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Selecting applications for funding: why random choice is better than peer review

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Abstract A widely-used method of research funding is through competitive grants, where the selection of which of the applications to fund is made using anonymous peer review. The aim of the present paper is to argue that the system would work more efficiently if the selection were made by random choice rather than peer review. The peer review system has defects which have been revealed by recent criticisms, and the paper gives one such criticism due to the Nobel prize winner Sir James Black. It is then shown, in support of Sir James' position, that the use of anonymous peer review leads to a *systemic bias* in favour of mainstream research programmes and against minority research programmes. This in turn leads to the stifling of new ideas and of innovation. This thesis is illustrated by the example of the recent discovery of the cause of cervical cancer – a discovery which has generated substantial profits for pharmaceutical companies. It is then shown that selection by random choice eliminates this systemic bias, and consequently would encourage new ideas and innovation

Keywords: Research Funding, Peer Review, Random Choice

1. Introduction

Let me begin by saying something about how research is funded at present. We can take as an example the funding of biopharmaceutical research in the United States. Mazzucato has discussed this example in her 2013 book (pp. 64-71). Government funding is mainly channelled through the National Institutes of Research (NIH). Mazzucato writes (2013, p. 69):

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"Through a system of nearly 50,000 competitive grants, the NIH supports more than 325,000 researchers at over 3,000 universities, medical schools and other research institutions in every US state and throughout the world. These grants represent 80 per cent of the agency's budget with another 10 per cent used to directly employ 6,000 individuals in its own laboratories."

We see from this that the predominant method (80%) of research funding is through competitive grants. It is still worth noting, however, that the system of competitive grants is not the only method of research funding, and that the NIH also employs a method of direct funding, though this accounts for only 10% of its budget. There remains, of course, a further 10% of the budget, which, presumably, is spent on administrative costs.

I will make a remark about the direct funding of research towards the end of the paper, but the main topic of the paper is the predominant method of funding research through competitive grants. In this method, different researchers, or teams of researchers, submit grant applications, and it is obviously necessary to choose which of these grants should actually be funded. The method used for this selection is almost invariably peer review. In this paper, however, I want to argue for the thesis that random choice is a better method for selecting applications for funding than peer review. At first this might seem a strange, perhaps even eccentric, position. So, in order to make it plausible, I will begin by pointing out that in recent years peer review has been subjected to a considerable amount of criticism, and many defects in the process have been exposed. In the next section of the paper (**2**), I will give what seems to me a particularly insightful critique of peer review, which was produced by Sir James Black in the last years of his life.

2. Sir James Black's critique of anonymous peer review

Sir James Black (1924-2010) is famous for inventing two very important drugs. These were propranolol, the first successful beta-blocker, used to treat heart problems, and cimetidine, marketed as Tagamet, for stomach ulcers. These two medicines were 'blockbusters' and made millions for the pharmaceutical industry. Sir James himself, however, was not particularly interested in money making, and apparently never asked for anything in the way of royalties for his work. In 1988 he was awarded the Nobel prize for the invention of important principles for drug development.

What prompted Sir James to write his critique of peer review was an article written in 2008 by Jean-Pierre Garnier, the former chief executive of GlaxoSmithKline, about a problem, which had emerged in the pharmaceutical industry. Sir James described this problem as follows (2009, p.8):

"Industrial productivity measured in terms of successfully-marketed new small-molecule drugs has declined disastrously in the last decade or so. This is often interpreted as evidence that new drug invention has entered a more difficult phase. Earlier successes are now interpreted as due to the culling of the low hanging fruit."

Jean-Pierre Garnier argued for 'the disappearance of low hanging fruit' theory, but Sir James thought that this was completely wrong. The invention of new drugs is, in his view, no harder (or easier) than it was in past decades. The reason why fewer new drugs are appearing is that the search for them is not being conducted as well as it was in the past. The decline in new drug invention is strongly supported by Mazzucato's analysis of biopharmaceutical research in the USA. She divides new drugs into (i) new molecular entities (or NMEs), and (ii) variations of old drugs. The variations of old drugs are produced by taking a drug, which is already known to be effective, and slightly altering its chemical structure and composition. Such drugs are also known as 'me too' drugs. Now Mazzucato shows (2013, p. 65) that between 1997 and 2004, the spending on R&D of the private pharmaceutical companies increased by about 60%, while the number of NMEs approved approximately halved. As Mazzucato justly remarks (2013, p. 64): "the large pharmaceutical companies have been fairly unproductive over the last few years in the production of innovations." In fact, as she also observes (2013, p. 64):

"Private pharma has focused more on 'me too' drugs (slight variations of existing ones) and the development (including clinical trials) and marketing side of the business."

