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# Should we fund research randomly? An epistemological criticism of the lottery model as an alternative to peer-review for the funding of science

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## Abstract

The way research is, and should be, funded by the public sphere is the subject of renewed interest for sociology, economics, management sciences, and more recently, for the philosophy of science. In this contribution, I propose a qualitative, epistemological criticism of the funding by lottery model, which is advocated by a growing number of scholars as an alternative to peer-review. This lottery scheme draws on the lack of efficiency and of robustness of the peer-review based evaluation to argue that the majority of public resources for basic science should be allocated randomly. I first differentiate between two distinct arguments used to defend this alternative funding scheme based on considerations about the logic of scientific research. To assess their epistemological limits, I then present and develop a conceptual frame, grounded on the notion of "system of practice", which can be used to understand what precisely it means, for a research project, to be interesting or significant. I use this epistemological analysis to show that the lottery model is not theoretically optimal, since it underestimates the integration of all scientific projects in densely interconnected systems of conceptual, experimental, or technical practices which confer their proper interest to them. I also apply these arguments in order to criticize the classical peer-review process. I finally suggest, as a discussion, that some recently proposed models that bring to the fore a principle of decentralization of the evaluation and selection process may constitute a better alternative, if the practical conditions of their implementation are adequately settled.

## Introduction

The way science is, and should be, funded by the public sphere has been the subject of renewed interest for the last decade. Firstly, scientists themselves have proposed various contributions which try to derive proposals for funding schemes from a description of the research process (Braben 2008; Couée 2013; Cadogan 2014). Should we let scientists themselves decide how to distribute

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money in a decentralized way (Bollen *et al.* 2014)? Should we promote a competition between projects, or allocate permanent funds (Couée 2013; Vaesen and Katsav 2017)? Should we trust peer-review, or choose the projects randomly (Gillies 2014)? Secondly, there is a relatively voluminous literature in economics, management science, and sociology that aims at characterizing the effects of contemporary modifications of funding policies on the production of knowledge (Gläser and Velarde 2018). These works study, for instance, the role of funding agencies as intermediaries between states and scientists (Edler *et al*. 2014), grant writing practices in academia (Velarde 2018), the diversification of sources of funding (Luukkonen 2014), or the mechanisms of research evaluation (Musselin 2014). These studies are valuable as descriptions of the effects of funding arrangements on the production of knowledge. However, their practical normative interests seem to be limited by their failure in deducing normative conditions for the public funding of science. The recent state of the art proposed by Gläser and Velarde (2018) states that this literature does not succeed in "establish[ing] clear causal relationships between funding and the content and conduct of research" (p. 4). The complexity and the variety of funding environments is a real obstacle to drawing general conclusions about the epistemic consequences of the choice of one given funding scheme over another.

Social epistemology has recently manifested a strong desire to play such a normative role in forming policies on the governance of science (Petrovitch and Viola, 2018). In particular, agent-based models have been proposed which try to characterize the epistemic effects of a given funding policy. On the basis of a model of epistemic landscapes (Weisberg and Muldoon 2009), Avin (2018a) tested various possible funding schemes, including a classic peer-review process based on the evaluation of the epistemic significance of the projects, and a lottery mechanism that distributes at least part of the resources randomly. Interestingly, this last proposition is strongly defended by various scholars coming from different fields, from natural science to social epistemology (Brezis 2007; Fang and Casadevall 2016; Avin 2018b,c; Gross and Bergstrom 2019, Roumbanis 2019). Random funding is then conceived of as a way of bypassing the recognized difficulties of peer-review to identify in a reliable and reproducible manner the merits of the projects to be funded (Graves 2011; Lee 2013; Boudreau 2016). However, even if we accept these classical criticisms of peer-review, is random funding really the most efficient way of overcoming such difficulties? Or is it simply a last resort solution, which consists in suppressing the evaluation and selection process rather than trying to reform it? Besides these epistemological considerations, taking seriously the "social" dimension of social epistemology might also imply considering how funding schemes take into account the societal impact of scientific research. For instance, peer-review based selection may be strongly committed to considering social needs. An overview of the rich history of the peer-review process shows how it has managed the balance between the scientific and societal interests of research projects, with regards to the discipline, to the funding agency, and to the historical evolution of perspectives on the place and role of science. Without going into too much detail on this point which exceeds the scope of this paper, it is worth noting that the evaluation of the societal impact of scientific research is currently a central issue for various funding agencies, even those dedicated to basic science. This focus on societal impact is quite a common subject of complaints from scientists who feel constrained to insist on the (sometimes over-sold) social importance of their research (O'Malley *et al.* 2009; Haufe 2013), or who consider that basic science is endangered by the importance given to social, political or economic needs (Schauz 2014). Recently, the notion of "Responsible Research and Innovation", which gained importance in the context of the H2020 European framework for the funding of science (Rip 2016), aims to accentuate the consideration of societal impact in evaluating research projects[2]. By contrast, random funding, in the way it is presented by its advocates, is not specifically designed to evaluate the societal impact of scientific research. The lottery model is notably restricted by Avin (2018a,b) to a fundamental or basic research, which would be separated from pressing utilitarian considerations external to the scientific field.

