward a research program; and at what level. In choosing which applications are supported, a clear control of the innovative process is accomplished.

It is normal when applying for a grant to present the case for funding under a series of headings. At the National Institutes for Health (NIH) which dispenses some \$24 billion annually for research (2001) and where about \$8 billion is allocated to peer reviewed grant applications, a recent review of the criteria for grant awards has been undertaken by the director Harold Varmus.[5] These guidelines for applicants ask for a presentation of

- the significance of the proposed research (problem importance; advancement of science; effect on field)
- the approach (experimental design and methods, anticipating problems)
- innovation (novel concepts, approaches, methods, challenge to existing paradigms)
- the investigator (record, competence)
- the environment (record of laboratory, unique features, collaborations, institutional support)

Whereas at the National Science Foundation (NSF), where the annual expenditure on grant aided research is some \$5 billion,[6] we find that the program officer's brief in reviewing and awarding grants has criteria which do not specifically press for innovation or novelty (quoted in reference 7) viz:

- Research performance competence
- Intrinsic merit of the research
- Utility or relevance of the research
- Effect of the research on the infrastructure of science and engineering

Other boards look for 'good science' even though reviewers cannot agree on just what this is.[8] In the UK applicants are asked to provide information on some or all of the following:

- Quality of the science
- Practicability of the project
- Interdisciplinarity or multidisciplinarity
- Reputation of investigators
- New researchers
- Reputation of institutions and the support offered by those institutions to the work

- Industrial support or collaboration
- Collaboration with other laboratories
- Utility / Outlets of the work
- Budget
- Proportion of an academic's time allocated to project.

Within the European Union this list is compounded with the need to have transnational collaborators and to be able to show that the work is both precompetitive and could not be achieved by any one nation state alone. There is also a requirement to demonstrate that the authors of the application have considered the ethical aspects of their work and, where animals or humans are involved, have so designed their experiments to achieve the maximum benefit for the minimum suffering or cost.

The peer review process for grant applications is just as wanting as the process used for assessing journal papers. For example, Cole and colleagues[9] showed clearly that while reviewers could agree on the top 25% of applications and the bottom 25% of applications, for the middle 50% it was a matter of a random choice (largely determined by the particulars of the individual reviewer) as to which half of this group would be funded. Other generalisations which emerged from such studies were that eminent scientists were just as likely to have applications rejected as younger scientists, but they still managed to obtain more grants because they put in more applications. Again the reputation of the laboratory at which the work would be effected was not a dominant influence on the award of a grant although it is clear that most (over 75%) of the grant monies available go to about 30 institutions in the US and 20 in the UK. There is also a perception that, at the NIH, there is a tendency for reviewers to accept 'safe' applications supported by preliminary data which militates against the award of monies to pursue more innovative objectives.[10]

In reviewing grant applications there are possibilities for serious conflicts of interest. The reviewers may have close or distant connections with the applicants and indeed even in the absence of a personal relationship, there could be a shared or competitive interest in the subject area.[11] In the UK and in European review groups, where industrialists are present, there are ever present dangers that commercial competitive considerations are operative when academic grant applications are reviewed.[2,11,12] While both the NSF and the NIH require grant holders to undertake specified courses in research ethics to attempt to prevent the malpractice engendered in undisclosed conflicts of interest, in most other countries the education of future researchers and industrialists does not require a definable exposure to ethical issues. A consequence of such conflicts of interest is that research, which is likely to yield innovative data, methods or products and which might result in an increase of the fame of the investigators is less likely to be funded. Reasons for the rejection often allege that the research proposed is impracticable, unlikely to succeed or is at an institution which has not a reputation in the area (this would be expected if the research really sought to break new ground as there would not be any institution which would have a track record in the new area).