She adds (2013, p. 65):

"75 per cent of the NMEs trace their research not to private companies but to publicly funded National Institutes of Health (NIH) labs in the US."

Thus what is important for the invention of new NMEs is the research funded by NIH. Between 1997 and 2004, the NIH budget roughly doubled, but the invention of NMEs, as we have seen, halved in the same period. This does suggest that there may be something wrong with the method used by the NIH for research funding, which as regards 80% of its budget, is competitive grants with selection by peer review. In the light of this, let us return to Sir James Black's criticism of peer review.

I first heard of Sir James' views about research organisation when I read an interview with him conducted by Andrew Jack, which appeared in the Financial Times on 2 February 2009. Here Sir James states (Jack, 2009):

"The anonymous peer review process is the enemy of scientific creativity. Peer reviewers go for orthodoxy."

I found this point of view much to my liking because I had just written a book (Gillies 2008) whose publication was timed to coincide with the appearance of the results of the 2008 RAE, and in which I criticized the RAE and the process of peer reviewing on which it was based. I accordingly sent Sir James a copy of my book, and he kindly sent me in return a copy of a paper: 'The Business of Science in the Pharmaceutical Industry', and suggested that we might meet for lunch to discuss the issues. It turned out that we lived quite near each other in Dulwich, and I had a memorable lunch with him at a local Italian restaurant on 18 March 2009. I was hoping that we might have further contacts about the problems of research organisation, but sadly Sir James died on 22 March 2010 at the age of 85. The 2009 paper, which he sent me, has not been published, but I have kindly been given permission to quote from it by Sir James' widow and executor: Professor Rona M. Mackie/Black.

In this paper, Sir James elaborates his criticism of peer review. He says of scientific creativity (2009, p. 5):

"We can't organise this process but we can destroy it. Today the peer review process, which controls the flow of public funds into academia, can do just that. … The peer reviewers prefer the security of the well-advanced application to the speculative, they prefer the group to the individual, they prefer the fashionable to the new. … It is the prospective nature of the review, carried out anonymously, that is the problem."

So, in effect, projects, which are novel, speculative and unfashionable in their approach, are unlikely to get funded. Yet these are just the sort of projects which can lead to exciting new discoveries and inventions. It should be noted that Sir James was particularly opposed to *anonymous* peer review. He thought that scientists should indeed criticize each other, but in an open fashion, putting their names to their criticisms. In this paper I will speak simply of peer review, but this should always be understood as meaning anonymous peer review. Open peer review is theoretically possible, but occurs rarely.

Sir James goes on to mention some famous scientists of the past – Paul Ehrlich and Paul Janssen. He writes (2009, p. 9):

"The question now is where are today's Ehrlichs and Janssens? I know we still have them; I have met them. As individuals with a new idea they find it almost impossible to be funded. The competitive, prospective, anonymous peer review system rejects them as too much of a liability."

Sir James also draws attention to another important consequence of the peer review system, which could be called **undesirable feedback**. Most researchers get to know what sort of projects are likely to pass the peer review system, namely those which accord with the dominant orthodoxy and do not introduce any novel and speculative elements. This knowledge often leads them to put forward an orthodox project because it has a better hope of getting funding rather than the project which they really think is more likely to get results, but which they judge to be too unusual to get funded. As Sir James says (2009, p. 5):

"The peer reviewers prefer the security of the well-advanced application to the speculative, they prefer the group to the individual, they prefer the fashionable to the new. Young people today, in the UK at least, choose their research programmes accordingly. I know because I have been told so many times when I criticise them for being pusillanimous about their choice of research programme."

This may look as if Sir James is criticizing the young researchers, but he is careful to stress that the fault lies not with the individual but with the system. As he adds (2009, p.5):

"The distortion is no-one's fault. It is the system which is in error."

