Focal Randomization: An optimal mechanism
for the evaluation of R&D projects

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Abstract

In most countries, governments intervene in the process of R&D by financing a substantial part of it. The mechanism employed for choosing the projects to be financed is a committee composed of experts who evaluate projects in their field of specialization, and decide which ones should be funded. This mechanism for evaluating projects is conservative. Proposals of new ideas are too often rejected, and inventions are commonly thrown out of the set of potential projects.

In this paper, I propose a mechanism that will allow less conformity: focal randomization. Focal randomization mechanism (FRM) states that projects which are unanimously ranked at the top by all reviewers, will be adopted. Projects perceived as valueless by all are rejected, while projects that are ranked differently will be randomized. I compare the average return under the present and proposed mechanism. I examine under which conditions this new mechanism is preferable, and its consequences on economic growth.

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I. Introduction

In most countries, governments intervene in the process of R&D by financing a substantial part of it. The reason for this intervention is that research and development undertaken by one firm has positive spillover effects on the entire economy. Since firms do not take these spillover effects into account, they invest in R&D less than the optimal amount. It is difficult to estimate these externalities, but it could double the real rate of return. Moreover basic research is a non-excludable investment and firms have no interest in undertaking it in a competitive market. Therefore, government financing is necessary. In some countries, it can amount to more than 60% of total R&D.

Such an intervention, however, creates its own problem: How does the government choose which projects to finance? It could be that the projects chosen are not those with the highest potential growth for the economy and therefore not optimal for the country.

In this paper, we discuss the mechanism of evaluating and choosing the projects to be financed in a given field, but do not treat the problem of deciding which field to finance. The mechanism employed in most countries is a committee composed of research fellows who evaluate projects in their field of specialization, and decide which ones should be funded. Alternatively, the decision is left to the committee chair taking into consideration the referee's reports. Both mechanisms are based on what is termed "peer review".

The problems with peer review have been analyzed in length, and in the next section, we give an *apercu* of the different problems. The consequences of making mistakes in funding research are more severe than with publishing articles, since in the latter, a good work might be known although later. However if great research was not funded, it will not come through.

However, most of these negative effects are not affecting the rate of growth of the economy as do one specific problem: the conservative bias. Peer review presents a bias against innovative applications: "A common informal view is that it is easier to obtain funds for conventional projects. Those who are eager to get funding are not likely to propose radical or unorthodox projects. Since you don't know who the referees are going to be, it is best to assume that they are middle-of the road. Therefore, a middle of the road application is safer" (Martin, 1997, p.3).

The best way to analyze the issue of conservative picking of projects is by using the typology of Arrow who divides new technologies between inventions and innovations; innovations being new applications of an already known technology,

while innovations are completely new technologies on which it is impossible to foresee their economic consequences. For projects that are in the category of innovations, it is not difficult for specialists in the field to evaluate them, and therefore the mechanism designed for evaluating innovations is adequate. However, projects entering the category of inventions (a new technology with unforeseen applications) cannot be evaluated properly and may therefore be rejected. Thus the method employed leads to the rejection of inventions.

The errors in estimating the value of these inventive projects can be very substantial. Some examples, from the past, highlight these errors of estimation. For instance, 50 years ago, the CEO of IBM claimed: "I think that the world market for computers is for no more than five computers". Eighty years ago, the commander of the Allied Forces during World War I said: "Planes are a nice toy but with no military value". One of the worst estimation on an invention was written by the CEO of Western Union in 1876 when deciding which project to finance: "The telephone has too many problems to be taken seriously as a telecommunication tool. No value for our firm".

The mechanism used nowadays for evaluating projects is conservative, and new ideas might be less accepted than they should be. Inventions are commonly thrown out of the set of potential projects. As a consequence, the government chooses investments with lower return; invention projects are riskier on average but yield higher return.

