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Investing in Fundamental Research: For Whom? A Philosopher's Perspective



Michela Massimi

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1 The Importance of Investing in Fundamental Research

In uncertain economic times and a volatile political landscape, the question of our panel discussion might strike as otiose. Faced with major and pressing challenges—from climate change to biomedical research, from cybersecurity to agrotechnology, just to mention some examples—our society seems already to be struggling in meeting research targets that affect millions of people around the globe. Why should we care about fundamental research? Tight national budgets often force hard choices about which kind of investments should be prioritised. And if cuts have to be made, investments in fundamental research tend to be the first ones in the line.

In what follows, I make some brief remarks about the importance of fundamental research for society—be it fundamental research in particle physics, cosmology, or other areas. I will make some specific comments about how I see fundamental research contributing to human cultural flourishing and conclude with some reflections about scientific progress in pursuing fundamental research.

A ground-clearing remark is first in order, though. Fundamental research is a misnomer, and on occasions, an unfortunate one too. For it suggests that research comes in two varieties: the fundamental and the non-fundamental. Or, the abstract and the applied, to use another dichotomy. Thus, posing the question about the value of fundamental research inevitably invites a wave of scepticism if the underlying assumption is that a choice is forced upon us between the abstract and the applied.

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But reality is a lot more nuanced than this dichotomy might suggest. For the boundaries between research into the foundations of a particular field feed seamlessly into technological innovation. And technological innovation in turn informs directions of research in fundamental areas. High energy physics is a case in point. It is for example well-known that innovations such as the World Wide Web were developed by scientists working at CERN. Technology such as the PET scan were spearheaded at CERN while developing new technology for particle physics. And more recently research on proton therapy has been carried out at CERN in the fight against cancer.

But leaving here aside these general considerations, there are other reasons as to why investing in fundamental research is important. These general considerations have got to do with what philosophers often describe as a duty of care that we have towards our ‘later’ selves, or the next unborn generations.

Imagine someone’s life as a slow motion cartoon where if the speed is sufficiently slow, you can almost see the individual snapshots the cartoon consists in. Each of these snapshots is indexed at a particular time $t_1, t_2, t_3, \dots, t_n$. The question arises as to whether choices made at a particular time, say t_3 should be done with an eye to benefitting the subject *at the time*, or with an eye also to benefitting the later self at time t_n . For example, one might enjoy smoking a cigarette at time t_3 and be careless about the long-term consequences of her/his choices at t_3 . Or one might become mindful of the long-term health risks that such action might engender at t_n and decide at t_3 to cut on smoking if at t_3 one cares enough about one’s later self and wellbeing at t_n .

Philosophical theories of personal identity and intergenerational justice depend on how we might be inclined to answer temporally-indexed questions of this nature. If we see ourselves as part of a continuum spectrum, where actions and decisions taken at any particular time are bound to affect the well-being and flourishing of our later selves at later times, then I think an easy answer is available to our overarching question, i.e. “Investing in fundamental research—for whom?”. The simple and straightforward answer is: “For *our later selves*”.

Investing in fundamental research is indeed for our later selves, and for the next generations. More broadly, it is for *humankind*. We do not invest in fundamental research for the sake of some immediate economic return that someone somewhere is directly going to benefit from. But for advancing scientific knowledge, for exploring uncharted territories, and for making progress in our collective understanding of the natural world we live in.

Thus, in a way, this is a very long-term and open-ended kind of investment. And in assessing the value of this kind of investment, in the light of the aforementioned duty of care towards our later selves, my inclination is to warn against a principle that economists tend to use all the time: the *principle of discounting the future*. The economic principle that a dollar today is more valuable than a dollar tomorrow is a bad principle for assessing the *intrinsic value* (as opposed to the cost-effects economic value) of scientific research in general, I think. This is the case no matter whether the research in question is about climate science, cancer research, or fundamental physics. We have a duty towards future unborn generations to study and understand the effects of anthropogenic climate change, to study and monitor the long-term risks of smoking cigarettes, as much as we owe to them a better understanding of the natural world we live in.