Moreover, he says later on in the paper (2009, p. 9):

"However, I think that I can recognise 'pioneers' when I find, on probing, people who are not working on what they would really want to do but on what they find they can get funded to do. I have found a lot of frustrated, young, researchers around today. I can only offer tea and sympathy[1]."

Moreover, Sir James, who admittedly was a modest man, thought that he would have been unable to make his own inventions under the present system. As he says (2009, p.5):

"Now I have been lucky; I've managed to avoid the peer review process."

I find Sir James' critique of the peer review system very convincing, but it naturally raises the following question. If peer review is not a good method for selecting which applications to fund, can we think of an alternative method, which would be better? In this paper I want to argue that random choice is a better method than peer review for selecting applications for research funding, and that, in particular, it solves some of the problems to which Sir James drew attention[2]. Although this paper focuses on the use of random selection in a particular case, it should be pointed out that this method has potentially a much wider application. A recent valuable discussion of random selection as a social decision making process is to be found in Frey and Steiner (2014). The authors point out the advantages of random selection, and show that it was often used historically – for example in ancient Athens, and the Venetian republic in the Middle Ages. They argue for the introduction of random selection in a variety of cases, drawn from areas such as business, politics, science and culture. The present paper gives another example of a situation in which random selection could fruitfully be applied. In the next section I will explain how, in this situation, a system of selection by random choice might work in practice.

3. A simple example of selecting an application for funding

To analyse the problem of selecting applications for funding, I think it is best to start by considering a simple example. If the underlying principles are established for such an example, I do not think it will prove difficult to extend them to more complicated cases.

Let us suppose therefore that a funding body has decided to fund a research project in a particular area for a definite period – say three years. They have decided that the project can

[1] Rona M. Mackie told me that this is something of understatement. Sir James Black was very keen to encourage young researchers and did whatever he could to help them, but he thought that his efforts were being frustrated by the existing system of research organisation based on anonymous peer review

[2] To avoid misunderstandings, I should say that, although this paper is written in an attempt to solve some of Sir James' problems, I do not know whether he would have approved of the suggested solution. The idea of using random choice only occurred to me in 2012, and so I was never able to discuss this idea with Sir James

employ one research assistant, and have agreed that they will also pay for equipment and overheads. However, there is money for only one such project in the area in question. They therefore invite researchers in this area to submit applications for funding. Let us consider first how such applications would be dealt with in the standard peer review system.

On the receipt of each application by the funding body, a number of referees working in the field (probably two) would be selected, and the application sent to them to be reviewed and assessed. The applications with the referee reports would then go in front of a committee set up by the funding body. The members of the committee would probably read both the applications and the referees' reports, and in the light of all this information reach a decision about which of the applications to fund. Of course there might be a number of variations, but, broadly speaking, this describes the usual method of selecting an application for funding using the anonymous peer review system. Let us next consider how the applications would be dealt with using the random choice method.

In this case, when an application was received, it would first have to be checked for two things. First of all it would have to be confirmed that the research project proposed was indeed in the field for which the grant was being offered. Secondly it would have to be checked that those involved in the grant were competent in the sense of having been properly trained to carry out research in the field (perhaps through having successfully completed a research PhD, or by having worked as research assistants on other projects, or by having a reasonable record of research and publications etc.). It would be important to check for competence in this sense otherwise there would be a risk of cranks, who had no proper idea of how to do research, applying for grants. It would be fairly routine matter to check that an application satisfied the above two points, and such a check would not involve any assessment of how likely it would be for the project to produce interesting and important results. An application, which passed the above two checks, might be described as a serious application. Once all the serious applications were in, the choice of which one to fund would be made by selecting one at random. For example, if there were 6 serious applications, they could be numbered 1 to 6, and the choice made by rolling a die. If 5 came up, application 5 would be funded. I now turn to my arguments that selection by such a random choice method would be better than selection by peer review.