In this paper, I propose a mechanism that will allow less conformity; it will lead to accept some projects with a high potential, without accepting projects that for sure have no future. The mechanism I propose is a "focal randomization". When referees have a good understanding of the value of certain projects, their report will be final. On projects that appear to be inventive, and which referees cannot evaluate, we propose that projects will be chosen randomly. This paper examines under which conditions this mechanism is preferable.

We present three models that differ in their assumption about the information set of the referees. In the first section, the model assumes almost perfect information, while in the model of the third section, referees have almost no efficient way for evaluating projects. It is clear that in reality, the third model is the one prevailing. Our policy recommendations, presented in the conclusion are based on this last model.

II. The literature

The literature on evaluation can be divided into two main categories: research on the effects of ex-ante evaluation and research on ex-post evaluation. The ex-ante evaluation is related to choosing research to be funded, or papers to be published. The ex-post evaluation literature focuses on how to evaluate the impact of the research funded.

This paper focuses only on ex-ante evaluation, and it should be noted that most of the literature on evaluation is in fact analyzing evaluation ex-post, i.e., how can we determine the results and effects of a particular research.[1] This literature is divided in two main channels: scientific and experimental models vs. management-oriented systems models.

Lately, Jaffee (2002) has proposed some new designs for ex-post evaluation: a regression-discontinuity design. His proposal is based on the fact that all projects can be divided into two groups: the projects that have been funded, and those that were not. In consequence, one can check if there is a significant difference between these groups. This will allow examining if funding has an effect on research.

This paper is about ex-ante evaluation. How do you value proposals and which one, do you want to fund. Peer review today is the most common mechanism for evaluative testing of R&D projects or research. Most of the main funding agencies believe that expert review is the most effective mechanism for evaluating the quality, leadership, and relevance of research (especially basic research) performed and funded by them. Ultimately, decisions regarding the selection and funding of research programs must be made by agency managers informed by expert review. So, the peer review is used in almost each country to distribute public funds from research: the EU, the US and the RAE in the UK.

For instance NAS (1999) assesses: "Federal agencies should use expert review to assess the quality of research they support, the relevance of that research to their mission, and the leadership of the research. Expert review must strive for balance between having the most knowledgeable and the most independent individuals serve as members. Each agency should develop clear, explicit guidance with regard to structuring and employing expert review processes. The most effective way to evaluate research programs is by expert review. The most commonly used form of expert review of quality is peer review".

Despite the wide use of this way of picking projects to be funded, there is some criticism on this mechanism. Peer review leads to many biases (see Smith, 1994).

The first one is called the *confirmatory* bias, which has been analyzed by Mahoney (1977). Scholars have a tendency to reject research that will show results against their own theoretical perspective. There is a clear bias for scholars to accept research that support or confirm their beliefs. Moreover, there are some proofs of a lack of impartiality, vested interests, or rivalry (see Martin, 1997).

Peer review tends to present also an institution bias --reviewers favor submissions from prestigious institutions (see Godlee et al., 1998). Prestige of the person or institution affiliation affects the reviewer recommendation. Peters and Ceci (1982) have

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[1] A broad coverage of ex-post evaluation can be found in Fahrenkrog et al., 2002.

shown that names and affiliations affect acceptance: they have resubmitted papers that had already been published after changing the names and affiliations of authors. Their results confirm a bias related to reputations of colleagues, and institutions.

There is also a gender bias. A Swedish study found clear evidence of discrimination against women in the awarding of research grants (see Wenneras and Wold, 1997). There is also a "positive" bias. It seems that referee tend to prefer positive than negative results. Already Bacon (1621) wrote: "the human intellect .. is more moved and excited by affirmative than by negatives."

The bias, I intend to examine in this paper is the "conservative bias". It is a bias against innovative ideas, and inventions Horrobin (1990) has suggested that peer review is a conservative process. It encourages the research in known field, and makes it difficult for multidisciplinary work. It looks for safe research so that innovative and risky and unconventional ideas will not be funded.

