The Pursuitworthiness of Experiments

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Abstract: When scientists decide to perform an experiment, they expect that their efforts will bear fruit. While assessing such expectations belongs to the everyday work of practicing scientists, we have a limited understanding of the epistemological principles underlying such assessments. Here I argue that we should delineate a "context of pursuit" for experiments. The rational pursuit of experiments, like the pursuit of theories, is governed by distinct epistemic and pragmatic considerations that concern epistemic gain, likelihood of success, and feasibility. I argue that, beyond the theoretically motivated research questions an experiment aims to address, we must also assess the concrete experimental facilities and activities involved, because (1) there are often multiple ways to address a research question, (2) an experiment may be particularly pursuitworthy because it addresses a combination of research questions, and (3) experimental facilities may give rise to research questions in the first place. In this sense experimental pursuitworthiness has a 'life of its own.' My claims are supported by a look into ongoing debates about future particle colliders.

Keywords: Pursuitworthiness, experiments, particle physics

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1 Introduction

When scientists decide to perform an experiment, they expect that their efforts will bear fruit.[2] While assessing such expectations belongs to the everyday work of practicing scientists, we have a limited understanding of the epistemological principles underlying such assessments. Here I will argue that we should delineate a "context of pursuit" for experiments. More precisely, I will take up a distinction between the context of acceptance and the context of pursuit originally introduced by Larry Laudan (1977) and explore its viability for evaluating experiments. Introducing pursuitworthiness as a distinct mode of appraisal for theories has been fruitful for our understanding of scientific methodology. I will argue that the same holds for the appraisal of experiments.

The main purpose of Laudan's introducing the context of pursuit for theories was addressing an issue of scientific rationality: without the context of pursuit, it remains mysterious why scientists should pursue new theories, so that such theories can accrue support, and can challenge dominant alternatives. However, scientific rationality is not limited to the development of theories. The rational pursuit of experiments, like the pursuit of theories, is governed by distinct epistemic and pragmatic considerations that concern the epistemic gain, likelihood of success, and feasibility.

What exactly should we evaluate when we assess experimental pursuitworthiness? In what follows I will argue that we should distinguish between theoretically motivated research questions, on the one hand, and experimental facilities and activities, on the other hand. Theoretically motivated research questions arguably play an important role in justifying experimental pursuits. However, an adequate picture of experimental pursuitworthiness cannot be achieved by looking at such questions alone. Scientists instead are concerned with the pursuitworthiness of experimental facilities and activities, for three reasons. First, there are typically multiple ways of addressing an individual research question. Second, an experimental facility may be particularly pursuitworthy because it addresses more than a single research question. Third, experimental research may be motivated by previous instances of successful experimentation and thus give rise to new research questions in the first place.

Thus, the paper contributes to the meta-methodological reflection of pursuitworthiness by motivating a closer look at experimental facilities and activities in addition to theoretically motivated research questions. The novel perspective advanced here will motivate *broadness of applicability* and *continuity with extant experimental practices* as important pursuitworthiness criteria for experimental endeavors. When it comes to assessing a specific project's pursuitworthiness and concrete funding decisions,

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however, these criteria will have to be balanced with other criteria such as an experiment's expected precision, innovative potential, and its costs.

My claims will be supported by a look into current discussions about future particle colliders. Particle colliders are huge experimental facilities that involve project planning and decision-making that can affect research agendas for several decades. Consequently, particle physicists engage in detailed and explicit evaluations of the promise of such facilities. This makes these endeavors an excellent case study for philosophical discussions of experimental pursuitworthiness.

In section 2, I will revisit the origins of contemporary pursuitworthiness discussions: Laudan's distinction between the context of acceptance and the context of pursuit. Philosophers who have taken up the distinction have almost exclusively applied it to scientific theories, largely neglecting the role of experiment as an independent element of scientific advancement. I will argue that this is an unfortunate lacuna. But what is the basis for *ex-ante* assessments of experimental pursuitworthiness? To address this question, we first need to spell out what such evaluations would ideally amount to. In section 3 I will introduce a prima facie plausible approach: scientific experiments are pursuits in contexts with scarce resources. Thus, they should be evaluated in light of the expected epistemic gains they can generate and their costs and feasibility. In section 4 I employ this approach to introduce and clarify a distinction between theoretically motivated research questions, on the one hand, and experimental facilities and activities, on the other hand, and I provide preliminary reasons against an exclusive focus of pursuitworthiness evaluations on research questions. In section 5 I will present current discussions about future particle colliders as an example that supports my claims about the pursuitworthiness of experimental facilities and activities. In section 6 I will discuss some potential objections against the applicability of the "context of pursuit" to experiments.