4. Random choice is cheaper

My first argument is that selection by random choice would be much less labour intensive and hence cheaper than selection by peer review. The peer review system requires quite a lot of administrative work to deal with the applications.Usually at least two referees have to be selected for each application. Someone has to send the application to these referees and monitor that their reports have been received. The peer reviewing itself both by the referees and by the committee members is time consuming and hence expensive. At first sight it might seem that peer reviewing does not cost anything because researchers are not paid for the work they do in reviewing. However, this only means that the cost is concealed. Suppose that researchers spend on average 10% of their time on peer reviewing. This means that if the pay for a researcher is £X per hour, £X in reality buys only 54 minutes of that researcher's time, since he or she has, on average, to spend 6 minutes of each hour doing peer reviews. This in turn means that the true cost of an hour of the researcher's time is £60X/54, or roughly £1.11X. So the true cost of research is increased by 11% if the average researcher has to spend 10% of his or her time on peer reviewing.

If we now contrast this with the random choice method, we see at once that this method does not involve any peer reviewing at all, and so contributes to reducing the costs of research. In addition, the routine checks, which have to be performed to ensure that an application is a serious one, are simple and straightforward. So the method of random choice also involves less administrative work by non-researchers. Overall then there can be no doubt that it would be cheaper than the method of peer review and so is a cost saving device.

Of course, however, we do not want to introduce cost saving measures if the quality of the output is thereby diminished. However, far from this being the case, I will next argue that the random choice method, on average over a large number of applications, would produce better results than those produced by the peer review method.

5. Peer review and systemic bias

Let us now return to a consideration of the deficiencies of peer review. We are considering peer review in relation to assessing the potential of research projects to produce new and important results. Suppose, for example, that there is funding available to carry out research to try to find the solution of some problem. Let us suppose that there are 4 approaches to the solution of this problem, and these lead to the formulation of 4 different research programmes[3] – R_1, R_2, R_3, R_4 say. Research is carried out for 20 to 30 years and eventually the problem is solved. It turns out that it was work on R_3, which solved the problem. The remaining research programmes did not produce any results, which contributed to the solution.

Now the question I would like to raise is whether there was any means of knowing before research started which of R_1, R_2, R_3, R_4 would be successful? It seems to me that there was not. Indeed it may well have been the case that research programmes R_1, R_2, and R_4 initially looked much more promising than R_3, so that only a very small minority of researchers in the field started work on R_3. In retrospect, we could say that those who chose R_3 had more insight than the others – but maybe they were just lucky.

How likely is it that peer reviewers, even if they are experts in the field, will deal well with this very hard problem of assessing the potential of research projects? My own view is that they are not likely to make a very good job of it, because no one really knows in advance what research projects are going to succeed. This is because research is, by definition, the exploration of things, which are as yet unknown. Moreover, there is another important reason why peer reviewers are unlikely to make a good job of assessing the potential of research projects. Peer reviewers do not really know which research projects are likely to succeed, but they do often

[3] For a classic discussion of the role of research programmes in science, see Lakatos (1970).

have quite strong prejudices about the matter. These prejudices, taken in aggregate, lead, as I will now show, to sub-optimal decision making.

The root of the problem is what I will call **researcher narcissism**. This is a condition, which affects nearly all researchers (including the author of the present paper). It consists in an individual researcher believing quite strongly that his or her approach to research in the field is the best one, and most likely to produce good results, while the other approaches are less good and less likely to produce any good results. The existence of researcher narcissism is not surprising. Most researchers will spend some time thinking about which approach to adopt to their research, and, when they opt for a particular approach, there will be reasons for their decision. Moreover, once having made the decision, it tends to get reinforced by the fact that they mix a great deal with others working along similar lines, all of whom are convinced that what they are doing is right. In addition, any researcher has a strong interest in his or her approach proving to be the successful one. If it is successful, the researchers working on it will be more likely to get promotions and perhaps prizes. Their PhD students are more likely to get jobs, and so on. Conversely, if their approach to research proves unsuccessful, all these desirable consequences are much less likely to follow, and they are likely to see the rewards going to those working on a rival research programme. Now it is a universal characteristic of humans to believe what is in their interest, and, in the present case, this amounts to the belief of researchers that they have adopted the best approach to research in the subject. Of course there may be some researchers who are not so convinced that they are in the right, but even they, if they are cynical, may act as if they believed strongly in their approach, since this is a good strategy for ensuring its success, or, perhaps, for ensuring the failure of rival approaches.

