

Editorial

I won a project!

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Yes, I know that playing the lottery is one way to pay taxes for those who do not know statistics. But in the future, we may “win in a raffle” to do many things, such as being a member of a board of directors, a councilman, or even a member of parliament because in the future it is very likely that councilmen, deputies and many other public positions will be chosen randomly. There is a controversial but solid theory supporting that randomness is one of the best mechanisms for optimizing selection processes^{1,2,3,4}. Scientists have already begun to test this idea, and in fact, they may already get a project if they present it to an interesting program of the Volkswagen Foundation called EXPERIMENT!⁵

The program “EXPERIMENT! In search of bold research ideas”⁶ aims to fund radically new scientific ideas, ideas that go against the dominant thinking in a scientific discipline, crazy ideas or ideas of dubious feasibility that would have no or very little chance of being selected in the classic science funding program. Projects cannot formally last more than eighteen months and have maximum funding of one hundred and twenty-thousand euros. The program started in 2013 and is an absolute success. Every year, the Foundation receives around six hundred applications, prescriptively German.

Six hundred and forty applications have been received this year.

The internal evaluation team of the Volkswagen Foundation selects one hundred and fifty of the most scientifically daring proposals, those best suited to the objectives of the program. Subsequently, these one hundred and fifty proposals are evaluated by a panel of ten scientists from different countries in the world, except Germany. This panel of experts rejects a few of those one hundred and fifty applications that for some important reason should not be funded by this program, mainly because they are not radically new or because they are obviously viable. Finally, out of all the others, the panel selects the fifteen that it considers the best, and which will be financed by EXPERIMENT! It is easy to see that selecting fifteen proposals, out of a hundred and fifty that have been selected from more than six hundred applications, is very complicated for an expert, not to mention agreeing on them with the other nine colleagues on the panel. To avoid endless discussions, each member of the panel has a joker, a wild card – which can only be used once – to approve a specific project, thus putting an end to the discussion about that project.

The Volkswagen Foundation tries to ensure that the selection is as impartial as possible. For example, the system is double-blind: neither the candidates know the panel members nor the panel members know who the candidates are. There are no names of people or institutions on the forms, and the foundation itself takes care of deleting any possible data from the proposal that could be used to identify the candidates’ names, age, genre, or university of origin. But even so, the existence of a problem of equanimity derived from the enormous competitiveness of the program has been detected.

When experts evaluate and compare those ca. one

¹ B. Henning, *The end of politicians: Time for a real democracy*, 2017.

² L. Carson, P. F. L. Carson, B. Martin, *Random selection in politics*. Greenwood Publishing Group, 1999.

³ O. Dowlen, *The political potential of sortition: A study of the random selection of citizens for public office*. Andrews UK Limited, 2017.

⁴ G. Delannoi, O. Dowlen, *Sortition: Theory and Practice*. Andrews UK Limited, 2016.

⁵ The non-profit Volkswagen Foundation is the largest private foundation for research and academic teaching in Germany, spending more than 200 million euros in 2018. Despite its name, it is independent and not affiliated with the automaker company.

⁶ <https://www.volkswagenstiftung.de/en/funding/our-funding-portfolio-at-a-glance/experiment>

hundred and forty research proposals that they have considered, in principle, eligible for funding by the program, they always find some of them outstanding, which should be clearly funded. Let us say there are five of them. However, when it comes to selecting the other ten that can still be funded, they find that there are many more than ten proposals that are so good that it is technically impossible to decide which of them is better than the others. And that's when problems arise. When the differences between projects are small, when it is difficult for an expert to assess the superiority of one project over another objectively, aspects come into play that are subjective to the evaluator and that cause the rational evaluation system to fail. Among these factors is the tribal instinct of scientists, that is, the irresistible tendency to support those projects that are closer to their discipline and their way of thinking, what we could call intellectual nepotism. In addition to introducing injustice in the evaluation, this bias favors the most common disciplines over the rare ones, reducing the thematic diversity of the selected proposals.