One reasonable concern one might have (and some people do indeed have) about investing in fundamental research is not however (or is not only) about the economic costs attached to it, or the long-term risky returns of the investments. In a way, investments of this nature invite us to pause and think about the progress made in the specific area, and to ask ourselves the extent to which further funding is warranted. We live in a society that is obsessed with metrics and ways of measuring progress towards targets. And when it comes to fundamental research in pretty much open-ended areas—be they particle physics or cosmology for example—even setting targets to achieve might prove very difficult.

Because of its very nature, fundamental research tends to be exploratory and open-ended, with targets that ought to be realistic but at the same time are as revisable and as open-ended as the field of inquiry itself. For it would be naïve at best to expect fundamental research to be railroaded in some predefined way towards pre-given fixed targets. Some of the most important breakthroughs might happen in unexpected ways, while some pre-given targets might prove unachievable, after all.

Here I think we need to be careful and not fall prey to a very common but in my view short-sighted view of how to assess scientific success and progress when it comes to fundamental research. There is a deeply instilled tendency to measure scientific progress and success in any given area in terms of numbers of scientific discoveries and the potential for discovery; by setting milestones and monitoring how well we have marched to achieve those. Think of it. It is a pervasive view that science progresses by discovering new things, or by inventing new tools or enabling new technological innovation. We discover a new particle; patent a new vaccine; unveil a new phenomenon.

Unsurprisingly, a sense of frustration tends to accompany areas of science where the notion of progress does not necessarily conform to this received and somewhat intuitively satisfying notion. High energy physics is one of those areas where frustration might occasionally become tangible among practitioners and the general public too. Will we find new physics beyond the Standard Model? Will the new generation of colliders deliver on the promise of finding new phenomena and shed light on various unsolved puzzles? Given the open-ended nature of fundamental research how to precisely answer these questions is far from trivial and obvious.

But here is my positive take-home message. Even in the worst-case scenario, even if we were not able to discover new particles, new phenomena, new physics beyond the Standard Model as particle physicists hope for (and have reasons for hoping for), we would still have made progress. Because scientific progress is not necessarily (or exclusively) discovery-driven. Progress in fundamental research is not just about finding out what is actual, but also (and equally importantly) is about *delimiting the space of what is possible*. Progress in HEP often means being able to rule out live options, carve out the space of what is reasonable to expect, or of what we believe to be possible to the best of our knowledge. That is what physicists at the LHC, CERN, have been doing over decades. They have fixed more rigorous constraints to rule out possible candidates for Beyond Standard Model physics. They have run high-precision measurements to refine our understanding of the Standard Model. They have clarified where the boundaries of the current space of possibilities lie. This is progress in physics and in science, more in general.

Consider an analogy with cancer research. Yes, we have not yet found as of today (July 2020) a final and definitive cure for cancer. But should we then conclude that there has not been progress made in fighting cancer? Of course, there has been huge progress made in better understanding the mechanisms of carcinogenesis and the specific details of particular kinds of cancers. Such understanding has advanced our ability to have targeted treatments and to improve the overall prognosis for millions of people worldwide.

Along similar lines, we might not have found as I write this piece (in July 2020) the key to various outstanding mysteries about the low mass of the Higgs boson and the nature of dark matter, among many others. But we do have made huge progress in better understanding the gaps in the existing Standard Model, in conceiving possible theoretical solutions for them, ruling them out too, and in setting more rigorous constraints on what might be possible on the basis of the available evidence. This is progress. Indeed this is the type of progress that warrants further funding investment even if the field is exploratory and open-ended.

The real question then becomes the following. How can we make sure that such investments are responsible and engage with local communities? How can we design relevant infrastructures that can be used and reused? And can we supervise and monitor the training and education of the next generations of scientists and make sure they gain a set of skills that are transferable and with some clear pathways for a wide range of applications? These are pressing and open questions to which I hope philosophers of science will contribute more and more in this ongoing dialogue and engagement with physicists.

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