Let us assume therefore that we have community of peer reviewers who are all researchers who suffer from researcher narcissism. How will this affect the way in which research projects are selected for funding by peer review? It is easy to see that, in this situation, there will be a systemic bias against those who adopt a minority approach or research programme. It is important to emphasize that this bias need not be held by specific individuals, but is rather something that arises at the level of the system as a whole. It is a systemic bias. At the individual level we have researcher narcissism, but this leads to a systemic bias against minority approaches at the level of the research community as a whole.

How does this happen? Because of researcher narcissism, a peer reviewer is likely to take a favourable view of a research project, which adopts the same general approach to research as that of the peer reviewer, and an unfavourable view of a research project, which adopts a different approach. Now let us suppose that peer reviewers are chosen more or less at random. Very few of these peer reviewers will, by definition, be working on a minority research programme. If the research project adopts the approach of this minority research programme, therefore, it is likely to be viewed with disfavour by the majority of the peer reviewers and so won't be funded. Conversely a research project which adopts a majority or mainstream approach is much more likely to be funded. Hence the existence of a systemic bias, which, in general, will lead to minority research programmes not being funded at all.

It is worth noting that this analysis entirely supports the claims of Sir James Black, which were described earlier, namely that "Peer reviewers go for orthodoxy.", " …peer reviewers … prefer the fashionable to the new", " … individuals with a new idea … find it almost impossible to be funded. The competitive, prospective, anonymous peer review system rejects them as too much of a liability." All these features of the use of peer review for selecting research projects for funding are explained by noting the phenomenon of researcher narcissism, and seeing that it leads to a systemic bias against minority approaches. Moreover, as Sir James points out, this systemic bias is reinforced by the phenomenon of undesirable feedback. Once researchers realise that they have a better chance of getting funding by proposing a research project, which accords with the orthodox, mainstream, majority approach, they will do so, even though they really believe that a minority approach might well give better results.

Now a systemic bias against minority approaches is obviously a very harmful feature of any system designed to assign funding to research projects. For a start, it discourages the introduction of new ideas and approaches in any field. Any new idea or approach will, by definition, be adopted by a small minority, perhaps by a single researcher. Hence, because of the systemic bias, it is unlikely to get funding. This leads to the continuation for many years of research programmes, which are popular but really are getting nowhere. Returning to our earlier example of the four different research programmes R_1, R_2, R_3, R_4 designed to solve some problem, where, after 20 or 30 years, it emerged that work on R_3 solved the problem, whereas work on R_1, R_2, and R_4 did not contribute anything to the solution of the problem. If R_3 initially seemed unpromising, and a systemic bias against minority approaches was operating in the funding system, then R_3 might never receive any funding and funds could continue to be poured into the alternative majority research programmes with no solution to the problem ever being found. To show that this scenario is not just hypothetical, I will give an example of a situation of this type, which occurred recently, in the next section.

6. An illustrative case history

In 2008, Harald zur Hausen was awarded the Nobel prize for the discovery that a form of cervical cancer is caused by a preceding infection by the papilloma virus. In the research, which led to the discovery, however, the majority of researchers favoured the view that the causal agent for cervical cancer was a herpes virus and not a papilloma virus. This was the majority research programme at the time. Zur Hausen and his group were the only ones who favoured the papilloma virus, and so were working on a research programme of a very small minority.

There were reasons why the research community favoured the idea that a herpes virus was the cause of cervical cancer. Several human cancers were known to be caused by herpes viruses. In particular, it had been shown that a herpes virus, the Epstein-Barr virus, was the cause of a specific cancer: Burkitt's Lymphoma. The dominance of the herpes virus approach is shown by the fact that, in December 1972, there was an international conference of researchers in the field at Key Biscayne in Florida, which had the title: Herpesvirus and Cervical Cancer. Zur Hausen attended this conference and made some criticisms of the herpes virus approach. He said that he believed that the results indicate at least a basic difference in the reaction of herpes simplex virus type 2 with cervical cancer cells, as compared to another herpes virus, Epstein-Barr virus. In Burkitt's lymphomas and nasopharyngeal carcinomas, the tumour cells seem to be loaded with viral genomes, and obviously the complete viral genomes are present in those cells. Thus a basic difference seems to exist between these 2 systems. (cf. Goodheart, 1973, p. 1417). It is reported that the audience listened to zur Hausen in stony silence (Mcintyre, 2005, p.35). The summary of the conference written by George Klein (Klein, 1973) does not mention zur Hausen. Clearly at that time, contemporary assessments of zur Hausen's research by the scientific community would have given him a low rating. He was regarded as a fringe crank, and his work was not referred to or taken seriously by the mainstream. In the long run, however, zur Hausen proved to be correct[4].

