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New Arguments for a *pure lottery* in Research Funding: A Sketch for a Future Science Policy Without Time-Consuming Grant Competitions

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Abstract A critical debate has blossomed within the field of research policy, science and technology studies, and philosophy of science regarding the possible benefits and limitations of allocating extramural grants using a lottery system. The most common view among those supporting the lottery idea is that some form of modified lottery is acceptable, if properly combined with peer review. This means that partial randomization can be applied only after experts have screened the pursuit-worthiness of all submitted proposals and sorted out those of lowest quality. In the present paper, I will argue against the use of partial lotteries or partial randomization and instead promote use of a *pure lottery* in combination with a radical increase in block funding. The main reason for holding this position is that a partial lottery cannot solve the problems inherent in the current funding system, which is based on grant competitions and peer review. A partial lottery cannot decrease the enormous time-waste, reduce the uneven distribution of time between researchers, neutralize expert biases or mitigate academic power asymmetries. Instead, we need a stronger focus on improving general time management in academia by implementing a more holistic model for organizing research opportunities in the future.

Keywords Pure lottery · Peer review · Evaluation of scientific ideas · Research funding · Grant proposals · Time management

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Introduction: Peer Review *and/or* Lottery?

There exists, to date, only a handful of decision-making methods in which lottery techniques are used as part of the distribution of research grants. Some of these methods have already been implemented by a small number of funding agencies and some are currently being tested, while other methods remain at a purely hypothetical level. Recently, Shaw (2022a) argued that, thus far, the general discussion about using a lottery system for research funding is rather underdeveloped and at times even misleading. I am certainly willing to agree, because this discussion has just begun to gain real momentum, and it has left some important issues unresolved. For example, can a lottery fully *replace* peer review when it comes to the sensitive task of allocating government funding for science, or must it always be combined with some kind of basic quality assessment as an assurance? In other words: Can a lottery be implemented without involving any expert judgments or screenings, and if it cannot, then what exactly do we gain by using a blend of peer review and lottery? This moves us closer to the most important question of all: What is ultimately at stake in this decision-making context from a broader perspective, beyond the narrow funding competitions that the majority of researchers today, throughout the world, have been forced to deal with?

In the present essay, I will add to this relatively new topic some important clarifications and additional propositions for a future research funding policy that is genuinely open to radical improvement. First, I will engage in this debate by showing the substantial difference between the hypothetical idea of using a *pure lottery*, on the one hand, and the growing interest among policymakers in implementing partial randomization or modified lotteries in their systems, on the other. Here, my main argument will involve demonstrating that a partial randomization in combination with peer review cannot really live up to the most crucial promises that come with a pure lottery, particularly if we consider a future research landscape that will increase in complexity and uncertainty. A modified lottery that relies only partially on randomization is obviously more compatible with the competition-oriented funding system that predominates today, which means that it preserves far too many of the shortcomings that accompany the current grant peer review system (see e.g., Roumbanis 2017, 2022). Implementation of a pure lottery, however, would require a more radical transformation of the entire model for organizing research opportunities, dramatically increased block funding being a pivotal part of that change. As a consequence, such a transformation of government research funding would have the potential to actually improve academic *time management* and recreate academia such that it becomes a "protected space" for many more junior researchers (Laudel 2017). Taking the existential issue of time seriously and seeing money for what it actually is–a means to an uncertain work-in-progress–should inform the future of science funding policy, causing it to shift from a detrimental focus on "excellent" grant proposals to instead exploring how we can best make use of the limited amount of time each researcher has at his/her disposal during an intellectual career. My goal with this essay is not to try to present a "waterproof" design for a new distribution model, but to provide a

first sketch for how such a model could look like. The sketch is necessarily based on hypothetical scenarios and approximations. Despite these limitations, my hope is nevertheless that this approach can inspire other scholars to continue theorizing this urgent topic so that it one day can lead to a transformative change.

Defending an Unrealistic Position?

Among researchers working within science and technology studies (STS), research policy, and philosophy of science, there has been a surprisingly lively debate lately on the very possibility of using a lottery or random elements for allocating grants (see, e.g., Barlösius and Philipps 2022; Bedessem 2020; Conix et al. 2021; Gildenhuys 2020; Horbach et al. 2022; Philipps 2022; Reinhart and Schendzielorz 2020; Roumbanis 2020; Shaw 2022a). If we are to come to terms with some of the most pressing issues and challenges associated with the overheated funding system researchers are trapped in today, I believe we must pay much more attention to the issue of time. First, there is a highly uneven distribution of time in academia today, and second, many researchers feel that they constantly lack time to engage more with both research and teaching (Adam 2021; Noonan 2015; Vostal 2016; Ylijoki 2021). The issue of time certainly has deeper consequences for science, especially if we consider the complex relations between everyday academic life and the aggregated macro levels. For example, in a study that was recently reported in *Nature* by Park et al. (2023), and based on the analysis of data of over 45 million papers and 3,9 million patents spanning over six decades, showed a remarkable decline in the disruptiveness of science, despite its exponential growth in terms of productivity. The authors' own conclusion, is that great many researchers nowadays tend to rely on narrower sets of existing knowledge, which may benefit "individual careers, but not scientific progress in general" (Park et al. 2023: 143). In other words: science has become hyper-specialized, leaving generalists and theoreticians with a shrinking space in academia. The fact that many researchers today are relying much more on narrower sets of existing knowledge must also be tied to the current funding regimes and the general lack of time. A lottery would presumably save a great deal of time for all research communities, given that researchers would no longer have to write grant proposals. For that to happen in reality, however, we also need to consider what kind of lottery we want to promote, and for what particular reasons. I will return to some of these reasons later, but I will first try to clarify my own position in this debate.