The aim of this article is first to propose a specific criticism, mainly based on epistemological considerations[3], of random allocation as an alternative to peer-review and, second, to propose an alternative principle of decentralization for the funding of science. I suggest that the associated funding scheme might perform better than either lottery or classical peer-review. In section 2, I give an overview of the contributions recently proposed to defend random allocation as an alternative to peer-review, and I reconstruct two central arguments that require discussion. In section 3, I develop a criticism of these arguments. To do so, I consider some features of the internal dynamics of the research process in order to suggest that random allocation might not be an optimal selection process. I show that these analyses may also be used to criticize the classical peer-review process (section 4). I also show how they can be used to discuss the relevance of random funding and of peer-review with respect to consideration of the societal impact of science. Finally, I suggest that

2The different H2020 calls for tenders published by the European Commission often makes explicit reference to this notion of responsible research as a mean of orienting scientific choices (see, for instance, the "Science with and for society" program, https://ec.europa.eu/research/participants/data/ref/h2020/wp/2018-2020/main/h2020-wp1820-swfs_en.pdf)

3I also evoke, as another important dimension to take into account, the question of the societal impact of scientific research.

my epistemological analysis implies that a more decentralized method of allocating resources might be preferable both to random funding and to classical peer-review (section 5).

## 2-Main arguments for a (more or less) random funding scheme

Criticisms of peer-review based evaluation of research projects by national funding agencies are not new and take many different forms. Even if this contribution does not aim to provide an exhaustive historical overview of these arguments which have been formulated over at least three decades (see, for instance, Travis and Collins 1991), let us recall that among other criticisms, peer-review in its current forms has been criticized for being biased by individual values (Lee 2013), which may lead to forms of nepotism and sexism (Wenneras and Wold 1997); for privileging safe projects over risky ones (O'Malley *et al.* 2009; Haufe 2013); or for depending more on the particular cognitive interests of the evaluators than on the intrinsic value of the proposals themselves (Boudreau 2016). Quite recently, a general criticism has arisen[4], which highlights the fact that peer-review based funding is aleatory for a large proportion of funded projects (Mayo *et al*. 2006; Graves 2011; Fogelholm *et al.* 2012). This argument, grounded on numerous empirical studies, highlights a major obstacle for the reliability and efficiency of peer-review (Snell 2015). First, panels of reviewers might not be able to evaluate and compare in a robust and reproducible way the intrinsic value of the proposed projects. Second, and consequently, the overall process of peer-review, which implies the time-consuming writing and evaluation of proposals, would represent a waste of time and money in the current highly competitive environment (Roumbanis 2019).

Based on these findings, various scholars have tried to elaborate alternative funding schemes. Among them, a proposition that is gaining a growing interest in the literature[5], is one based on the introduction of a random element in the mechanisms of funding. Brezis (2007) first proposed a process of "focal randomization" for the funding of R&D by governments. In a nutshell, the idea is the following: the projects that are chosen without ambiguity by all the reviewers are adopted. Similarly, projects unanimously judged as valueless are excluded. For the projects that are situated between these two extremes, a lottery system would randomly choose the projects to be funded.

4We thank an anomymous reviewer for advising us that this criticism can also be found in older contributions, such as Cole *et al.* (1981).