2 Two contexts of appraisal and the pursuitworthiness of experiments

The concept of pursuitworthiness can be traced back to a distinction between two contexts or modes of appraisal, introduced by Laudan (1977): the context of acceptance and the context of pursuit. According to Laudan, in the context of acceptance scientists are concerned with selecting "among a group of competing theories and research traditions" the one that is to be treated "*as if it were true*" (1977, 108). This mode of appraisal is applied, for example, when scientists consider employing a theory for designing further experiments. For instance, they apply it when they decide whether it is safe to administer medication to a volunteer in a randomized trial, or how measurement devices should be designed. In the context of pursuit, by contrast, scientists decide which theories and research traditions to work on, investigate, or explore. According to Laudan, these are often theories and research traditions that are “patently less acceptable, less worthy of belief, than their rivals” (110).

According to Laudan, the two modes of appraisal are governed by two criteria. What matters for the acceptance of a theory is whether it represents *progress*. This, in turn, depends on whether the total number and significance of problems (both empirical and conceptual) it solves is larger than that of all competing theories. What matters in the context of pursuit, by contrast, is the *rate of progress*. According to Laudan, “it is always rational to pursue any research tradition which has a higher rate of progress than its rivals” (1977, 111). Even if a research tradition T1’s current problem-solving capacity is lower than that of another tradition T2, T1 is pursuitworthy if the rate at which solutions are produced by T1 is higher than T2’s rate of problem solving (and that of other rivaling traditions).

In Laudan’s account the primary object of appraisal are research traditions, which explicitly include methodological commitments (see Laudan 1977, 81) and, arguably, commitments as to what kinds of experiments are to be performed. Laudan’s distinction and similar ideas have been discussed by many philosophers of science. However, the focus of this literature has been the pursuitworthiness of theories and the conceptual part of research traditions, but not of experiments.

Whitt (1990; 1992), for example, discusses “indices of theory promise”. As formal indices she identifies a theory’s analogies and experimental strategies. Analogies have important heuristic value insofar as they “will direct scientists to the resolution of a particular subset of empirical problems within the theory's domain” (1992, 621). Experimental strategies are important insofar as they can be employed to address empirical problems.

Likewise, Šešelja and Straßer (2014) provide a coherentist approach to epistemic justification in the context of pursuit with an exclusive focus on “theory pursuit”. This has consequences for the kinds of indicators of promise that Šešelja and Straßer identify: potential consistency, potential inferential density, and potential explanatory power (ibid. 3122). According to Šešelja and Straßer, these indicators track the potential coherence of a theory, that is, the degree to which a theory may exhibit coherence in the future if it is further developed. This concept of coherence applies to theories and other cognitive elements of scientific research, but it is not obvious how it would be applied to the context of experiment.[3]

Recent discussions have explicitly addressed the various kinds of items that considerations of pursuitworthiness are concerned with. Šešelja et al. (2012) distinguish scientific theories, epistemic objects, and technology. Others have put a focus on the pursuitworthiness of scientific questions (Wilholt 2020; DiMarco and Khalifa 2022;

[3] For a notion of coherence that applies to experimentation see Chang’s (2017) concept of ‘operational coherence’ which is “about the harmoniousness of actions, not primarily about the logical relationship between propositions” (pp. 108 f.).

Barseghyan 2022) or ideas (Shaw 2022; Duerr and Fischer 2025). While these latter items also cover the empirical part of the research process (experimental *questions*, *ideas* for experiments), discussions of experimental pursuit have not been at the focus of these studies. [4]

The neglect of experimental pursuitworthiness is a worrisome lacuna for two reasons. First, questions of theoretical pursuitworthiness *depend on* questions of experimental pursuitworthiness. Consider Whitt's account that identifies "experimental strategies" as a central formal index of theory promise. Her evaluations of theory promise depend on the concrete experimental strategies that are suggested by the theory. The concrete features that make experimental strategies supportive of a theory's promise, however, remain implicit. There have also been several case studies from theoretical physics, especially from areas in which empirical input is hard to get by as in String Theory (Camilleri and Ritson 2015; Cabrera 2021), Beyond the Standard Model particle physics (Chall 2020; Fischer 2024b; King 2025), and cosmology (De Baerdemaeker and Boyd 2020; Wolf and Duerr 2023; 2024). We will see that the pursuitworthiness of experiments is important in these areas: often it is the difficulty or even unavailability of experiments that seems to make questions of theory pursuit such a pressing issue in the first place.