Shaw (2022a) argued that "funding-by-lottery" must be referred to as a family of different approaches, all of which in fact retain elements of peer review. Thus, it is directly misleading, in his view, to say that a lottery could fully replace peer review. According to Shaw, it should primarily be considered a new supplement, not an alternative, because "it is obvious that a full-blown lottery, one that *entirely* outcasts reason and embraces chance, cannot realistically inform science funding policy. As far as I can tell, no one has defended a full-blown lottery" (Shaw 2022a). But there is at least one exception. When I first discussed this topic some years ago (Author, 2019), my intention was in fact to defend the idea of using what

Shaw calls a "full-blown lottery" to replace peer review, not the idea of using it as a mere supplement. The title I used for my article was "Peer review *or* Lottery?" and my use of the conjunction "or" was by no means an arbitrary decision. Although I discussed the lottery idea from a more general perspective, by taking into account, for example, Avin's dissertation (2015) and highlighting some of the rare examples of funding agencies that have used modified lottery techniques, my original intention was nevertheless to argue for what I now prefer to call a *pure lottery*. The positive benefits of chance can certainly be used in different ways, like the way in which the *Swiss National Science Foundation* uses a lottery mechanism as a potential "tie-breaker" for all their funding calls. Moreover, the *Volkswagen Foundation* in Germany has tested and recently finished its experiment with partial randomization. There are a few other funding agencies that also have implemented modified lotteries, for instance, in Austria, Canada, New Zealand, and the UK. These are all interesting efforts to improve resource allocation, yet in my view they remain a kind of "decaf lotteries." There is nothing wrong with these initiatives per se, but they cannot truly live up to the great potential of a pure lottery. Recently, I came across another article by Shaw (2023) who, as I mentioned above, have claimed that a pure lottery cannot be a realistic option for science funding policy. However, in this article he proposes that a "nonpartial lottery" could be used in distinct epistemic contexts, what he defines as "luxury science."[1] This approach seems to imply, as far as I can tell, a rather limited focal randomization. How do you draw the line between "luxury science" and other forms of research; can there be different degrees of luxury involved in different projects? Will the majority of researchers still have to write time-consuming proposals, while the lottery will be used only for theoreticians? Are we really going to save that much time with this limited use of a lottery system?

If we are to solve some of the more critical issues that have been well-known for decades among experts on research policy, more radical measures must be seriously considered. To truly change and transform how research opportunities are organized in relation to time management, modified lotteries and partial randomization are inadequate, as they would only result in very limited differences within research communities. For example, they would not be able to mitigate biases and arbitrariness efficiently, a point I will discuss in the next section. Additionally, as long as peer review takes place prior to the lottery, a large amount of time can hardly be saved, because most researchers will still have to write and/or read proposals. Schendzielorz and Reinhart (2022: 16) also highlighted this crucial shortcoming, noting that it has become pretty much a false premise that is repeatedly used by proponents of modified lotteries as well as a persuasive selling argument for funding agencies. Extremely short applications could of course be time-saving, although I am not sure what they would be good for, except for administrative purposes. The historical example of physiologist and Noble Laurate Otto Warburg's minimalistic application, which he sent to the *German Research Foundation* in 1921, is quite amusing, given the lengthy applications that have become standard today. Warburg's

[1] I would like to thank one of the reviewers for bringing this article to my attention.

application consisted of only a single sentence, "I require 10,000 marks," and was funded (Serrano Velarde 2018).

Aside from the time issue, we also have the aforementioned problem of how to mitigate cognitive biases, conservatism, and other shortcomings that can push decision-making in a certain direction. To be sure, it may actually be considered impossible to protect the peer review process from these human aspects, which are deeply embedded in the evaluation process (Huutoniemi 2012; Roumbanis 2017). For this reason, defending a pure lottery would seem to fit rather well with Feyerabend's epistemological anarchism, which promotes hardcore pluralism in science and rejects all types of "minimal criteria for pursuitworthiness" (Shaw 2022b; see also Feyerabend 1964/1981). As I see it, this is an important philosophical position, because it touches on the most fundamental problems regarding the nature of human knowledge, almost in an ancient Pyrrhonistic manner. What I draw from this unique view is that we need to show more humility in the funding context and apply what the Greek skeptics called *epoché* ("suspension of judgment") in relation to the innumerable epistemic possibilities that belong to an unknown future. From the point of view of everyday research practices, the interdependency between basic and applied science would also be much easier to maintain if there were a more even distribution of opportunities in academia. Shaw (2022a) is certainly right in pinpointing the legal groundings that currently forces the funding agencies to specify their particular scope of interest, for example: What type of research is going to be supported, and who is eligible to apply? This kind of boundary work or "gatekeeping," is usually declared in the organizations' general mission statement. Given the current funding systems, proposals must therefore at least be screened for relevance and suitability, which is not the same as screening for quality and originality. However, all of this would change dramatically if the new distribution model that I will propose here would be implemented, because then the public funding agencies would *not* have to screen for relevance or anything. Instead, their primary role would be to allocate all the money directly to the university organizations and then monitor that the money is spent solely on research-related activities (e.g., salaries, data collection, conference costs, workshops). This shift in power would be both "reactionary" and innovative, with a much more substantial amount of the public funding of research being earmarked for all hired academics, and the rest being distributed by an unbiased lottery system. To be sure, this would place an even greater responsibility on those handling the recruitment processes locally to promote diversity and pluralism in the selection of new employees. Academics are certainly not immune to implicit biases and status hierarchies (Ridgeway 2019). But this is a well-known challenge and many organizations today are actively working to mitigate irrelevant factors and unwanted biases to impact on hiring decisions. The growing awareness about these issues can, in other words, lead to a more careful monitoring. Yet, the academic hiring of PhD-candidates, research assistants, postdocs, lecturers, and full professors, have always depended on the "sense of taste" and intellectual connoisseurship (Merton 1973; Roumbanis 2017) of the most experienced and successful people in academia. This is a fundamental part of judging scientific qualifications and promise, and could hardly be removed without losing something essential. On the other hand, a pure lottery could be used in a number of creative ways to make room for unbiased

decisions when job candidates with equally good qualifications are to be selected. It is a disruptive force that can change power dynamics and promote pluralism.