In order to tackle this problem EXPERIMENT! has, for the last two years, launched an experiment that may seem too daring to some. But that's what this program is all about! The experiment consists in selecting not only the fifteen projects by the panel of experts but also an identical number of projects by lottery. Not among all the projects submitted, but among all the projects considered eligible for funding by the panel, including the fifteen approved for their technical quality in the opinion of the evaluators. That is to say, fifteen projects are selected by technical evaluation of the experts and fifteen projects by pure chance, by lottery. A total of twenty-five projects have been selected this year because, during the lottery, projects already approved by the panel can be awarded. Only a list of the twenty-five projects is made public without revealing which were selected by the panel and which by lottery, and the follow-up and treatment that the Foundation will make of all of them will be identical. The comparative study of the benefits of the two selection systems will be carried out by an external evaluation company. We will see what comes out of this trial, the first to be conducted with a significant number of projects.

The idea of raffling project funding repels the academic world. Accustomed to peer review, i.e., decisions about the quality of a paper (to be published) or a project (to be funded) or a researcher or professor (to fill a position) are made by experts of the same rank as the candidates, the proposal that an entire academic effort be the subject of a lottery draw, abandoned at random, seems unfair, irrational, even obscene. However, precise-

ly one of the stronger points of the lottery system is the cost/benefit ratio for the researcher as well and for the advancement of science.

A study has recently been published which concludes that when calls for funding research projects are very competitive, the effort researchers waste in writing their proposals may be comparable to the total scientific value of the research they intend to support⁷. The authors of the study themselves suggest that it would be more effective to replace peer review with a partial system of lotteries – such as EXPERIMENT! or to fund on the basis of researchers' past scientific successes rather than on their research proposals for the future.

Of course, many considerations can be made about the goodness of a lottery funding system. It depends on the external framework in which the researcher operates, the type of research program, the length and difficulty of the application forms, the number of calls to which a researcher can apply in a given country, the reasons for which it is presented, whether merely scientific or rather promotional, etc. But, in my opinion, the draw system is not unworthy and it should be investigated on which context its effectiveness depends and which modifications would optimize it. It should be explored as what it is, as a complex system, and its behavior analyzed with numerical simulations and the analysis of real cases such as the EXPERIMENT! program. And, of course, the equations “selection by peer review = fair and rational” and “selection by lottery = unfair and capricious” should be forgotten: the lottery comes into play when the technical evaluation system by peer review ceases to be fair and effective, and not to replace it but to improve it.

Nowadays, the use of chance in the management of public affairs is reduced to popular juries in some countries. However, the lottery selection mechanism has been used in many moments of history by political systems that have worked well, from classical Greece to the prosperous and stable republics of Venice or Florence⁸. In the outstanding Greece of the 6th century B.C., practically all public positions were chosen by lottery. Even army positions, excluding, for reasons of efficiency, those of the highest rank. The lottery system was widely used in the selection of public offices in Florence in the fourteenth and fifteenth centuries, and even the *doge* of Venice, as well as many of the public and elective offices of the city of the Signoria, were chosen by a complicated

⁷ K. Gross, C. T. Bergstrom, Contest models highlight inherent inefficiencies of scientific funding competitions. *PLoS biology*, 2019, vol. 17, no 1, p. e3000065

⁸ B. Manin, *The Principles of Representative Government*. Cambridge University Press, 1997.