Second, since the years of Laudan's initial account, the philosophy of experiment has become again central to the philosophy of science (see, e.g., Hacking 1983; Weber 2004; Steinle 2016; Boyd 2021). There has been fierce debate about what might be called the *context of acceptance* of experiments. On the one hand, Collins's (1985; 2004) studies on replicability and the experimenters' regress suggest that it is difficult to establish hard criteria for the acceptability of an experiment, and that the acceptance of experimental results is a matter of negotiation and social factors. On the other hand, Franklin (1989; 1999) has forcefully defended an epistemology of experimentation in physics that involves a variety of strategies that are suited to restore trust in experimentation.

Experimentation plays more than the traditional auxiliary role as the testing arena for theory: experimentation has a "life of its own" (Hacking 1983, 215). If that is the case, one should not expect considerations of theory pursuit simply to carry over into the case of experiment. For example, we will see in more detail below that discussions of what is a pursuitworthy particle collider can be considerably independent from the pursuitworthiness of the theories-to-be-tested. Thus, the "pursuitworthiness of experiments" merits an analysis of its own. Note, however, that the independent character of experimentation also points to a fundamental challenge for discussions of experimental pursuitworthiness: if experimentation has a "life of its own", on what grounds can pursuitworthiness assessments be made?

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[4] A notable exception is Laymon & Franklin's (2022) *Case Studies in Experimental Physics: Why Scientists Pursue Investigation*, which discusses a series of intriguing case studies of experimental pursuits but does not provide an overarching account of the pursuitworthiness of experiments. See also Franklin and Laymon (2024).

3 The economic approach

The discussion here will be limited to *epistemic* pursuitworthiness, that is, an assessment of research projects regarding their capacity to advance and improve knowledge. The discussion will also be limited to one promising approach that will be applied to what will turn out to be a rich example. Towards the end of the paper, I will relate these specific discussions to more general concerns.

When it comes to experimental pursuitworthiness there is competition between various ways of achieving novelty. This has consequences for the kinds of questions that arise in experimental pursuitworthiness: the focus lies on allocating scarce resources according to what novelty is most relevant. Thus, what matters for decisions about experiments-to-be-performed are (i) the potential epistemic gains to be reaped, (ii) the *ex-ante* likelihood of achieving them, and (iii) the efforts required to achieve the gains. This calls for an economic approach to pursuitworthiness assessments, an idea first proposed by Peirce: "Proposals for hypotheses inundate us in an overwhelming flood, while the process of verification to which each one must be subjected before it can count as at all an item, even of likely knowledge, is so very costly in time, energy, and money" (cited in McKaughan (2008, 456)).

The idea of economic approaches to pursuitworthiness can be related to extant studies in research economics (see, e.g., Stephan 2012), and it has attracted some attention in philosophy of science. In particular, there have been attempts to put this idea in formal terms such that the potential epistemic gains of a research effort are weighted by the likelihood of achieving them (the expected epistemic gain, EEG) and then set off against the associated costs (see Nyrup (2015), Fischer (2024a; 2024b) for examples of such formalizations). Here we will limit the discussion to qualitative considerations (for a detailed discussion of such qualitative considerations of theory promise see, for example, Duerr and Fischer (2025)).

Let me make few comments about gains and costs. Epistemic gains can be of various kinds, including (among other things) new theories, models, and hypotheses, discoveries of novel phenomena and replication of extant studies. Epistemic gains can also be of an indirect character, such as the development of experimental methods and researcher training. Clearly, not any advancement and improvement of knowledge will be equally valuable—even from the perspective of science-internal assessment (see, e.g., Kitcher (2011), Ch. 5). What makes decisions about pursuitworthiness so difficult is, among other things, that overarching guidelines for comparing epistemic gains are very limited.

Costs are both financial and non-financial. Financial costs are the funds required for performing research, for example, for setting up and running a lab with required facilities and researchers. Depending on the level of decision-making, cost assessment (as well that of gains) will have to take various contextual factors into account. For example, if a Principal Investigator decides to perform an experiment the costs will depend on the infrastructure that is already in place in their lab. Apart from such financial costs considerations of pursuitworthiness should also take into account non-financial costs associated, for example, with moral hazards. How such costs are to be weighted in concrete decision-making is, again, hard to say at the general level.