This paper will propose a mechanism based on peer review that will cure the problem of conservative bias. It will also permit to weed out other wrong doings with peer review. Lately, there are proofs of misconducts with peer review: the most important are failure to check proper data and plagiarism. The NSF conducted an inquiry into plagiarism. They found cases in which researchers who were asked to peer review a proposal for research turned it down, and proposed it to another grant; the methodology has been copied word by word! In the UK, detecting fraud has become the aim of the committee on publication ethics (cope).

I will show in this paper that "focal randomization mechanism" (FRM) can permit to reduce the bias against innovations, and can also reduce the effects of wrong doing of peer-review.

III. A Framework for Focal Randomization Mechanism (FRM)
1. The First Model: Homogeneity of reviewers.
Our first model assumes that all reviewers are similar, and they all take the time that is needed to evaluate well the different projects.
A. Assumptions
i. We have k projects, from which only h can be funded.

ii. Projects are divided in two groups: innovations and inventions. Innovations are technologies based on an already known technology. A referee that is specialized in this field is capable, if he spends some time, of analyzing the different projects and evaluating them correctly.

Inventions are projects presenting a new technique and with unforeseen knowledge as to its future value. We assume in this first model that referees are capable of making the distinction between innovations and inventions, even if they cannot evaluate the

invention projects. In this model we also assume that all referees make the same decision about inventions and innovations, since they are all alike. There are k^o innovation projects, and k^v invention projects ($k^o + k^v = k$).

B. The model

Let us define:

Voi   The true value of the i innovation project, i=1...ko

Vvi   The true value of the i invention project, i=1...kv

Uoi   The value given by the referees of the i innovation project

Uvi   The value given by the referees of the i invention project

We order the projects in an increasing value such that

$Vo1 < Voi ... < Voko$.

$Vv1 < Vvi ... < Vvkv$.

The value of an innovation project, Voi, is based on two parameters. The first is its originality compared to the technology already known, that we define as Di (for distance from the known technology), where $Di \in [0,15]$, and the longer the distance from the known technology, the higher the value.

The second element is the cleverness of the project, Bi (for brightness); $Bi \in [0,200]$. The more clever a project, given its Di, the higher the value. Therefore we have:

$$Voi = \alpha Di + \gamma Bi \qquad\qquad (1)$$

We assume in this first model that the referees identify, without error, the value of the innovation. We have, therefore, that:

$$Uoi = \alpha Di + \gamma Bi \qquad\qquad (2)$$

An invention has a third element which is the inventive part of the technology, λi. Therefore:

$$Vvi = \alpha Di + \gamma Bi + \beta \lambda i \qquad\qquad (3)$$

Referees cannot recognize the true value of an invention, λi, and they all give the value λ^*.

$$Uvi \quad = \alpha Di + \gamma Bi \ + \beta \lambda* \qquad\qquad (4)$$

Moreover, we assume that since referees have difficulty in recognizing the future value of inventions, they tend to underestimate it, and give to $\lambda*$ a lower value than the average of λi.

C. The mechanism for choosing optimal projects.

In order to present the mechanism of "focal randomization" in a clear and vivid way, we will use a numerical example.

We assume that four projects have to be chosen out of ten (k=10, and h=4). There are seven innovations, and three inventions (this is in the information set of the referees). The value given by the referee to these inventions is low. Therefore in the present mechanism, none of the inventions will be chosen.

We set: α =10, γ =1, and the Dis and Bis are:

[4,5,7,8,12,3,9], [0,30,70,65,50,160,110]

Therefore Vois and Uois are:

[40,80,140,145,170,190,200]

For the inventions we set α =10; γ =1; β=1; $\lambda*$=20 and the Dis, Bis and λi s are:

[6,5,1]; [0,0,0]; [30,80,300]

Therefore Vvis and Uvis are respectively:

[90,130,310]; [80,70,30]

Given these data, we show in the first row of table 1, the optimal choice from the point of view of the country. The invention (the tenth project) and innovations number 5, 6 ,7 should be funded.