The Epistemic Dilemma of Peer Review and the Ambivalent Status of Research Proposals

Currently, most scholars who promote the lottery idea seem to think that the most plausible way to use randomization is by combining it with some form of peer review mechanism. For example, Avin (2015) suggested that the best model for allocating research money must be based on both peer review *and* lottery. He calls this model "triage." A funding agency should first implement a screening process to weed out all the low-quality proposals, and then use random selection on all the remaining proposals that live up to some minimal criteria for pursuit-worthiness. Avin argued that such a system should allow for all kinds of unorthodox and high-risk projects to join the lottery. In principle, this sounds like a good solution. But this is in fact where I deviate from Avin's otherwise interesting perspective. I have some serious doubts about the actual benefits of such a modified lottery, both in relation to time-saving–researchers would still have to write and read proposals (not to mention the costs for the entire administration)–and in relation to the fundamental uncertainty that comes with the quality judgments made in the screening phase. How can you ensure a broader admission that includes unorthodox projects or projects that are simply perceived as boring, but that still can lead to valuable results? There is always a danger that even the most competent and disinterested experts will dismiss some proposals for being of poor quality, too risky, or just not exciting enough when they screen through a large number of proposals and do not have enough time to compare them efficiently. The main problem seems to be that many unorthodox ideas or high-risk proposals often tend to be assessed as being of lower quality. In fact, very unorthodox proposals may even seem to be wrong or to make false claims. It can be quite difficult for an expert to point out exactly what gives him/her that impression about a certain research project, but in the screening process there would in any case not be time to explain such an impression. Against Avin's modified lottery, I wrote:

> "…this entails an element of judgment, the value of which can be discussed for reasons of principle. For instance, shouldn't a lottery be completely free from all manner of judgment or assessment? Isn't that the very idea underlying a lottery? Even a poorly written proposal may hide great potential." (Roumbanis 2019a)

I was probably too subtle in the way I formulated my critique. However, my doubts regarding the reliability of screening proposals have since then been further reinforced. For example, in a recent study, Veugelers et al. (2022) examined applications sent to the ERC's "Starting Grant Program" by using a novelty measure, a proxy for high risk, and found a major penalty against "risky proposals." One of their explanations for the result was directly tied to the first stage of the evaluation process, "when panel members screen a large number of applications

based on a short summary of the proposed research and a CV listing the candidate's main publications" (Veugelers et al. 2022). Their general conclusions in this study confirm the problematic effects of the initial screening of proposals, but then of course we have all the other issues that often arise later in the review process, e.g., the variations in scores and the impact of disagreements in the panel groups (Roumbanis 2017, 2022). In addition, a mainstream proposal can also hide many risks and uncertainties, without them being formulated explicitly.

I think it is fair to state that an important part of the problem with the current funding system can be directly linked to the *research proposal* itself–all the time consumed in creating it and the relative uncertainty it creates. My question is: Do we really need proposals to be written and evaluated at all, what is this institutionalized practice good for anyway? This is indeed a very crucial issue, especially if we wish to consider the benefits of a pure lottery. Many critics will probably respond by saying that the current system gives all researchers an opportunity to systematically formulate their own ideas independently and to receive feedback from reviewers (but see e.g., von Hippel and von Hippel 2015). In any case, how did academic scholars back in the day formulate their ideas without writing research proposals? The answer is: they formulated their ideas independently anyway and received direct feedback from their trusted peers. Today, great many applicants receive a list of scores and/or standard comments from the funding agencies, because of the increasing number of proposals that reviewers are asked to evaluate (Roumbanis 2019a). There is simply not enough time to provide meticulous feedback to all applicants. But for defenders of the current system, the practice of writing proposals is still an important academic institution that should be preserved, even though they might easily switch their opinion when talking to colleagues about the stress of writing proposals and the frustration of being rejected. I have witnessed this behavior many times. This ambivalence is not directly their own fault, though they cannot entirely escape responsibility for reproducing the beliefs surrounding these rituals. The hyper-competitive funding system creates a great deal of cognitive dissonance among scholars, not least because it touches on the academic status and reward system, which generates much stress and anxiety (Edlund and Lammi 2022). The ability to write grant proposals that get funded has become an important indicator of academic "excellence" (Langfeldt et al. 2015; Peterson and Husu 2022). There is, nevertheless, something highly problematic about a reward system that has made acquiring funding a core task. The whole funding system seems to incentivize researchers to use different opportunistic strategies: to cut corners, exaggerate, and violate basic scientific norms (Conix et al. 2021; Roumbanis 2019b). There is certainly a need to recreate a sound reward system, so that the younger generation of researchers can learn to hold on to their idealistic spirit and to act with humility in their profession. The real rewards should instead be associated only with substantial effort and contributions to the advancement of scientific knowledge. Monetary issues should be dealt with by economists.

