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A dispersion-driven method for grant and proposal allocation

N. Mirabal and J. L. Contreras
Dpto. de Física Atómica, Molecular y Nuclear
Universidad Complutense de Madrid
Madrid, Spain

Inspired by a recent editorial (Langer 2012), we suggest a relative simple scheme to grade grants and proposals that might enhance scientific innovation.

Innovation should be at the forefront of research practice. Unfortunately, it is easy to fall into the habit of repeating the same type of experiment over and time again (Langer 2012). The latter is not inherently bad since it guarantees safe returns, but it does not necessarily lead to innovation. Unfortunately, under most circumstances, peer reviewers tend to lean heavily towards ongoing experiments and improvements of verified results. Recent analysis shows that papers initially rejected received significantly more citations than those accepted on their first attempt (Calcagno 2012). These results highlight the valuable input received from reviewers, but it also reflects the dangerous side of scientific consensus. Possible alternatives to the current funding scheme include choosing fundable proposals by a lottery (Langer 2012), or providing funds to individual centers of excellence without ties to specific goals (Loeb 2012).

We recognize that peer review is a necessary overseer and it is not obvious that giving away with it completely will provide direct improvement. However, some additional fine tuning might help recognize innovation. New ideas tend to be controversial and might take time to digest. In 1933 Fritz Zwicky had collected evidence to postulate dark matter (Zwicky 1933), but his work was not broadly accepted until the 1970's. Practically – in many modern day peer review rooms – funding requests to carry out Zwicky's original research might have lead to a broad range of grades. It is reasonable to assume that at least one peer reviewer might have deemed Zwicky's findings extraordinary. But low grades might have blurred the high grade and probably Zwicky's proposal would not have passed the first round of discussions. A direct way to reveal this type of anomaly is to measure the dispersion of the grades, in its simplest version through their standard deviation. Assuming the qualifications of all reviewers are equal, we suggest that the final decision on grants and proposals should not only consider average grade but also its dispersion.

But how to allocate funds in this fashion? For that, Loeb (2010) has advanced a reasonable breakdown into safe and risky investments that might guide us here. Adopting such division, 80% of grant/time allocation would go to secure research *i.e.* proposals with the highest average grades. The remaining 20% could be divided among the risky but with potential high gains *i.e.* proposals with the highest standard deviations. Percentages and weights given to each variable can be adjusted if needed.

Certainly, a number of risky ideas that turn out to be groundbreaking in the long run might go unnoticed if no reviewer deems it extraordinary. For those, only perseverance and resubmission will find a reward. Finally, an argument could be made that such scheme would improve the health of the reviewing system. Presently, reviewers understand the devastating effect that a single bad grade has on the fate of a proposal, and may feel pressed to artificially inflate their grades. Knowing that an alternative scheme is in place might relieve them from this pressure and make grading more reliable. On the other side, proposers might also have greater incentive to be creative if there are improved odds for innovative proposals.

This scheme is by no means the only solution to the problem nor does it guarantee innovation. At present, it is just a modest proposal to enrich the pool of ideas meant to help promote it.

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