Under the present system, and under our assumptions that innovations are well ranked, we get that the invention (#10) is not going to be funded, and therefore the value for the country is lower.[2]

Our "focal randomization" mechanism in this simple model states: "All projects that are inventions are pooled, and in a random way, we pick one of them. About innovations projects, we take the best of them".

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[2] In this presentation, we do not discuss risk. It seems quite obvious that inventions have higher risk. However, in reality, scholars, who rank the projects, present their result mainly by discussing the expected value of the projects.

Table 1

	Optimal choice		The present system		Randomization	
1	#10	310	#7	200	1/3(#8+#9+#10)	177
2	#7	200	#6	190	#7	200
3	#6	190	#5	170	#6	190
4	#5	170	#4	145	#5	170
Total		870		705		737

From this example, we see that focal randomization allows a greater return to government financing because under the present process, all inventions would have been rejected.

It is clear that countries in which the government is very risk averse, this mechanism should not be adopted. If, for some reasons, the country prefers a lower growth in the near future to a much higher growth in the far future, it should not invest in inventions. This would be the case if the country runs a big current account deficit and cannot easily borrow abroad. However, if the country can borrow overseas, and has no credit rationing, then it should invest in inventions in order to increase the growth rate in the long run. For developed countries, governments have to finance the projects which display high risk, since the private market will invest in low risk projects with high returns. So if the government does not fund the "inventions", it looses some of its goal.

We note also that if there were perfect correlation between Dis and $\lambda$is, randomization would not be needed and invention with the higher Dis would be chosen. It would still be necessary to separate the process of choice for innovations and inventions.

This model has assumed that referees have perfect information about the value of innovations. In the next model, we present a more realistic model.

2. Second model: Heterogeneity of reviewers.

In this section, we waive the assumption that referees have perfect knowledge on the value of an innovation, as well as the assumption that referees are all alike. Sometimes, some of the reviewers do not invest enough time in order to perfectly understand the import of the project. It is widely known that: "We are concerned that the standard of the reports that we receive from our peer reviewers is not always very

high. Many of the men and women whom we ask to review for us are busy people. Perhaps they don't have the timer or motivation to do the job as well as they should".

In consequence, we assume that time spent on reviewing affects the quality of the report. We maintain that referees can make the distinction between inventions and innovations.

A. Assumptions

i. We have k projects, from which only h can be funded.

ii. Projects are divided in two groups: innovations and inventions. As opposed to the first model, a referee is not always capable of evaluating correctly innovations. The more time he spends, investigating the project, the better proxy he gets to the project value.

B. The model

As in the first model, we have:

Voi   The true value of the i innovation project,

Uoij   The value given by referee j to the i project

The definition of Voi is identical to the first model:

$$Voi \quad = \alpha Di + \gamma Bi \qquad (1)$$

In the previous model, the referees evaluate Di and Bi without error. We now assume that the cleverness part, Bi, can be evaluated with no error: all referees can say if a project is clever or not. However, the distance (the novelty) from the technology already known, Di, is not easily evaluated as in the previous model. We define Tj as the time that referee j takes to investigate an innovation, and assume that if the time invested is higher that the innovation distance, i.e., $Di \leq Tj$, then the referee can correctly estimate the true value of the innovation. However, if $Di > Tj$, then he does not understand the true value. We assume that the more time a referee spends analyzing the project, the closer he gets to the true value Di; and the greater the difference between Di and Tj, the larger the error in valuation. The specific form chosen, for sake of simplicity is:

$$Uoij \quad = \quad \begin{cases} \alpha Di + \gamma Bi & \text{for } Di \leq Tj \qquad (5) \\ \alpha Tj + \gamma Bi & \text{for } Di > Tj \end{cases}$$

The difference, Δij , between the true value, and the estimation is therefore:

$$\Delta ij \quad = \quad \begin{cases} 0 & \text{for } Di \leq Tj \qquad\qquad (6) \\ \\ \alpha(Di - Tj) & \text{for } Di > Tj \end{cases}$$

The error of estimation is zero if the time invested by the referee is greater than Di, and is a positive function of the difference between the innovative value of the project and the time given by the referee, Di - Tj.