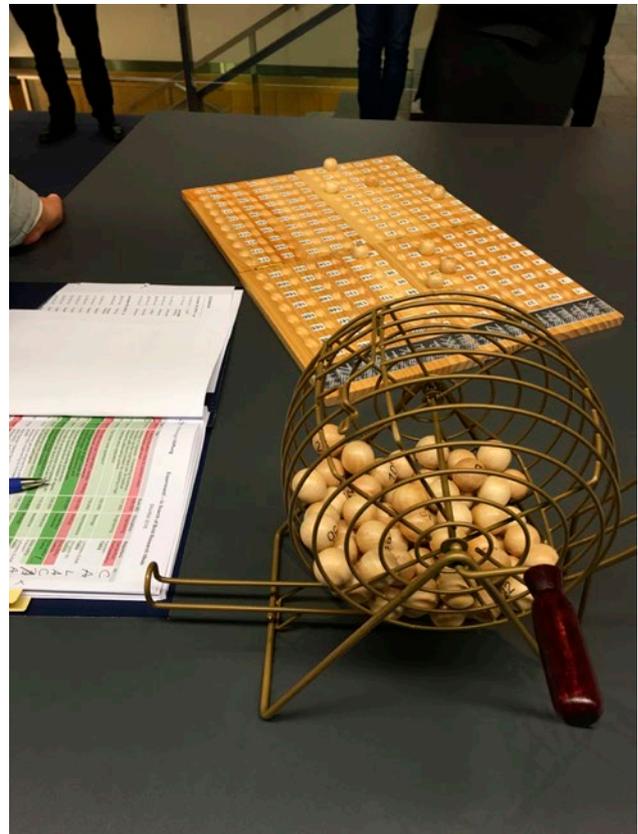
system that included largely random selection⁹.

The advantages of the random selection system are many, since, for example, it complicates corruption and bribery, makes factions useless, makes unnatural agreements impossible, disqualifies long-term promises, and reduces electoral expenditure to almost zero. Imagine a congress in which deputies were elected at random. Imagine a lady from Spain, a farmer, a lesbian chosen by pure chance to be member of the European Parliament. She could not say “we lesbians think”, nor “we women farmers believe”, nor “we Spanish want”, because she would realize, or they would make her realize, that she is not there representing anyone except herself and that the strength of the system is that each of the raffled seats in the Parliament votes and decides in their own conscience, for their own interests. That sum of non-prostituted interests is what gives strength to the lottery election mechanism. But let’s leave the management of public affairs for another time, and let’s return, to finish, to the academy, that is what interests me now.

In my opinion, the most worrying thing about the evaluation of EXPERIMENT! is how to make an objective and relevant comparison between the two groups of projects funded, those selected by the panel of experts and those selected by lottery. As we made clear at the beginning, this program is looking for bold, daring, doubtlessly viable projects based on ideas that move in the diffuse and changing frontier of knowledge. How to evaluate the results of projects that by their very nature should fail in most cases? What criteria should be used to qualify the productivity of a project that is going to explore a niche not yet trodden by science? This problem is totally new in evaluation and its solution is nothing trivial.

On the other hand, the result of the comparison will be very dependent on the composition of the panel, on the selection criteria of its components. When we had to design the evaluation system for the EXPLORA Program – dare to discover, dare to be wrong – a pioneering Spanish program in the financing of bold ideas, it became clear that the database of the National Evaluation Agency should not be used. The reason is that this task requires colleagues who are open-minded, non-egocentric, intellectually generous, with excellent scientific culture, and if possible with a certain sense of smell to detect in a proposal the semi-hidden potential that straddles the genius and the naive. We have to look for evaluators who would have bet on Columbus, on Marconi, on Wegener. That is not easy. Only nine years

ago, during the evaluation of a program for bold ideas, an advanced facial recognition project and another one about crypto currency were rejected, because they were useless (who’s going to be interested in that?). The role of the panel of experts is crucial because the final list of projects selected by these programs where the intellectual risk is assessed is the only, or more precisely, the best message that can be sent to future candidates to convince them that, fortunately, there are programs that don’t care about financing failure if the frontier of knowledge is explored with audacity.



ACKNOWLEDGMENTS

The author acknowledges the team of the Experiment! Program of the Volkswagen Foundation, and Dr. Enrique Perez (Institute of Astrophysics of Andalucía) for useful discussions of this subject. Dr. Alfonso García-Caballero is also acknowledged for help with the English version of the manuscript.

⁹ J. S. Coggins, C. F. Perali. 64% Majority rule in Ducal Venice: Voting for the Doge. *Public Choice*, 1998, 97(4), 709-723. <https://doi.org/10.1023/A:1004947715017>