In the future, most researchers will probably be perfectly content without having to convince a group of anonymous reviewers to give them money. Researchers will continue to grow their academic reputations for being skillful, talented, creative, bold, meticulous, etc., without having to write proposals and prove their worthiness. What is much more important for universities is to create more "protected

spaces" (Laudel 2017) and to offer an increasing number of PhDs decent opportunities, without them having to pretend they know beforehand the challenges and failures they might experience in the future. Everybody cannot be enrolled as PhD candidates, there must necessarily be selection procedures. But that is not unique to academic science, it applies to many other professions as well. However, once an individual has managed to pass the threshold, and even received a doctoral degree, then he/she should at least be able to expect reasonable working conditions.

A Sketch for a New Funding Model

The most important requirement before implementing a pure lottery would be to dramatically increase block funding at the expense of competitive funding. In Sweden, for example, the amount of governmental support for research via block funding in 2019 was *only* 43.6 percent (Bengtsson et al. 2022). The rest of the budget that year was distributed by a few governmental funding agencies that use traditional peer review to make decisions about grant funding. The strong and somewhat naïve belief in the incremental benefits of having researchers compete for funding must be seriously challenged. There is no convincing evidence to show that the performance and impact of Swedish researchers at the aggregate level has improved significantly with increased competition, in fact quite the opposite (see, e.g., Hwang 2018). Academia is already highly competitive in nature. For the truly passionate and dedicated researcher, only one thing really matters: to make valuable contributions to the progress of science and the common good (Merton 1973; Weber 1922/1946). Competition in the realm of science is intensified by old classical norms that celebrates originality and the significance of discoveries, something that generates both open conflicts and hidden rivalries between researchers. Thus, researchers do not need more competition to keep them motivated, especially not competition to get research money. When science has become a vocation–a way of life–then writing proposals in order to bring in new grants can only be a means to an end. If a dramatic increase in resources were distributed directly to all university departments, researchers would have more individual freedom and flexibility to choose the research directions they wish to pursue locally, allowing them to take more long-term approaches. Naturally, block funding should be distributed by giving a reasonable share to all active researchers with a legitimate affiliation (e.g., university or research institute), and use of these resources should not depend solely on the judgments of a handful of powerful and successful professors (cf. Vaesen and Katzav 2017). This change would also make it even more important to promote the value of *shared responsibilities* and collegiality. There is always a considerable risk that "academic tribes" and "academic inbreeding" leads to a detrimental intellectual stagnation at universities, but as Horta (2023: 609) recently highlighted, this can only be combated by implementing transparent recruitment processes and alternative reward criteria. Yet, at the same time, increased block funding should follow some form of hierarchical model, so that researchers who have proven their great scientific competency and contributed most to scientific enterprises will have sufficient time for research as part of their employment. Thus, all researcher would automatically have a fixed

amount of time for research in their contracts, without having to apply for it. But this amount could differ in percentages, for example, between a senior top-researcher and a young assistant professor. Exactly how this should be determined in practice is a fairly open question. In any case, there could be relatively clear-cut career ladders, whereby a researcher could receive increased amount of fixed time for research in relation to teaching duties. Academic performance should not only be understood as producing many articles or patents for new inventions. Individual research performance could also be more "Socratic" in nature, that is, less results-oriented and more characterized by contributing to the intellectual atmosphere during seminars and workshops. Individual performance can also be manifested by playing a supportive role for colleagues, such reading their manuscripts or being a good mentor to younger researchers. Researchers can be good to each other in very different ways. But the system should also enable solitary researchers ("eremites") to do their work within the academic organization. Still, one very important effect of this new model would be that even less established, qualified, or socially skilled researchers would be given more time and protection with this model than they receive under the current funding regime. The new distribution model should also allow researchers to fail several times and explore new pathways without feeling afraid of failing again. Failures are a natural part of scientific progress. This would also be a first step for all researchers to regain the *trust* they deserve, giving them more freedom to experiment and theorize within their respective professions. More block funding and a pure lottery would certainly break the outdated and dysfunctional funding system that has incentivized researchers to publish things that actually should not be published, if it were not for their professional survival or opportunistic strategies (Dalen and Henkens 2012). Furthermore, academia is currently providing PhD students and postdocs with quite different opportunities to advance as independent researchers (Ylijoki and Henriksson 2017). An increased amount of block funding could prevent the uneven distribution of opportunities, regardless of the researcher's academic status and social capital.

What, then, should we do instead? First, let us imagine that approximately 60–70 percent of government funding for research and higher education in a country like Sweden is distributed directly to the universities via block funding, then the remaining 30–40 percent could be allocated by using a pure lottery instead of grant peer review. That would give a considerable number of researchers with fewer opportunities an extra boost and a respite from teaching duties. For example, in Sweden, many researchers with tenure positions are primarily hired to do teaching and administration; but there are often expectations and even formulations in their employment contracts that they must actively apply for external funding (see also e.g., Stephan 2012). There must certainly be clear rules and local collegial procedures to maintain a good balance in the general distribution of new opportunities and the organization of collective duties at university departments. Everyone cannot apply for the lottery at the same time. In addition, a well-established professor should not be eligible for the lottery, but he/she would have limited need for more time, after having acquired time automatically via block funding (and not having to waste time on writing and/or reviewing proposals). The lottery would primarily be used to distribute *extra opportunities* for a number of postdocs and other less established researchers, giving

them new chances to work on their research ideas. An additional rule should be that, if an applicant receives a grant through the lottery, then he/she would not be eligible to join the lottery for the next 2 or 3 years, thus giving that opportunity to others.