How is the referee choosing the amount of time he spends on each project? On one hand, he wants to take the least time possible, k.Tj, since he could use this time for alternative tasks. It is assumed that every referee has his own subjective value to time, sj. On the other hand, the referee does not want to make too large an error about the true value of the project, since he is concerned that the best projects will be chosen. Therefore, the loss function he wants to minimize is:

$$L = \text{Min} \ (\Delta j)^2 + sj \ (k.Tj)^2 \qquad\qquad (7)$$
when $\Delta j = \sum \Delta ij$ i.e., the sum of the errors he makes.

The optimal Tj for each referee is the argmin of function L. It is found by taking the FOC of equation L. Thus, referees with high subjective value of time, will relatively prefer to spend less time Tj on each project, and for all project such that Di > Tj their error will relatively be greater. The optimal Tj for each referee is denoted Tj*.

Concerning inventions, we assume the same function as in the previous section:

$$Vvi \quad = \alpha Di + \gamma Bi + \beta \lambda i \qquad\qquad (3)$$
$$Uvi \quad = \alpha Di + \gamma Bi + \beta \lambda* \qquad\qquad (4)$$

C. The mechanism for choosing optimal projects.

We show that the optimal way to rank projects under these assumptions is to accept the projects that all referees have ranked at the top, and to reject all the projects that all referees have put at the bottom. For projects on which referees do not agree on the ranking, randomization is optimal. We term this mechanism: "focal randomization", since randomization is performed only on a subset of projects.

We show, in a numerical example, which reports will be chosen, and compare the mechanism presently used with the one proposed. As in the previous case, we present the focal randomization using the same basic example as before, although five projects

will be funded. So we assume k=10, and h=5. We have seven innovations, and three inventions, as in the previous case.

We set: α =10, γ =1, and the Dis and Bis are:

[4,5,7,8,12,3,9], [0,30,70,65,50,160,110]

For the inventions we set α =10; γ =1; β=1; λ*=20 and the Dis, Bis and λi s are:

[6,5,1]; [0,0,0]; [30,80,300]

Therefore the Vis of innovations and inventions are as in the previous case:

1   2   3   4   5   6   7   8   9   10
[40,80,140,145, 170,190, 200,   90, 130, 310]

About the valuation of the peer review, let us assume that we have three referees with sj such that the optimal time spent for reviewing the projects are 4, 5, and 9 respectively. Therefore from equation (5), Uoij j=1,2,3 are :

Uoi1= [40, 70, 110, 105, 90, 190, 150]; the ranking is: 6, 7, 3, 4.
Uoi2= [40, 80, 120, 115, 100,190, 160]; the ranking is: 6, 7, 3, 4.
Uoi3= [40, 80, 140, 145, 140,190, 200]; the ranking is: 7, 6, 4, 5.

and in average we get:
Uoi = [40, 77, 123, 122, 110, 190, 170];   (8)

In table 2, we present the different options under the different mechanism. In the first row of table 1, we present the optimal choice from the point of view of the country. We focus only on innovations and projects # 4, 5, 6 ,7 should be funded.

Under the present system, it is equation (8) which leads to the choice of the projects, and as shown in table 2, the projects funded are: 3, 4, 6, 7.

Our "focal randomization" mechanism in this simple model states: "All projects that are ranked in a consistent way at the top by all reviewers, will be adopted. Projects that are ranked differently will be randomized".

In our example, 7 and 6 and 4 are adopted by all reviewers. On 3 and 5, we will randomize. The results are presented in the last column of table 2.