5And, more marginally, in the practices of funding. Let us cite, for instance, the "Explorer Grants" of the Health Research Council of New Zealand, the "Science for Technology Innovation Seed Project" in New Zealand, or the "Experiment!" grants of the Volkswagen foundation.

This mechanism is thus supposed to diminish the often-cited bias of peer-review, in particular its preference towards "conformity" (p. 1), and its dependence on the individual preferences and interests of evaluators. Pursuing the same objectives, Fang and Casadevall (2016) have proposed a very similar solution, which they call "modified lottery". Avin (2018, a, b, c) has summarized and defended these arguments by using an interesting agent-based modeling approach inspired by the literature on the division of cognitive labor (Weisberg and Muldoon 2009). His conclusions are similar to those mentioned before: at least in some cases (notably, when the epistemic field to be explored is poorly known), a system of lottery which randomly allocates resources may out-perform the classical peer-review process. Indeed, in these cases (that Avin assimilates to "basic research", p. 33), it would be very difficult for a panel of reviewers to estimate the interest[6] (in Avin's social epistemology vocabulary, the "utility function") of a given project, and random allocation would then limit the bias linked to *a priori* evaluation. This solution is defended on the basis of more qualitative arguments by Roumbanis (2019) which brings to the fore its potential for increasing "epistemic diversity, fairness and impartiality within academia". Finally, Gross and Bergstrom (2019) defend partial randomization on the basis of economic arguments: when the number of grants is small, the preparation of numerous research proposals that will never be funded constitutes a significant waste of time, and thus diminishes the overall efficiency of the funding scheme based on peer-review.

An overview of the pro-lottery literature indicates that different kinds of argument are proposed in favor of a random allocation of resources by funding agencies. A first category attacks the various kinds of systemic bias classically associated with peer-review, that we recalled at the beginning of this section. These bias might be linked to personal characteristics (sex, ethnicity), to the institutional reputation of the researchers, or to their institutional affiliation. This first category of argument, which is indeed entirely relevant, specifically concerns the intersubjective interactions between reviewers and grant-seekers. In this paper, we rather focus on another group of justifications for the lottery model, which are grounded on a certain conception of the very logic of scientific inquiry. We divide this second family of arguments into two main branches. The first can be called the "exploration" argument, and it argues that random funding may allow the funding of projects whose interest is difficult to evaluate at a given moment, because they present a high degree of novelty in the method used or in the goal which is pursued. This argument is notably the one elaborated by Avin (2018 a,b,c). The second, which one might call the "equally-good"

6Let me note that in this frame, the interest of a project is judged with respect to the overall goal pursued within the concerned "epistemic field".

argument, insists on the fact that many projects seem to be of equal intrinsic value if we refer to the existing corpus of knowledge, techniques and scientific questions. For these two arguments, partial random allocation would thus have a double advantage: a gain of time and money, and the correction of some supposed defaults of peer-review, notably its overall conservatism and its strong dependence on the subjectivity of the evaluator.

In section 3, I suggest that both these arguments (the explorer and the equally-good) are mistaken for the same epistemological reason. In a nutshell, my argument is founded on the idea that random funding is a last resort solution which underestimates the density of the interconnections between the variety of technical, experimental, conceptual, and theoretical scientific goals, and which overestimates the frequency of isolated, genuinely "exploratory" programs, whose interest for current scientific practices is hard to estimate.

## 3-Funding science by lottery: an epistemological criticism

I begin this section by a presentation of the epistemological grounds of my argument: the notion of system of scientific practice as characterizing the very structure of the research process (section 3-a); then I return to the random allocation models and explain why they might be not an optimal solution (section 3-b).