The requirement to do *good science* (aka: highest quality research; best research; research of real merit; research that clearly goes from observation to hypothesis to experiment) can be a hindrance to innovation. The derivation of new or enchanced knowledge or science by the application of the Scientific Method, however one looks at it, requires a high degree of criticality. Ideas or hypotheses are proposed, and every effort is made to test them stringently by as wide a variety of techniques (often requiring considerable innovation) as it is possible to bring to bear. Before one can proceed to innovations, it is customary to test the structure-function relationships of the relevant components so that one can make predictions (which do not normally transpire) as to how to achieve something of commercial value. The suggestion of an empirical approach to achieve an innovation in the hands of some, if not most, peer reviewers raises the spectre of instant rejection. (An empirical approach could involve an investigation of a number of innovative ways that might achieve an increase of yield, efficiency or a decrease in cost; such ways do not derive from a finely crafted deduction from a hypothesis except in the most general sense that a change in a manipulated parameter should provide a change in an observed parameter.) Yet it was the pre-Christian Empiricists who decried the use of theories (because in those times the theories tended to be based on the pantheistic system and its ramifications) when they came to treat people medically. Rather, they worked on a trial and error approach based on observation and experimentation to advance their art and achieve greater efficacies; in so doing they were capable of incorporating innovative approaches on the basis of the effects of the trials. This was indeed a rational approach.

To finesse the peer review process, applicants for grants spend much time analysing the way the grants are allocated. Who is on the reviewing committee? What kind of science is fashionable? Who received a grant and for what kind of project? What are the buzzwords, expressions, buttons and knobs which need to be activated or tweaked? How can I make my application look like an already successful application with the minimum of alterations necessary to provide novelty? With whom is it advisable to have a conversation outlining what it is that I propose to apply for? Is it possible to obtain advanced information as to the directions of the research that is going to be called for? And how can one get on to the committee that decides the future direction of research so as to influence that committee to go in the direction of one's own speciality? The need to have collaborators, an industrial outlet or expression of interest, multidisciplinarity, etc., tends to reduce or trim any outstanding ideas or objectives to the banal: the antithesis of the innovative.

In spending taxpayers' money it is expected that prudent sensible projects will be pursued. Peer reviewers are taxpayers and they would not wish their money to be squandered on projects which go under the epithets of: blue skies, ground breaking, step change, high risk or wacky. There is an inherent conservatism or attempt to increase the chances of as valuable a return as possible for the expenditure incurred. Hence the peer reviewers prefer to play safe, and to discharge their social responsibilities with the smallest chance that they could be held up as examples of careless, wanton or reckless spenders of public funds.[2]

The peer review process is not comfortable in continuing to support people whose projects fail to achieve their often overly ambitious objectives. However, ambition and failure are often crucial characteristics of the innovation process. It may take years or *decades* of repeated attempts to achieve a desired objective before a method is found to bridge the gap between the intent and the realisation. Were this to be sought within a system of educating future research workers, or as a way of progressing an academic career based on peer reviewed financing, then it is not likely that innovations will be forthcoming.

The peer review process may be considered the best option for the allocation of grants. But it has its costs, and one of these is that it is not an effective promoter of innovation. On the one hand, the mundane generation of innovative products or methods by small incremental improvements is precluded because this can be seen as poor science, empirical and lacking in imagination. On the other hand, bold innovative challenges to either theoretical concepts or practical ends are rejected as being contrary to common sense or impractical. If innovations do transpire from peer reviewed grant allocations, then it is probably in spite of the review process rather than because of it.

4. Conclusions

Reviewers are human and subject to the foibles of that species. One such peccadillo is a propensity to avoid the novel and take comfort in the familiar and its variants. Such characteristics clearly hinder the emergence of innovative ideas, methods or objects. Whether it is in the acceptance and appreciation of an article or paper for a journal or an application for a research grant, the unusual is set aside as being unworthy of further exposure or development.[12,13] This xenophobia has served humans well during the periods of our evolution in which the exotic world outside the one which we could control brought only the shocks and discomforts of the unknown. But that self-same world has within it the seeds of our future success. Our job is to root out those areas where there are benefits and discard the remainder. We can only do this if we are both bold and temperate; courageous and pragmatic; adventurous and prudent. Were ancient humans to have relied on a peer review process for their progression it is likely that we would not have passed our first major challenge which was the mastery of fire. It takes a maverick to make the first moves to tame the untameable. At this stage of our development, it is not beyond our collective wit to devise a system which both protects the basic qualities we value while at the same time challenging the theoretical and practical grounds on which we tread. So, it is clear the monolithic review processes responsible for so much of our research effort are but part of the answer: there has to be something other for innovation.