With these qualifications in mind, the economic perspective is *prima facie* plausible from a normative perspective. Experiments *should* aim for the highest expected epistemic benefits and trade them off against the costs. Consider two experiments with equal EEG but different costs: the project that achieves the EEG at the lower cost is clearly favorable. Likewise, consider two experiments with equal costs but different EEG. The project with the larger EEG is clearly to be favored.[5] Pursuitworthiness considerations here take a comparative form. The question is whether one experiment is *more* pursuitworthy than another experiment. Alternatively, one may ask whether an experiment is pursuitworthy at all, that is, whether the experimental costs are justified by the expected epistemic gain—independently of alternative projects.

4 Experiments and research questions

In what follows, I will undertake a more detailed analysis of the relation between theoretical and experimental pursuitworthiness. Evidently, theoretical pursuitworthiness does play an important role in assessing an experiment's pursuitworthiness. But I will also show that there are important aspects in which experimental pursuitworthiness has a 'life of its own', meaning that it is not simply reducible to theoretical pursuitworthiness but has partial autonomy.

Theoretical pursuitworthiness *does* play an important role for experimental pursuitworthiness, typically, by setting the research question that the experiment addresses. Such questions can have varying degrees of specificity (see, e.g., Hughes 1982; Hilpinen 1988; Hintikka 1988). For example, they arise in hypothesis testing. In this case the experiment is directed at finding out whether the hypothesis is true or false. The experimental question will be pursuitworthy if the expected benefits of finding out the answer outweigh the costs of addressing the question. Speaking comparatively, one question Q1 will be more pursuitworthy if it achieves the same expected gain as Q2 at lower costs (or higher gain at same costs). In this case the potential epistemic gain is determined by the hypothesis. If the hypothesis is highly ambitious, the experimental question has the potential to generate high epistemic gain. If the hypothesis is unambitious there is not much to be gained by testing it. The costs, by contrast, will

[5] Often both expected gains *and* costs differ. This gives rise to additional issues that require a weighing of costs and benefits. For example, when both the expected epistemic gain and costs associated with experiment E1 are higher than those of experiment E2, one needs to decide whether the higher expected epistemic gain associated with E1 justify the additional costs. To facilitate such judgments the economic approach needs to be supplemented with additional assumptions about what constitutes an epistemic gain and the associated costs in a specific research context.

depend on the concrete experimental facilities and activities needed to answer the question.

Hypothesis testing is often contrasted with other forms of experimentation that are more explorative (Steinle 2016; Karaca 2013; 2017; Mättig 2022; Beauchemin and Staley 2024). Steinle (1997) initially characterized exploratory experimentation as a form of “systematic experimentation which is not guided by theory” (S73). So, one might think that cases of exploratory experimentation are instances in which pursuitworthiness is motivated independently of theory. However, in more recent discussions it has been acknowledged that theory can play various roles even in exploratory forms of experimentation. Mättig (2022), for example, has introduced a distinction between target, background and motivating theory. Even if exploratory experimentation is not concerned with a specific theoretical target, it may still be driven by theoretical motivation.

The overarching point here is that theoretical pursuitworthiness affects experimental pursuitworthiness in various ways, and it does so by acting as the basis for formulating a research question that can be more or less specific and may concern the confirmation or disconfirmation of a hypothesis, explorations of some specified part of parameter space, the measurement of free parameters, and certainly in many other ways.

However, we will see that only looking at individual research questions may lead to misrepresentations of an experiment’s pursuitworthiness. Evaluations of pursuitworthiness also need to address the experiment itself, that is, the concrete facilities and activities that are employed to address research questions.[6] Experimental facilities here refer to the labs, instruments, and setups that need to be put in place such that an experiment can be performed. Activities refer to the experimental procedures that are performed with these facilities. There are three reasons why this is an important complementary view.

First, for each theoretically motivated research question there is typically a variety of ways of addressing it. If one agrees on the pursuitworthiness of such a question, there may still be disagreement about the specific experimental setup that is to be prioritized to address that question. Identifying a pursuitworthy research question provides only incomplete guidance if there are no concrete recommendations for how that question is to be addressed. Experiments can differ regarding the costs that they produce while addressing the question. Moreover, there may be differences in the conclusiveness of the evidence provided for or against the hypothesis. Thus, while theoretically motivated research questions are important to set the goal of an inquiry, there are additional

[6] A quick note on terminology. The following discussion will be framed in terms of ‘questions’ versus ‘facilities and activities.’ In my view not much depends on the notion of ‘question’ here. One could instead talk of ‘ideas’ or ‘goals.’ What does matter, though, is that I take these questions to have a primarily theoretical motivation that will be contrasted with the constraints on pursuitworthiness that arise from looking at concrete experimental facilities and activities.

questions of the pursuitworthiness of experimentation that need to take into account specific experimental facilities and activities.