Zur Hausen's research was funded by his university in Germany. So there was no need for him to apply for funds using what Sir James calls: "the competitive, prospective, anonymous peer review system". Let us now consider what would have happened to him and his group if they had had to apply for funds in this way. From the account I have just given, it is obvious that if a peer review had been conducted in 1973, then the research programme of zur Hausen and his group would have got a very low rating. It is likely that they would have been unable to obtain research funding, and the discovery of the cause of cervical cancer would have been long delayed. Millions of dollars would still have been spent on searching for a herpes virus causing cervical cancer, but no result would have been produced. Moreover, if all funds for research had been assigned by "the competitive, prospective, anonymous peer review system" it would have been very difficult for zur Hausen or anyone else to challenge the majority research programme (searching for a herpes virus as the cause cervical cancer), because anyone who initially suggested an alternative approach would have received a low rating in the anonymous peer review and would very likely as a consequence been denied funding. The characteristic undesirable feedback would have occurred. Even those scientists who had begun to doubt whether a herpes virus really caused cervical cancer, and would have liked to try another approach, would have been reluctant to put forward an alternative approach for fear of having their funding cut off. As a result the development of a vaccine, which protects against this unpleasant and often fatal disease, would have been delayed for several decades, while huge sums of money would have continued to be spent on research.

If the competitive, prospective, anonymous peer review system had been the only way of getting funds for research in the 1970s, zur Hausen might have been prevented from making his important discovery simply by being unable to obtain funding for his research programme. This rather supports Sir James' claim that he too might have been unable to make his advances if the competitive, prospective, anonymous peer review system had been prevalent in his day, and that he was (2009, p. 5): "lucky … to avoid the peer review process." It is also worth noting that sales of the vaccine against cervical cancer have generated large profits for GlaxoSmithKline and other pharmaceutical companies. In 2010, GlaxoSmithKline's revenue from sales of the vaccine were £242 million, and then in 2011 they were £506 million – up 108%[5]. Overall profit after tax is about 20% of sales. So this roughly translates into profits of £48 million in 2010, and £101 million in 2011. If zur Hausen had been prevented by the funding system from making his breakthrough, these profits would not have occurred. This lends support to Sir James' criticism of the former CEO of GlaxoSmithKline (Jean Pierre Garnier), and to Sir James' claim that part

[4] This account of Zur Hausen's work is based on discussions with Brendan Clarke and on his (2011)

[5] Source: GlaxoSmithKline Annual Report 2011, pp. 54 & 224

of the reason why fewer successful new drugs are being discovered is the prevalence of the competitive, prospective, anonymous peer review system.

6. Conclusions

It is now easy to complete the argument of this paper. We have seen that the selection of grants for funding by the peer review system leads, because of researcher narcissism, to systemic bias against minority approaches. This leads in turn to minority research programmes failing to gain funding, and this is reinforced by the undesirable feedback effect. Once the researchers realise that their chances of getting funding are much higher if they stick to the mainstream approach, they will devise research projects accordingly, even if they really think that an alternative approach might be more fruitful. In particular then the peer review system leads to the suppression of new ideas and new approaches, since these always start life as minority research programmes. It is hardly surprising that this in turn slows down the rate of new discoveries and inventions, whether of new drugs or of anything else.

Research is exploration of the unknown. So, in reality, no one really knows which of a number of proposed research programmes will prove successful. In this situation, a good funding system should ensure, so far as possible, that every different approach or research programme is funded. The peer review system has exactly the opposite effect. It tends to concentrate funds on the majority approach, whatever that is, while cutting off funding from minority approaches. It leads to the obviously suboptimal policy of putting all one's eggs in one basket.