### *a-Systems of scientific practice*

The so-called "practice turn" in contemporary history and philosophy of science (Soler 2014) is characterized by an explicit desire to study scientific practices in all their diversity (Nordmann 2015). This practice turn generally aims at replacing a certain view of scientific progress characterized as "traditional" in which scientific dynamics are considered as "primarily generated and shaped by theoretical developments" (Ankeny and Leonelli 2016, p. 18). The question of knowing whether this historical reading of the transition from a "theory-focused" to a "practice-oriented" philosophy of science is fully correct remains open. The important point is that this practical turn was accompanied by an attempt to integrate the different kinds of scientific practice into a (more or less) systematic view of scientific development. I will retain here the conceptual frame proposed by Chang (2014) in which scientific activities are integrated into hierarchical and interconnected *systems of practice*. In Chang's view, scientific activities, whether they consist in technical, instrumental, experimental, or mental operations (manipulating live cells, lighting a match, testing a microscope, isolating a phenomenon in the laboratory, defining a concept) are considered as *actions* or *activities*, performed by an epistemic agent in order to fulfill “an identifiable aim” (p. 72). An important point is that all these activities possess both “inherent purposes” (their local, internal objective) and “external functions” (the more general aims to which they contribute). For instance, the act of lighting a match mobilizes a set of internal operations, which are accomplished in order to fulfill this objective. But it may also be part of a more global process, for instance, lighting a Bunsen burner. This last objective may itself be an activity developed to fulfill a larger goal: leading an experiment, in order to find of a new therapy or to answer a theoretical question, for example. Finally, each scientific activity, whether it be technical, instrumental, experimental, or theoretical, takes place in a network of superposed objectives, constituting what Chang names a *system of practice*[7]. Obviously, because of this continuous superposition of goals, what counts as a system of practice is not fixed. It is *relative* to the scale at which we observe it; that is to say, it depends on the objective to which we choose to refer when describing the research process or project under consideration. As we will argue bellow, this notion of system of practice might also be extended by taking into account wider societal goals of research.

Importantly, a given activity may belong to different systems of practice. For instance, the activity of preparing a sample for an observation in electronic microscopy may be shared by biology, chemistry, or physics. To take a more precise toy-example, let us imagine a general objective *O*: the identification of the proteins involved in the replication mechanisms of a given virus *V*. This objective may be important both for fundamental biology, which is interested in the comprehension of the molecular networks underlying life processes, and bio-medicine, which may look for a new treatment against *V*. *O* is thus shared by distinct communities, possibly embedded in distinct systems of practice. In this frame, we can imagine a group of researchers trying to develop new anti-viral molecules targeting the replication mechanisms. To do so, they may develop a new technique *T* to isolate and purify the proteins involved in this molecular network. Yet this technique *T* may also be interesting for other scientific communities: by example, for bacteriologists looking for a new vaccine or antibiotics against a resistant bacteria *B*. The objective of developing *T* is thus also shared by another system of practice.

7A useful graphic representation of Chang’s notion of system of practice is presented in the commentary of Chang’s text by L. Soler and R. Catinaud (Soler 2014, p. 80-92).

Let me use a more concrete example to illustrate the structure of the systems of practice: the works led by Beadle and Tatum (1941) on *Neurospora* to discover the links between genes and enzymes. As shown by Kay (1989), Beadle and Tatum started with a typical cognitive question, which had emerged from the state of knowledge and practice in biochemistry and genetics: are genes enzymes, or do they determine the functioning of enzymes? To solve this research question, they used culture media depleted in some of the nutrients necessary for their biological model of growth (the *Neurospora* mold). When choosing these depleted nutrients, they selected molecules which had an interest for agronomy and food companies (certain important vitamins). The choice of this specific protocol to answer the cognitive question at stake was clearly driven by Beadle and Tatum's desire to make their work *also* useful for another kind of problem (namely, the dosage of vitamins in food, see Kay [1989] and Morange [2000] for details), which was the motor of another system of scientific practice, developed by agro-food industries. Importantly, these interconnections between different kinds of research questions operate at different levels of the systems of practice. First, the overall cognitive, theoretical issue of the nature and role of genes was important both for cognitive purposes (in biochemistry and genetics), and for agronomy, since genetics was also very useful in agriculture (Harwood 1987). Second, at a smaller scale, the *technique* used to answer this cognitive question (the choice of the growth media) was also independently connected to another kind of problem (the vitamins supply in food) which was important for agro-food systems of practice. This simple example thus shows that a given research project may exhibit a multiplicity of interconnections between different kinds of theoretical, experimental, and technical practices coming from distinct systems of practice. The density of these interconnections determines the overall interest of the given project, as being itself structured by a superposition of theoretical, experimental, and technical activities.