Table 2

	Optimal choice		The present system			Randomization	
1	#7	200	#6	190	#7		200
2	#6	190	#7	200	#6		190
3	#5	170	#3	140	#4		145
4	#4	145	#4	145	1/2 (#3+#5)		155
Total	705			675			690
Innovation projects							

We see that the possibility of randomization for the innovation projects which fall into the middle category increases the total value of the projects. The inventions, as in the previous case, are determined by randomization, and therefore, comparing the new method to the one in use today, we get:

Table 3

	Optimal choice		The present system			Randomization	
1	#10	300	#6	190	#7		200
2	#7	200	#7	200	#6		190
3	#6	190	#3	140	#4		145
4	#5	170	#4	145	1/2 (#3+#5)		155
5	#4	145	#5	170	1/3 (#8+#9+#10)		177
Total	1005			845			867
projects							

In this model, we have assumed that referees can make a distinction between inventions and innovations. It is clear that focal randomization is the preferable mechanism. It gives a better result that the regular peer review mechanism that is used nowadays. However, this assumption can be sometimes inappropriate, and it is waived in the next model.

3. The Third Model: The General Case

In this section, we keep all the assumptions of model 2, except the one which claims that referees can make the distinction between inventions and innovations. Moreover, referees differ in their perception about inventions. Some referees, when analyzing an invention project, may believe that it is a good idea, and its probability of success is high, but others will disagree.

A. Assumptions

i. We have k projects, from which only h can be funded.

ii. Projects are no longer divided into two groups from the point of view of the referee. Referees are different in their subjective value of time, as well as their degree of imagination and possibility of being inventive.

B. The model

As in model 1 and 2, the true value of innovations and inventions are respectively:

$$Voi \quad = \alpha Di + \gamma Bi \tag{1}$$
$$Vvi \quad = \alpha Di + \gamma Bi + \beta \lambda i \tag{3}$$

In fact, we can write that the value of a project, independently of being an invention or an innovation is always equation (3), when for an innovation the value of λi is 0. Therefore we get:

$$Vi \quad = \alpha Di + \gamma Bi + \beta \lambda i \tag{8}$$

We order all the projects (inventions and innovations) in an increasing value such that

$$V_1 \ < \ Vi \ ... < Vk \quad .$$

The referees try to estimate these values. We denote Uij the value given by referee j to the i project. It is, as in the previous model, a function of the time spent analyzing the project and the cleverness of it. It is also a function of the referee's opinion on how innovative is the project. As stated earlier, no one can really perceive the value of an invention, but some referees are more inventive than others, and have better intuition as to what the future portends. We call λj the intuition of scientist j, which is distributed normally on the whole range $[0, \propto]$. The referees, therefore, make two types of errors, one on Di and one on λi.

On the distance, Bi, the error made by referee j is as in model 2:

$$\Delta^o_{ij} \quad = \quad \begin{cases} 0 & \text{for } D_i \leq T_j \quad (9) \\ \\ \alpha(D_i - T_j) & \text{for } D_i > T_j \end{cases}$$

When considering the inventive element, λ_i, we assume that (i) the more creative the referee, the better he estimates the invention element; (ii) if the referee is more creative that the project proposed, he makes no error on the value; and (iii) the error is an increasing function of the difference between the true value and his creative possibilities. Therefore:

$$\Delta^v_{ij} \quad = \quad \begin{cases} 0 & \text{for } \lambda_i \leq \lambda_j \quad (10) \\ \\ \beta(\lambda_i - \lambda_j) & \text{for } \lambda_i > \lambda_j \end{cases}$$

We get that the valuation given by a referee is:

$$U_{ij} \quad = \quad \begin{cases} \alpha D_i + \gamma B_i + \beta\lambda_i & \text{for } D_i \leq T_j \text{ and } \lambda_i \leq \lambda_j \\ \\ \alpha T_j + \gamma B_i + \beta\lambda_i & \text{for } D_i > T_j \text{ and } \lambda_i \leq \lambda_j \\ \\ \alpha D_i + \gamma B_i + \beta\lambda_j & \text{for } D_i \leq T_j \text{ and } \lambda_i > \lambda_j \\ \\ \alpha T_j + \gamma B_i + \beta\lambda_j & \text{for } D_i > T_j \text{ and } \lambda_i > \lambda_j \end{cases}$$

(11)

C. The mechanism for choosing optimal projects.

We show that the optimal ranking mechanism is to accept projects that all referees have ranked at the top (on which the variance between referees is low), and to reject the projects that all referees have put on the bottom (on which the variance between referees is low). For those on which referees do not agree on the ranking (the variance is high), randomization is optimal. We compare in a numerical example the mechanism presently used with the one proposed.