I will try to give a brief example of the potential difference that could result from this new model. We can imagine a full professor with about 50 percent time to spend on research activities from block funding with the current system. In practice, of course, other duties like teaching, staff meetings and administration will always take time from research. In any case, if this professor were then to receive a grant (the success rate at the *Swedish Research Council* is, however, only approx. 15 percent), then that would obviously add more time for research. But we must not forget to subtract from this the time spent on writing proposals, time that he/she could otherwise have used to conduct the actual research, organize workshops or write articles, etc. Based on data from Statistics Sweden, Hwang (2018) estimated that somewhere between 10 and 20 percent of the available research time a researcher has at his/her disposal is used for writing proposals. A full professor also often spends a certain amount of time reading other researchers' proposals. With the new model in place, the same professor would automatically have at least 70 percent time to spend on research, primarily due to receiving more block funding and saving time by not having to write and read proposals. But what about all the less established researchers (e.g., postdocs, assistant professors, and others)? How would the new distribution model improve their situation as researchers? First of all, like a full professor, they would also not have to spend time on writing proposals, but could instead use the time to focus on doing research. Also, given that block funding would be dramatically increased, a minimum of 10–20 percent would accrue to everyone with a proper academic affiliation. Then finally, with a pure lottery, additional resources would be distributed randomly to a significant number of less established researchers every year, giving them better opportunities to further their projects. All of this should be part of the general time management at every local university department, with the focus being on creating a "protected space" for all affiliated researchers, irrespective of their academic status.

But who should be responsible for determining an individual researcher's eligibility to join the lottery? Assessing eligibility criteria could, if needed, be completely formalistic, in the sense that it would not require quality assessments of applicants' research ideas in advance, but only a standardized screening of organizational importance. For example: "Has a PhD/Does *not* have a PhD", "Has _% block funding," "Has a lottery grant/Does not have lottery grant," etc. One should of course elaborate in more detail on these eligibility criteria, but the point is that these criteria could easily be checked by a simple unbiased computer algorithm. This type of administrative mechanism would not have to be based on advanced machine leaning techniques, but rule-based algorithms would suffice. Hence, formal applications would not be required for this system to function, only the ordinary information that is continuously collected and monitored in the university data base.

Beyond the Question of Deservedness?

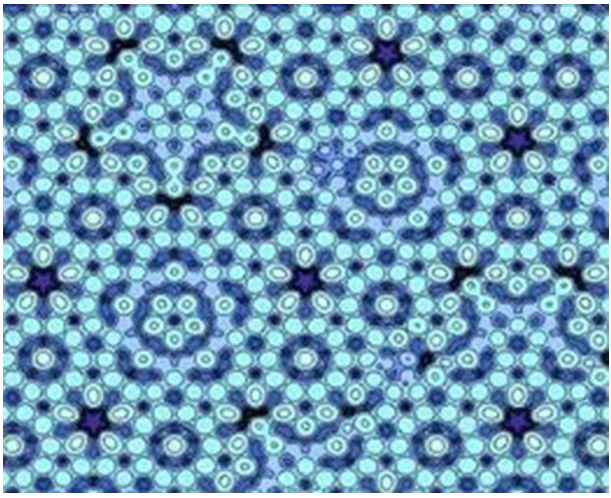
With the distribution model I have sketched above, the idea is that all researchers would receive more time for both research and teaching. Time management would be highly prioritized at university departments, with a view to avoiding a skewed distribution of duties and opportunities. A stronger focus on creating more transparent and inclusive recruitment processes would also be equally important (Horta 2022), especially as a tenure position would automatically offer a scholar time to spend on doing research alongside other tasks. A completely unbiased process will be difficult to achieve, but new hiring techniques, like for example, having anonymized applications without personal letters, could facilitate less discriminatory recruitment decisions. Indeed, a lottery mechanism could even be used to select among equally qualified candidates for a permanent position if deemed necessary by the board of evaluators. Meanwhile, diversity and pluralism are core values that are already making an impact on many European university policies (Philippczyck et al. 2023). Although gaps between what organizations say and what they do might exist, these ambitions must be cultivated and seriously taken into account, regardless of which funding system that will dominate in the future.

Nevertheless, a recuring concern surrounding the very idea of increasing the levels of block funding at the expense of competitive grants is that it might lead to an increasing waste of resources on unproductive and mediocre researchers. This concern has also been expressed regarding the lottery idea, thus, that it would simply lead to growing frustration among the group of researchers who perceive themselves to be more qualified and deserving (Höylä et al. 2016). This is indeed an intriguing concern, but it is likewise closely tied to the competitive mindset that stems from the current funding system. Obviously, people have always quarreled about the reasons why someone deserves something more than someone else; such disputes will probably never entirely disappear. However, the feeling of deservedness has become a peculiar kind of issue when so much in science is governed by money, despite many researchers not being genuinely aware of the uneven advantages and the happenstance that influence academic life (cf. Ylijoki and Henriksson 2017). The truth is, however, that history will eventually reveal that many of the well-established and successful researchers of today are largely substitutable, even if they publish a lot, receive a lot of funding and get prestigious prizes. This is not intended to belittle their contributions, because they all probably made their best effort, but instead to try to look beyond the given horizon and to call for more humility. But as Martin (2016: 15-16) correctly remarked, "there have undoubtedly been problems in the past with university research–for example, research findings that were never published; the lack of clear and coherent research strategy." And we might also add those researchers who do not try hard enough to contribute to the advancement of science and higher education, despite being given opportunities to do so. That will always remain a problem as well. One primary concern is obviously associated with the responsibility of making good use of taxpayers' money, which was essentially the main reason for institutionalizing the peer review process for research funding in

the US after the Second World War. Today, many governments are spending several billions every year on R&D, which of course makes the distribution issue relevant for the public. Still, universities have undergone a number of important organizational changes, but as Martin (2016: 18) also highlighted, "while some of the early efforts to improve the efficiency of university research may have resulted in significant gains, attempts to achieve further gains have come at disproportionate costs."