These examples suggest that *if we consider all the (technical, experimental, theoretical) dimensions of scientific activities*, the research process appears to be structured by a set of interconnected and hierarchically organized objectives of different natures: technical, instrumental, experimental, but perhaps also cognitive or utilitarian. In this framework, the intrinsic *interest* of a given research process depends on its integration within this complex network of scientific practices. Here, this central notion of interest should be understood as being nearly equivalent to that of "significance" in Kitcher's (2001, 2011) model of "well-ordered science", elaborated as an alternative to classical peer-review evaluation of research projects. Let us recall here that Kitcher's aim is to make external (economical, political, societal) needs more visible in science policy choices. For Kitcher, it is thus necessary to recognize that the significance of a research project is linked to the convergence of different kinds of objective and, in particular, cognitive and utilitarian goals[8]. In Beadle and Tatum's example, the effective mix of different kinds of objectives may illustrate this view, by showing the fine interactions, at different levels of the research activities (general goals pursued, experiments and technics implemented), of the scentific field with the whole societal sphere. These considerations might also lead us to adopt a broader view of the notion of system of practice than that of Chang. Indeed, the local interconnection of the research activities is not limited to the scientific field: it also includes problems identified in the social, political, economical spheres. In this specific example, these interactions concern both the general objectives of the research, and the technical practices developed to fulfill it. More generally, the hypothesis I make here is that all systems of scientific practice might be connected, at different levels, with needs or issues external to the scientific field. A good funding scheme should then be able to identify these interactions and to consider them in the overall evaluation of the research projects.

*b-Random funding: why it might not be optimal*

Let me recall the central elements of the two general arguments commonly used to defend random funding by considering the logic of scientific research. The “explorer” argument insists on the fact that some projects presenting a high degree of novelty, and whose intrinsic value is hard to evaluate, might not be correctly assessed by a panel of reviewers. The “equally-good” one interprets the observed variability of peer-review by the difficulty of comparing (on the basis of the current corpus of knowledge, methods and research questions) the interests of projects whose value appears to be nearly equivalent.

These two positions are thus based on a double hypothesis: (i) there really do exist many (or a sufficient number to justify a random allocation) research projects whose *interest* (in the sense I gave previously to this notion) cannot be comparatively assessed, either because they are too innovative, or because there is no objective reason to favor them over other equally good proposals; (ii) random allocation is thus, most of the time, the best way to apprehend the complex network of interconnected (technical, experimental, theoretical) scientific practices (proposition *P*). This proposition *P* may be justified in a theoretical or in a practical dimension: *P* is true if it is logically *or* practically impossible to find a better way to assess the comparative interest of a research project (that is to say, the way it is integrated into the systems of scientific practice). To address the

8To recall the central example used by Kitcher (2001, p. 63), the cloning of the sheep Dolly in 1997 brought together general, large-scale epistemic goals (understanding the mechanisms of differentiation of cells) and utilitarian goals (creating GMOs, improving livestock, etc.).

practical dimension of *P*, the best solution is certainly to propose an alternative method of funding that could be shown to be better in evaluating the interest of a given project. The aim of this paper is not to propose such a model, but I will show in section 5 how some recent idealized proposals for rethinking the mechanisms of the funding of science may constitute possible starting points to dismiss *P* in its practical dimensions. What about the theoretical dimension of the pro-lottery thesis *P*? If we admit the image I gave previously, *P* may be severely weakened as a theoretical affirmation. Indeed, if we recognize that the research process is structured by a dense network of interconnected and hierarchically-organized scientific objectives, from the most technical to the most theoretical, then we have to deduce (against the "explorer" argument) that there may exist just a few projects (or at least, fewer projects than supposed by the pro-lottery arguments) that are so isolated from existing practices that it would be impossible to estimate in any robust way their comparative interest. More generally, my thesis here is that the theoretical defense of random funding as the best alternative to peer-review is mistaken as it underestimates the fact that the structure of the systems of scientific practice constraints strongly (or more strongly that assumed in pro-lottery literature) the kind of project that may be considered (relatively) interesting-that is to say, useful for a (relatively) large number of scientific practices, including technical, experimental or theoretical activities. Because of these interconnections, it seems reasonable to suppose: (a) that there are not a large number of "exploratory" projects that are strongly disconnected theoretically, experimentally and technically from existing systems of practices; and (b) that it is at least theoretically possible to obtain a more precise comparison of the value of "equally good" projects if we take into account the various kinds of objectives of different kinds they pursue. In other words, the "explorer" and the "equally-good" arguments would need to be grounded on a more thorough conception of the very dynamics of the research process itself to be fully convincing.