The same example is used as in the two previous models, but innovations and inventions are presented together. As in the first model, k=10, and h=4. We have seven innovations, and three inventions, but the referees are unaware of it. We set: $\alpha = 10$, $\gamma = 1$, $\beta = 1$; and the D_is, B_is and λ_is are as in the previous examples and shown in table 4, row 2, 3, 4, respectively:

Table 4

Rank	(2) Di	(3) Bi	(4) λi	(5) Vi	(6) Ui₁ (T=4, λ=20)	(7) Ui₂ (T=5, λ=30)	(8) Ui₃ (T=9, λ=150)	(9) Average
1	4	0		40	40	40	40	40
2	5	30		80	70	80	80	77
3	6	0	30	90	60	80	90	77
4	5	0	80	130	60	80	130	90
5	7	70		140	110	120	140	123
6	8	65		145	105	115	145	122
7	12	50		170	90	100	140	110
8	3	160		190	190	190	190	190
9	9	110		200	150	160	200	170
10	1	0	300	310	30	80	160	90

Let us assume that there are three referees with Tj and λj that are respectively: (4, 5,9) and (20,30,150). Therefore from equation (11), Uij j=1,2,3 are as presented in column (6)-(8). The average of these three referee review is presented in column (9).
The projects chosen under the different systems are:

Table 5

	Optimal choice		The present system		Randomization	
1	#10	310	#9	200	#9	200
2	#9	200	#8	190	#8	190
3	#8	190	#6	145	#6	145
4	#7	170	#5	140	1/3[#5+#7+#10]	207
Total	870			675		742

As can be shown in table 5, the focal randomization leads to higher average return. Randomization allows projects with high returns to be accepted. This does not imply that all inventions financed will be successful. On average, however, inventions have a higher return than innovations. This is the key element to the importance of inventions, being accepted.

IV. Conclusion

This paper has presented some simple models, although quite closed to reality, in which randomization produces the best solution. Since referees tend to better understand projects that are close to existing technology, they may reject inventive projects that will yield high return in the future. Peer review leads to conformity, while a random choice will permit the funding of inventive projects.

I would like to underline that focal randomization should not be on the whole set of proposed projects, because some of them are valueless which referees perceive immediately. Other projects are very good, and should be chosen without randomization. On the rest of the set of projects, randomization seems to be optimal. Therefore it is a focal randomization. First the projects are grouped in three categories, and only on the target group of the "unclear" value, randomization is performed.

Rational scholars feel sometimes uneasy with a randomization mechanism. Why should bright scholars throw dice, when it is so clear to them, that they make the right choice! Therefore, they do not like to waive the right to choose.

First, it is important to underline, that scholars keep the power on what we term focalization, i.e., the scholars decide which projects enter the group of projects entitled to randomization. When all scholars decide unanimously that a specific project is not good, it is immediately thrown out, and when all decide that a project is good, it is immediately chosen. However, there are many projects on which there are disagreements. Focal randomization will not throw these projects but will perform randomization on this focal group.

These models have taken into consideration that the mistakes of the referees are due only to lack of information. It could also be that referees choose projects in which they are not completely disinterested. They could act not in the public interest exclusively, but might have self interest, and might, for some subjective reason, dislike a project. Their report would, therefore, not take only the public interest into consideration. This "public choice" perspective would strengthen the importance of introducing randomization, into which no elements of sympathy, approval or power enter. Randomization on the projects where referees disagree not only increases diversity, but is a way of avoiding the tendency to accept projects of "club" insiders.

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