The very question of deservedness has become a problematic issue. What was previously rather taken for granted, that is, allowing academic scholars to do research and teach students, is nowadays far from a given. It is hard to disregard the pleasant feeling of receiving a reward in a highly competitive context, but with this reward also comes the pressure to deliver. In a sense, the grant has become an ambivalent mercy bestowed by the funding agency. Still, one would rather have this luxurious problem than not. But who deserves to be given the opportunity to carry out their research? That is, of course, the difficult puzzle grant reviewers are asked to solve, both individually and as a group. Wouldn't it be better to just follow the Pyrrhonistic philosophy and suspend expert judgments in the case of distributing opportunities? That is indeed a hypothetical question, yet we could get closer to an answer by establishing a contrafactual focal point that can be explored through the prisms of real-life empirical cases.

But first, let us do a quick and simple thought experiment: What if we were told today that all active researchers who received funding during the past five years were not actually selected by experts, but by a pure lottery. That would probably be quite a surprise for all of us. But would it radically change our opinion about all these individual researchers who are working hard and doing their best to make new contributions to science? Would this unexpected news cause us to believe that they are no longer worthy of pursuing their projects? And if so, then why were they permitted to apply for a research grant in the first place, if they were not considered worthy enough as professionals? That seems paradoxical. We could of course criticize the selection method, but why question the researchers? If we don't genuinely trust all those individuals who want to spend more time and energy on doing research, then what does that really signal to the politicians and the public? Given that contemporary science is much more interconnected and complex than ever before (Rosvall and Bergstrom 2009), through modern communication technologies and the flows of information between researchers globally, then both minor and major results will continue to pop up here and there, with or without formal proposals being written. If all researchers who are considered eligible to apply for funding by their home departments are rewarded a grant by either the "luck of the reviewer draw" or by pure chance, what difference will it make from a more holistic perspective? Hence, to put the issue of deservedness in perspective, I will now shed light on two contemporary scientists who are world-famous for their contributions to their respective research fields: material scientist Daniel Shechtman and biochemist Katalin Kariko. Both embodied the deep desire to develop research projects with great potential, but during a certain period, these projects were not deemed pursuit-worthy by their respective academic communities. The extraordinary strength and resilience of these two individual researchers made them endure the hardships, but things could also have ended differently. I will use these two cases to show, (i) the value of having



Source: Van Noorden (2011); Credit: Ames Laboratory. An atomic model of a silver-aluminum quasicrystal showing pentagonal mosaic-like patterns

block funding to support researchers with tenure positions in exploring their own ideas, and (ii) the importance of implementing a pure lottery that spreads opportunities more randomly so as to reach scholars with fewer opportunities. This will be part of my overall argument, which is that we should establish many more "protected spaces" in academia and use pure lotteries instead of grant peer review to increase time-saving, independence, pluralism, and humility.

Case 1: Shechtman's discovery of quasicrystals

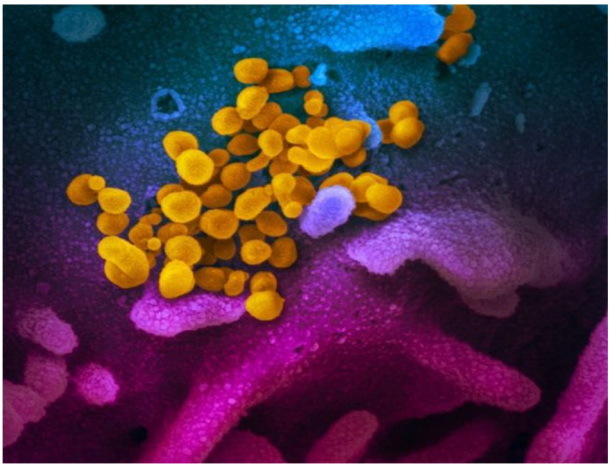
It was on 8 April in 1982, during a seemingly normal experiment in the laboratory, that Shechtman came across an unexpected and strange physical phenomenon. He was on his first sabbatical at the US National Bureau of Standards in Maryland (nowadays the National Institute of Standards and Technology, NIST) when he suddenly made his discovery after having bounced electrons onto an artificial alloy of aluminum-manganese in a transmission electron microscopy. Shechtman's findings initially perplexed him, as they showed unusual tenfold symmetries that violated the classical hexagonal symmetries in basic crystallography. In his logbook record, Shechtman stated that he wrote, "10 Fold???" and later said to himself, "There can be no such creature" (Hargittai 2011). Yet, after a few days when he had completed his experiment, he concluded that these unconventional structures were in fact real. And quasicrystals have since been defined as aperiodic solids that exhibit rotational symmetries incompatible with conventional periodic lattice order, a discovery that "has provided a paradigm shift in solid-state physics because it had long been assumed, …that the best and most stable long-ranged order should be realized in the form of a periodic solid constructed by regularly repeating unit cells" (Abe et al. 2004: 766).

But despite his fantastic discovery, Shechtman immediately became the subject of strong opposition from many people in his field, and colleagues tried to convince him that he was simply wrong. The head of the US laboratory where Shechtman spent his sabbatical put a basic textbook on his desk and urged him to read it. After a few days, he told Shechtman to leave, saying, "Danny, you are a disgrace to my group" (Jha 2013). Shechtman then also had to wait for over two years to get his pathbreaking results published. Professionally this was a very lonely time for him (Hargittai 2011). However, with some support from more open-minded colleagues at the Technion back in Israel (where he had been employed since 1975), he could finally see his work in print in 1984. But even later, this did not prevent people from further ridiculing him, especially Linus Pauling, one of the greatest authorities in the field and celebrated winner of the Nobel Prize in Chemistry. During a conference, Pauling said: "There is no such thing as quasicrystals, only quasi-scientists." In due time, however, the discoveries Shechtman made in the early 1980s would come to be widely accepted, and in 2011 he was awarded the Noble Prize for his great contribution to science.