What can we say about the other dimension of the science funding problem —that is, the degree to which we take into account the societal impact of scientific research? As we noted before, the lottery model is explicitly presented as applying to basic, or fundamental science, since it might not be relevant to funding research on pressing social needs (Avin 2018, b). Consequently, applied or use-inspired science on the one hand, and fundamental science on the other, should be funded according to separate mechanisms. Yet, we have suggested that the notion of system of practice, if understood in a broad perspective including both the scientific field and the whole social, economic and political sphere, leads to consideration of the fine interactions which exist between the problems identified by scientists and social needs. These interactions operate at all levels of scientific activity, and a lottery process for the funding of science might underperform in identifying these convergences.

The next step is to test how these arguments might apply to classical peer-review, before proposing an alternative both to peer-review and to random funding.

**4-What about classical peer-review?**

Even if the main objective of this paper is to give a criticism of the funding by lottery model, it could be interesting to test whether these arguments might also be applied to a discussion of the relevance of a more classical peer-review process. One of the features of this peer-review based selection of research projects is the evaluation, by a limited college of experts, of the intrinsic value of the research projects under consideration in a given discipline or specialty. This evaluation establishes a balance between the epistemic interest, the feasibility, and the societal impact, depending on the funding agency and on the funding program. As we have highlighted previously, ciriticisms of peer-review insist on the subjective bias due to the very composition of the college of experts. If we focus here strictly on cognitive or intellectual bias (and not on subjective bias), a general criticism would be that the limitation of the sources of expertise (due to the limited size of the panels) gives a partial and/or biased estimation of the interest of a given project for a whole scientific community[9]. It is worth noting that our remarks about the structure of the system of scientific practice might be used to reinforce this criticism. Indeed, the small-scale interconnections between systems of practice may be largely transverse to existing academic disciplines or research groups, and thus to panels of experts. If we follow the broad conception of the notion of system of practice we proposed in the previous section, this argument is also applicable to suggest that classical peer-review is not optimal in taking into consideration the societal impact of research. Limited groups of experts might have difficulties indeed in identifying local, small scales interactions between the scientific field and the social, economical and political spheres. A central

9This argument is quite old, since it goes in the sense of classical Polanyi's (1962) criticism of all forms of centralized control of the orientations of science. For Polanyi, the best way of managing scientific systems is to let each scientist collect the necessary informations to make his own choices about the orientations of his research. Clearly, this theoretical argument is not adapted to the practical constraints linked to the limitation of resources: a choice has to be done between research projects, and consequently an evaluation of the projects is necessary, which limits individual scientific freedom. Besides, in Polanyi's view, external (societal, political or economic) concerns should not interfere with fundamental science, which puts aside the question of the societal impact of scientific research. However, it is worth noting now that Polanyi's view that “subsidies should be curtailed in areas where their yields (...) tend to be low (...). So long as each allocation follows the guidance of scientific opinion (...) the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole” (*Ibid.*, p.60-61) is not very far from the arguments I develop in the following of the paper.

challenge, given these conclusions, is to assess if it is *practically* possible to outline a funding scheme that would efficiently take into account our epistemological analysis, and which might do better than both random funding and peer-review evaluation.

## 5-Discussion: a more decentralized way of evaluating and funding science?

In the current interdisciplinary debates on the conditions of the funding of science by (often, national) agencies, the random allocation of resources as an alternative to peer-review is gaining a growing popularity in the literature. In this paper, I have proposed a theoretical criticism of this lottery solution, by suggesting that all research activities are embedded in a dense network of objectives of different natures (technical, experimental, theoretical, but also cognitive or utilitarian) which determine their interest for existing systems of practice. As I noted in section 3, this theoretical criticism should be completed in a practical plan by proposing an alternative funding scheme which could be better adapted than either classical peer-review or random allocation. My aim is obviously not to elaborate such a model, but I believe my previous epistemological analysis may be used to defend a more *decentralized* way of selecting projects[10].

