A new example of what not to do in science

A well-known example of what not to do in science is to publish one's research results not in a single paper, but in as many small papers—called Least Publishable Units (LPUs)—as possible. This is advantageous because researchers are nowadays judged by the sheer number of their publications, but the downside of it is that it is then no longer immediately evident for the community what the results of the research are. Based on a recent development, this paper introduces the new concept of a 'Least Interesting Unit' (LIU) as an investigation that is not entirely uninteresting, but just interesting enough to pursue. It is argued that pursuing as many LIUs as possible is then a new example of what not to do in science.

A recent development in the way to do theoretical physics is that it has been suggested that physicists should stop working on grand ideas, because of the risk that it leads to a pile of theories that cannot be tested in the nearby future: instead, theorists should focus exclusively on ideas that lead to an immediately verifiable prediction of a new phenomenon, even if they know beforehand that the idea cannot be correct—which makes the idea, of course, less interesting. This suggestion has been made to create a "win-win" situation: physics "wins" because it's an advancement in physics every time when a newly predicted phenomenon gets observed, and the theorists "win" when his prediction gets confirmed—such a shift towards a purely pragmatic job execution thus clearly enhances one's career perspectives in academia.

The aforementioned concept of a LIU is introduced to capture the essence of the above specific development in physics in more general terms: that enables a more general discussion of the ethics involved, that is, a discussion of the right and wrong involved that is not limited to this specific development and not limited to this specific branch of science called physics. Thus speaking, such an investigation into an idea that is not very interesting because we know beforehand that it cannot be correct but that is just interesting enough to pursue because it predicts a new phenomenon, can be viewed as an example of what an LIU is in theoretical physics. And the suggested new norm in physics, that theorists should henceforth focus their work exclusively on such ideas, can be viewed as an example of a norm that one gets when everybody is to pursue as many LIUs as possible in theoretical physics.

That being said, consider the situation in which it has become the new norm in all branches of science that every researcher should pursue as many LIUs as possible. There is a good side and a bad side to it. On the one hand, advances in science become a sure thing: as researchers devote their time to a series of easy-to-accomplish ideas, instead of to one time-consuming breakthrough idea, then there will be a higher number of contributions to science. But on the other hand, these contributions are minimal: that means that in this situation, valuable resources will be spent on mediocre ideas instead of on potential breakthroughs. In addition to that, giving up on the grand ideas is like giving up on science: it is true that most of these ideas turn out to be wrong, but the few that were correct have virtually determined the entire historical development of science. So concluding, pursuing as many LIUs as possible is a new example of what not to do in science.

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