Harold Varmus at the NIH has recognised this and has required that grant applications be vetted with respect to their innovativeness. In the UK the Department of Trade and Industry, in conjunction with the Engineering Physics and Science Research Council, has devised a number of programs where the power of the peer review process is moderated by program managers and where the need for originality is spelled out in more detail. However, in these double or triple stage processes, peer review (obtained both by a process akin to the refereeing of journal articles plus a review committee of non-experts) is used to assess the worthiness of a project for progression to funding. The thrust to innovation is further impugned by the need for such projects to be feasible and to be able to demonstrate that industry is seriously (i.e. prepared to pay for a proportion of costs) supportive of the proposal.

If public monies are to be devolved to innovative ends then it is clear that terms and conditions of such investments would have to be different from those governing the kind of research that results in just good science or good engineering. In the past, society has experienced particular and innovative needs such as ways to deal with Foot and Mouth Disease, the need for new pesticides, ways to farm fish, cures for cancer, cloning sheep from somatic cells, etc., and has set up research institutes to explore the ways such objectives might be achieved. This approach has often yielded high dividends although the investment needed is considerable (tens-hundreds of millions/annum) and the time scale of its application is often long term (many decades). Nevertheless, scientists and engineers sequestered in such institutions have been highly innovative without the up-front scrutiny of peers. (This method of funding innovation is not without review and examination by visiting bodies of peers [sometimes referred to as the 'great and the good'] but such investigations are effected retroactively to make sure that public money has not been squandered and that progress towards the specified goals has been maintained.)

Another way of achieving such ends is to encourage governing bodies to announce large prizes that can be won by the achievement of specific innovations. This has many historical precedents such as the development by John Harrison in the period between 1734-1773 of a time-piece that would enable the longitude of ships to be determined with an accuracy of 0.5^{o}.[14] Another method would have to include the unfettered support of individuals who have 'visions' of a world which runs in a different way. These people may well be reluctant to disclose the area in which they seek to innovate, for fear of being forestalled, for fear of being held in ridicule, for fear of having their ideas stolen and for fear of rejection. This situation was graphically described by Bentley Glass in 1966.[15]

> *"What has been said about referees applies with even greater force to the scientists who sit on panels that judge the merit of research proposals made to government agencies or to foundations. The amount of confidential information directly applicable to a man's own line of work acquired in this way in the course of several years staggers the imagination. The most conscientious man in the world cannot forget all this, although he too easily forgets when and where a particular idea came to him. This information consists not only of reports or what has been done in the recent past but of what is still unpublished. It includes also the plans and protocols of work still to be performed, the truly germinal ideas that may occupy a scientist for years to come. After serving for some years on such panels I have reached the conclusion that this form of exposure is most unwise. One simply cannot any longer distinguish between what one properly knows, on the basis of published scientific information, and what one has gleaned from privileged documents.*

> *The end of this road is self-deception on the one hand, or conscious deception on the other, since in time scientists who must make research proposals learn that it is better not to reveal what they really intend to do, or to set down in plain language their choicest formulations of experimental planning, but instead write up as the program of their future work what they have in fact already performed."*

Should we accept the financing of key people, then the investment has to be long term (three or five year contracts are insufficient) and we will have to accept that there will be failure all along the way except for the final step. Also, it will have to be understood that for every ten projects which are set up, nine will not achieve their objectives, even under the most propitious conditions. Innovation is a high risk, high gain activity; we cannot afford to ignore this kind of enterprise, but at the same time we should not commit all our resources to these ends. Rather we should determine that a proportion (say 10 or 20%) of our research funding be devoted to innovative ends and then **radically** revise the way we allocate such funds. Having eliminated impossible proposals, the choice of the projects, or better still, the people, to fund may then be determined by a random process or one which reflects social priorities (a determination which could be undertaken by a lay panel). Under such a regimen it is essential that peer reviewers and industrial representatives are not involved in the selection process.

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