Various idealized models were recently proposed which consider that resource allocation should be a collective epistemic duty that should not be centralized in the hands of expert panels[11]. For instance, Bollen (2014) argues for a system of "scientific agency" where each researcher would be in charge of directly allocating a certain amount of money to the individuals he wants to have funded. Barnett *et al.* (2017) defend a "democratic" system, where each researcher expresses his preferences by voting for a certain number of scientists who deserve to be funded. Even if these models are still projected ideas and are far from constituting operational funding schemes, they are interesting insofar as they introduce a principle of decentralization to the selection process. Such a principle of decentralization might be relevant if we consider the epistemological analysis given in section 3. Indeed, in this frame, the comparative interest of a given project is determined by the multiplicity of its interactions with existing systems of scientific practices. Thus, a central quality of a good funding scheme is its ability to identify and measure this convergence of interests coming from distinct theoretical, experimental, and technical scientific activities, and from distinct systems

10Obviously, these theoretical arguments should confront with arguments from sociology, management sciences or the political sciences.
11It should be noted here that these models do not consider the articulation between public and private funding. Similarly, the way these distinct sources of funding should be articulated in the frame of the decentralized model I propose in the following remains an open issue.

of practice, on specific research projects. Reciprocally, a centralized funding scheme, using a limited college of experts selected for their belonging to one given discipline or sub-discipline, tends to render these local, small-scale interactions invisible. It is thus tempting to consider that to have a better vision of the comparative interest of the research projects in competition, we need to *enlarge* the process of evaluation to a group of scientists that would be more representative of these transdisciplinary networks of scientific practice. A more decentralized means of selection, where each researcher would have the epistemic duty of selecting projects, thus seems to be more in conformity with these epistemic constraints than does random allocation, which appears to be a last resort solution once the default of classical peer-review are taken as established. Obviously, such a general idea needs to be thoroughly refined to be applicable as an efficient funding mechanism. The way grant applications are written and presented[12], but also the sociological relationships between individuals or research groups are important issues that need to be taken into account when developing a credible decentralized funding scheme. Given these appeals to caution, it might be useful to sketch more precisely a possible funding scheme based on such a decentralization principle. The challenge here is to test whether a decentralization principle could genuinely constitute a more promising alternative than either random allocation or classical peer-review as an efficient new way of evaluating and funding scientific research.

Let us first note that the abstract funding model proposed by Bollen (2014) might be sensitive to various well-known biases that are also present in classical peer-review, and that the funding by lottery scheme aims to avoid. First, it implies directly funding people, and not projects. Consequently, we might expect that bias linked to personal characteristics (Lee 2013), to intellectual authority, to interpersonal relationships, or to past success[13] are even more operative than in classical peer-review. This point is also higlithted by Barnett *et al.* (2017), which note that more "democratic" ways of funding research, where individual scientists would directly vote for colleagues, might be sensitive to various kinds of bias[14]. Second, the direct distribution of resources by scientists, if it was applied as such, would be strongly inefficient, because the popularity of a researcher (and, even more, the relevance of a project), is certainly not in a linear relationship with his material needs.

12The way research projects are presented and written would indeed be profoundly modified in a more decentralized model, since researchers might try to convince a larger audience of the multiple interests of their proposal. This would encourage scientists to insist on the various dimensions of their activities, from the most technical to the most theoretical set of objectives they pursue, in order to make visible the possible interconnections between distinct systems of practice.

13We might cite here the famous "Matthew effect" operating in peer-review (see, for instance, Bol *et al.* 2018).