This was a brief illustration of a story that ended well. But hypothetically, let's suppose that Shechtman's work on quasicrystals had instead been conducted by someone else, and that the project was completely dependent on funding. Suppose that this person had neither a tenured faculty position (a "protected space") with a reasonable amount of research time nor a grant to further explore his/her ideas? And what if this person lacked the stamina and perseverance to keep faith in the project and instead changed his/her subject? All of this could of course happen regardless of funding conditions and academic status. But my point here is that Shechtman would probably never have received a competitive grant if it had been up to Pauling, and the reason would have been that both the very idea and the results from the laboratory were fundamentally in conflict with Pauling's deep-rooted understanding of the nature of crystals. What if Shechtman had not had a tenured faculty position at Technion, his "protected space," would his discovery have been jeopardized? We cannot possibly know this for sure, we can only speculate. Although this example may seem extreme, it is hardly unique in the history of science. In fact, this case tells us something essential about the power that individual reviewers can exert over research ideas they do not like for some reason. A pure lottery would be completely immune to all types of epistemic biases and other types of human prejudices and could, therefore, constitute a dynamic complement to block funding. Using a pure lottery, even researchers with seemingly "crazy" or "wrong" ideas would have a better chance than they would with a normal peer review process.

Case 2: Karikó's pioneering research on mRNA

The fantastic career story of biochemist Katalin Karikó provides us with another interesting case for this discussion; it presents us with an all-too-common situation that many researchers experience today: wasting time on writing proposals and being rejected. But it is also an extraordinary and unique case that is easy to appreciate in retrospect as an academic success story, especially because it turned out to



Source: NIAID-RML Photograph showing a digitally colorized scanning electron micrographs of SARS-CoV-2 virions emerging from human cells cultured in a laboratory

help shield the world against the SARS-CoV-2 virus. Unlike Shechtman, however, Karikó did not have a "protected space" in academia, in fact she had to struggle hard to make her discovery at all. The case could therefore help us to further open up the discussion regarding the problematic opportunity mechanisms governing contemporary science.

Like many other young researchers in the life sciences during the 1980s, Karikó tried to establish herself as an independent scholar in the US. She was determined to explore new ways to modify synthetic mRNA constructs to be used for immunotherapy. In an interview with the *Proceedings of the National Academy of Sciences*, Karikó described how she, in 1989, accepted a non-tenured faculty position at the University of Pennsylvania (Nair 2021). From there on and until the mid-1990s, she spent much of her time trying to get grants to develop new mRNA therapies to cure human diseases. At one time, Karikó recalls having submitted over 20 proposals for both smaller and larger grants, but the only thing she received was rejections. Eventually, her failed efforts to convince the reviewers to support her ideas "resulted in her being taken off her faculty position by the university" (Franzoni et al. 2021: 5). This was indeed a difficult period for Karikó, who found herself downgraded to a non-faculty position at the same university and with minimal future prospects. Then one day in 1997, by coincidence, Karikó met the newly hired immunologist Drew Weissman at the copy machine, and they started talking about their common research interests in mRNA. This was the crucial moment that led to a very fruitful collaboration, which later resulted in a real breakthrough for the development of new mRNA-based vaccines. In 2005, they published their most important paper, which was preceded by a great deal of hard work, as Karikó noted: "We literally did all the work ourselves, Drew and I. Even at the age of 58, I didn't have much help or funding to perform the experiments, so I did them with my own hands" (Nair 2021: 3).

Karikó did not have any resources to use from her non-faculty position, and no grant money either, something that most likely delayed her work. But even after publication of their pathbreaking results in 2005, it still seemed as though the research community was not particularly interested in mRNA, and when Karikó later submitted an R01 proposal to the NIH, it was not even discussed during the panel group meeting, "having been judged by reviewers to be in the lower half of the applications" (Franzoni et al. 2021: 6). In retrospect, we can probably all agree that she deserved better support to pursue her research project, yet the reviewers could not appreciate the promise in her proposals. This is analogous to the large unknown population of researchers who also never receive adequate support and for whom the proposal itself becomes a real obstacle. We can only imagine all of the other researchers who, unlike Karikó, finally give up their ambitions and decide to do something else. That is a great waste of potential. A pure lottery would at least give a researcher in a precarious situation a better chance of being rewarded a grant than the peer review process does. Karikó's story is in many ways unique, but her funding situation is not. And with an increasing number of researchers competing for a limited amount of money, it sometimes seems as if the most pressing issue has been lost behind distracting buzzwords like "excellence," "innovation," "quality" and "impact." How could the expert reviewers have failed repeatedly to recognize Karikó's genuine promise? Why couldn't they see the importance of her research ideas? That is impossible to know. But what we really learned from the Covid-19 pandemic was that science, correctly orchestrated, can achieve miracles when researchers are given the right opportunities. Time management and timing will probably become even more crucial in light of new possible existential threats in the future, and having simplified and more efficient distribution techniques will be fundamental. Suppose that Karikó never had to spend time writing all the proposals that were constantly rejected. That would certainly have saved her great many extra hours that she could instead have spent on conducting new experiments in the laboratory. Then let us suppose that her home department had been able to give her at least some extra resources. That would also have contributed to her having more time at her disposal. Still, given her less privileged position, wouldn't a pure and unbiased lottery have been a better method than peer review to give her at least a fair chance?