There is also an irony here regarding competition. Many believe that competition improves efficiency, and they would therefore argue that assigning research funding competitively through the system of researchers making applications, which are judged by peer review, should improve efficiency. This peer review system certainly increases competitions among researchers, but, and here comes the irony, it decreases competition between research programmes, since, as has been shown, it tends to reinforce the majority research programme by withdrawing funding from its competitors. However, what leads to better research is not competition between individual researchers but between research programmes. As Lakatos said (1970, p. 68):

"*The history of science has been and should be a history of competing research programmes … the sooner the competition starts, the better for progress*." (Italics in original)

The protection of a majority research programme against possible rivals inevitably leads to research programmes being continued after they have reached what Lakatos called a "degenerating phase" (Lakatos, 1970, pp. 51&52).

Having analysed all the defects of the system of selecting applications for funding by peer review, it remains only to show that they are all avoided by using random choice. Random choice immediately eliminates systemic bias, since, if choices are made randomly, there is no prejudice in favour of one type of application rather than another. The undesirable feedback associated with the peer review system is also replaced by a desirable feedback. Every researcher making an application is free to design their research project in accordance with what they think will produce the best results, since there is no advantage to be gained by doing otherwise. Naturally then nearly all researchers will carry out their research in what they think, rightly or wrongly, to be the best way. This will encourage the introduction of new ideas and approaches, and overall improve the performance of the system.

Another advantage of the system of random choice is that it does not have to be introduced across the board. If some still doubt whether it would be as good as peer review, or think that it might have hidden defects, the system could be tried out in some areas to see if any undesirable consequences appear. Some funding bodies have initial grants for researchers who are starting their careers. This would be a good area for the introduction of random choice, since it would leave the young researchers entirely free to try out what approaches they think best, without worrying about whether the older and more established peer reviewers might get offended. Initial grants can be combined with what is called 'evolutionary funding' in which an initial grant can be continued if it is seen as having had some success[6]

Earlier (in section **2**) we mentioned the decline in the last decade or so in the invention of new molecular entities (NMEs) in the pharmaceutical area, despite considerable increases in funding. This strongly suggests that it would be desirable to try out some different systems of research organisation to see whether performance could be improved. Of course, for those who adhere to Garnier's 'low hanging fruit' theory, such attempts to improve performance would be useless. According to this theory, we have already gathered the low hanging fruit, i.e. we have already invented the NMEs which were easy to invent, and it will inevitably become harder and more expensive to invent further NMEs. However, this theory does not seem very probable. There have been enormous advances in knowledge of physiology and of the details of pathological processes. This means that there are many more known possibilities of how to target disease processes with drugs. Is it not likely that some of these new avenues of exploration will yield good results? Moreover, Garnier's theory leads to unduly conservative consequences. It would surely be better to try out new systems of research organisation, many of which are easy to implement on a trial basis, rather than saying that we must resign ourselves to the declining productivity of existing systems.

In this paper I have considered the system of competitive grants, and have suggested that it could be improved by substituting selection by random choice for selection by peer review. However, funding by competitive grants is not the only system of research funding. Our earlier analysis (in section **2**) of research funding by the NIH showed that while 80% of the NIH funds are assigned by competitive grants, 10% are assigned by direct funding. Might direct funding be preferable to funding by competitive grants? Could performance be improved by assigning 10% of funds by competitive grants and 80% directly? Many people would argue against such a suggestion on the grounds that competition is bound to introduce improvement. However, as we showed earlier in this section, the type of competition which improves research output is competition between different research programmes, and the system of funding by competitive grants actually reduces this desirable kind of competition.

Once this point about competition is fully grasped, it is no longer so clear that funding by competitive grants is preferable to direct funding, and a need arises to compare the merits of the

[6] These ideas about initial and evolutionary funding were suggested to me by Robin Poston

two types of research funding. This task lies beyond the scope of the present paper, though, in my 2008 book, I have made some suggestions about how a system of direct research funding could be introduced in the university context (see chapters 7 & 8, pp.65-86). Another valuable discussion of this issue is to be found in Ioannidis (2011) and the comments thereon. Ioannidis surveys various ways of funding research, including random selection of the grants to be funded, which is already used in some cases. He analyses the advantages and disadvantages of the various systems[7]

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[7] I am grateful to an anonymous reviewer for drawing this important paper to my attention

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