14These possible bias include "vote rigging, lobbying and it [the funding of science] becoming a popularity contest" (Barnett *et al*. 2017, p. 1)

However, I argue that if it would take a different form, a decentralized scheme of funding might correct these bias, and constitute an interesting alternative both to peer-review and to the funding-by-lottery model. I then propose setting up a decentralization principle in the following way. We might imagine, rather than an evaluation of people, a participative evaluation of each *project*, including all the individual researchers, and possibly a public external to the scientific field: citizens, groups of citizens, or their political representatives. The general idea would be that each participant establishes a list of projects he wishes to get funded, given the limitations of the total budget allocated by the funding agency. Basically, the projects most often approved would be funded. This voting mechanism makes it possible to measure the convergence of individual interests on specific research projects. In other words, it allows the identification of projects which are significant for various systems of practice. In this frame, the evaluation of research projects becomes a collective epistemic duty, and not the role of a limited college of experts who cannot represent the full complexity of the systems of scientific practice. If it does not eliminate a systematic bias in peer-review linked to personal characteristics, I believe that a scheme that followed this principle might attenuate such bias with respect to Bollen's (2014) model, which gives a greater importance to people over projects. Besides, I would argue that given the conditions we detail below to address certain crucial practical issues, it might offer a credible alternative both to peer-review and to the lottery model.

Firstly, the participation of external publics in this process of evaluation and selection ensures local interactions between the cognitive goals of research, and its broader societal impact. To improve their chance of getting funded, projects should clarify their concrete implications for certain segments of the population, or for society as a whole. However, the mechanisms of this participation might be highly variable. A major issue is the nature of the participants: who should have a right to vote? Should it be the citizens themselves or their representatives? Should it be some identified stakeholders (NGOs, militant groups, etc.)? Another challenge is the management of the balance between the choices made by scientists and those made by the external public: should we focus on the intersection of preferences, and fund the projects that are approved by all the different stakeholders (that is to say, which are present both in the priority lists of the researchers and of the external participants)? Or should we merely consider the overall percentage of votes for each project? Without entering into more details, it is clear that the chosen solution would reflect a certain perspective on participation (which is also a political and ethical issue), and a certain position about the autonomy that science should have with respect to the social and economic worlds.

Secondly, such a decentralized model faces a range of practical questions concerning the flexibility and reactivity of the funding scheme. Indeed, systems of practice evolve continuously: new questions are raised, new technical needs appear, and new obstacles are identified. How should the way we fund science be adapted to this essential feature of the dynamics of scientific research? Clearly, this challenge is similar for all funding schemes, including the lottery model and classical peer-review. The time-lapse of funding, on the one hand, and the possibility for scientists to freely change the direction of their inquiry on the other seem to be two central features that require discussion. The very structure of the information flows between individuals and communities is also crucial: the main expected virtue of the decentralization of funding is to gain access to a better objective knowledge of the networks of practices of different kinds (technical, experimental or cognitive) that characterize the dynamics of scientific research. To do so, such a decentralized model imposes, first, that evaluation is a collective epistemic duty, and, second, that individual (or group of) researchers have an interest in extending their knowledge of other systems of practices, in order to convince various communities of the interest of their project [15].

Thirdly, such a decentralized scheme of funding would have a impact on the structuration of the scientific field in stable specialties and communities. On the one hand, some programs require vast amounts of money which must be allocated on the basis of political, large scale choices which exceed classical procedures of resource distribution. We might refer here, for instance, to policies concerning the exploration of space, or to major investments needed for further achievements in theoretical physics (particle accelerators). This is not in contradiction with our decentralized scheme, which is conceived of as an alternative to the standard calls for tender of national funding agencies. On the other hand, both a random allocation system and classical peer-review might promote the funding of domains of study only supported by a limited group of researchers. Contrary to this, the alternative model I propose in the present discussion favors the overall interest of a given project both inside and outside the scientific field. This might imply that some rare subjects could disappear if they do not succeed in exhibiting their cognitive, technical or utilitarian value for other systems of practice. This effect is a direct consequence of the epistemic model of the dynamics of scientific research we have adopted in this paper. This theoretical background might obviously be

15In a more practical plan, various possibilities might be proposed. For instance, a system of key-words or expressions, similar to that used by numerical tools of bibliography classification, might ensure the identification of projects depending on their technical, experimental, cognitive objectives.

discussed on empirical grounds, exactly as I discuss the background hypotheses concerning the logic of inquiry which justifies the funding-by-lottery solution. It is one of the aims of this paper to bring to the fore the importance of the theoretical perspective on inquiry we (sometimes implicitly) adopt when choosing a public funding scheme for science. As far as epistemology may have its say in the debates concerning the governance of science, this explicitation of the models of scientific development that drive our political choices is certainly one of the necessary tasks that philosophy is best fitted to take in charge.

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