Concluding Discussion

The recent academic debate on the possibility of using a lottery for allocating research grants suggests that partial randomization would be a good complement to peer review. Thus, some kind of modified lottery could be acceptable for distributing scarce resources as long as expert judgment is still involved in the process. As a decision-making technique, a modified lottery implies that funding agencies should first organize a peer review process (screening) to check the eligibility and pursuit-worthiness of all the submitted proposals, and then use randomization for the final selection. At first glance, this way of allocating resources may seem to be rather promising for some innovative policymakers and researchers. The problem is, however,

that such a model is not radical enough and cannot solve the well-known shortcomings associated with the current funding system (Roumbanis 2017, 2019a, 2022), which primarily uses grant competitions and peer review. In the present article, I have argued against the modified lottery, because it is too much of a compromise and it fails to make a substantial difference for the research communities that need more time. Hence, I proposed that we look beyond the predominant funding regime, with its notorious belief in grant competitions and peer review, and instead establish a more holistic perspective on research, one that recognizes the value of time. The suggestion to implement a lottery first requires that we focus more fundamentally on time management, that is, on how we can best make use of the time every researcher could potentially have at his/her disposal during a research career. The great urgency of many contemporary research issues (in medicine, climate research, AI, migration, politics, etc.) must necessarily be part of the general plans of the universities and the policy-makers, but without marginalizing what Abraham Flexner (1939; the Founding Director of the Institute of Advanced Studies, Princeton) famously praised as the curiosity-driven, "pursuit of useless knowledge." The truth is that science is both ordered and chaotic, intertwined by our fundamental desire to understand; the difference between pure and applied science are often, from a historical perspective, divided only by a very fine line. In other words, our new mission must be to attend much more carefully to the hidden potentials embodied in all the highly educated researchers working at every university department and research institute. To focus on time management, we must also reconsider the very meaning of deservedness in academia by replacing an overly naïve meritocratic-careerist mindset with deeper humility when judging the promise and pursuit-worthiness of different projects. I used the case of Karikó to underscore the problem of a researcher being repeatedly judged to not be qualified enough or to not have an idea worthy of funding, even though she obviously had great potential to make a very important contribution. But the reviewers could never appreciate her true promise in the proposals (perhaps some of them could, but not as a group). This is a well-known dilemma that Travis and Collins (1991: 335) brought our attention to when they talked about the grant peer review process as "a blackball system whereby one poor grade can damn a proposal." Experienced researchers are certainly capable of judging the difference between a highly unorthodox research idea (or "crazy," in Niels Bohr's sense) from a merely ridiculous one, but that is not really the main problem. What makes the situation so difficult with today's peer review processes, is the way in which expert judgments are merged and transformed; it represents an evaluation technology that prevents many bold and/or seemingly unfashionable research ideas from being supported (Roumbanis 2017, 2022). More importantly, Karikó is certainly not alone if we consider today's growing population of researchers, and given that the acceptance rate at many of the major funding agencies is between 10 and 20 percent. If we turn the perspective around, this means that over 80 percent of the active researchers who need support to do their work are rejected every year and have thus wasted time writing lengthy proposals, which in effect delays scientific progress. This will always mean bad timing for the majority of researchers, and timing is often crucial in many respects. Instead, future research funding policy should be based on a basic trust in researchers, enabling them to dare to explore their ideas and even to fail

without the fear of not following a conventional career path, with a steady publication record.

In my view, one first decisive step must be to dramatically increase block funding to recreate and facilitate more "protected spaces" in academia, thereby helping a greater number of individuals pursue their research (Laudel 2017). Using a distribution model where government block funding is increased to 60-70 percent (in Sweden today, that figure is only around 45 percent) would allow university departments and researchers to utilize their time in a more flexible manner. This would without a doubt save a great deal of time and energy as well as potentially create a more pluralistic epistemic landscape in the future. The available research budget could be distributed more equally up to a certain level, then be combined with a new pluralistic performance system and predetermined career ladders. Academic performance should not only be assessed based on conventional research outputs–for example, number of articles, journal rankings, previous funding, prizes, or patents–but also emphasize other kinds of contributions. What I have called "Socratic" contributions to the research communities should also be properly valued. Here I am thinking about intellectual skills that can enhance the academic dialogue during research seminars and workshops, the reading of colleagues' article/book drafts, or being a good mentor to younger colleagues, etc. These values are actually invaluable for intellectual progress and should therefore also be incentivized and prioritized by academic communities.

Finally, we have to reconsider using a pure lottery as a different and unconventional type of complement to block funding. There is one foundational question that will always remain: Is letting chance determine how to distribute opportunities among researchers really a realistic and responsible solution? First, a pure lottery could actually add an extra dynamic dimension to the funding context by spreading 30–40 percent of the remaining part of the annual governmental budget to less established researchers. This would absolutely be the most responsible way to allocate resources when you cannot know for certain where to draw the line. Moreover, given that less established researchers would have more time if block funding were increased and would not have to spend their valuable time on writing proposals, they would already have an improved situation. The more established and successful professors would have enough time built into their university employment, while teaching duties and other administrative burdens would be more evenly assigned. Eligibility for joining the funding lottery would be based on fair rules, resulting in more efficient time management and a cooling down of the worst kinds of "Matthew effects." As opposed to a modified lottery, which would still rely on peer review to weed out the weakest proposals, a pure lottery would not require proposals to be written at all. That would naturally save a considerable amount of time for everyone in academia, and those eligible to participate in the funding lottery would be assigned a personal number. A pure lottery is totally unbiased and gives all participants an equal chance to receive a grant. There could hardly be a more equitable method of distributing opportunities to researchers who would most likely have failed to get support from a panel of experts. Suspending judgment when it comes to the delicate task of giving

highly educated people the opportunity to contribute to science means demonstrating humility in the face of the unknown.

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Conflict of interest I have no conflict of interest.

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