Second, experimental facilities can be employed to address multiple questions. In fact, one way to increase an experiment's pursuitworthiness is to make it reusable for further such questions. An exclusive focus on individual research questions bears the risk of missing out on synergies that can be achieved with an experimental facility that allows one to address a multiplicity of pursuitworthy questions.

Third, a focus on the pursuitworthiness of research questions implies what may be called a 'question-first approach.' The idea here is to ask: given a certain pursuitworthy research question, what are the experimental facilities and activities best suited for addressing that question? That approach can be reversed to what may be called a 'facility-first approach.' Given that a certain facility is available or feasible, what are the potential questions that may be addressed with it? The idea behind the facility-first approach is to capitalize on the maturity of adopted technologies and protocols to generate new research questions.[7] It should be noted, though, that this point is more speculative than the other two, especially because a facility-first approach faces two kinds of challenges. First, this approach assumes that mature technologies and protocols can to some degree be transferred between research contexts. For example, Sullivan (2009) discusses the multiplicity of experimental protocols as a potential challenge. While Sullivan's contribution draws on examples from neuroscience the argument likely applies to other fields as well. Second, maturity by itself is not a sufficient condition for generating pursuitworthy research questions. Instead, to generate new pursuitworthy questions mature technologies and protocols must be applied in some novel way.

Note that the three points mentioned here are not to be seen as criteria for experimental pursuitworthiness. Instead, they are meta-methodological considerations that motivate us to look also at facilities and activities in assessments of experimental pursuitworthiness. Each describes an aspect of experimentation that will simply not come up if experimental pursuitworthiness is discussed exclusively from the perspective of individual theoretical research questions. At the same time, the mentioned points do have consequences for the kinds of criteria that will have to be considered in concrete assessments of pursuitworthiness. In particular, the second point about facilities addressing multiple questions will motivate *broadness of applicability* as a selection criterion. Moreover, the third point will motivate *continuity with extant experimental practices* as a selection criterion. Both are criteria that would attract less attention from a purely theory-centered assessment of experimental pursuitworthiness. However, it should also be emphasized that specific assessments of pursuitworthiness (and especially decisions as to what projects to fund) will have to consider a broad range of

[7] Relatedly, pursuits can also be motivated by the availability of methods (see De Baerdemaeker's (2021) discussion of method-driven experiments).

other criteria as well, such as an experiment's precision, the costs incurred, and its innovative potential—criteria that will likely stand in tense trade-off relations with broad applicability and continuity.

The upshot here is that to assess experimental pursuitworthiness both dimensions need to be considered: theoretically motivated research questions *and* experimental facilities/activities. While the pursuitworthiness of experiments certainly depends in important ways on the theoretically motivated questions that it addresses, it also has aspects that are partially independent.

5 Promises and particles

My example is the current discussion about future particle colliders. This is a suitable example because there is an exceptionally high degree of *ex-ante* reflection about such projects—for two reasons. First, the pursuit of a project of the size and duration of a particle collider needs to be justified particularly well. Second, the designing and the planning of a future collider involves a large group of stakeholders, whose joint efforts need to be coordinated. Accordingly, considerations of pursuit need to be made explicit and documented.[8]

Issues of pursuitworthiness are pressing for high-energy physics as an experimental research program. Probing energy scales up to the electro-weak symmetry breaking (EWSB) scale has been rewarding both experimentally and theoretically, with a steady influx of particle discoveries and an ever better understanding of interactions. This research program has culminated in the highly anticipated discovery of the Higgs boson in 2012 at the Large Hadron Collider (LHC).

Physicists have also been expecting to find evidence for physics beyond the Standard Model—but no conclusive evidence has been found to date. These expectations had been nourished by a variety of known shortcomings of the SM. An important—but by no means the only—argument was the so-called naturalness principle (Susskind 1979; Williams 2015; Fischer 2023): certain fine tunings could be taken as an indication that new physics should be within the reach of collider experiments. Unfortunately, these expectations have not been fulfilled. In the "post-naturalness era" (Giudice 2018) known shortcomings of the SM still motivate looking for BSM physics. But the big question is: can we hope to find new physics in the energy regime to be probed by colliders-to-be-built, or do we have to expect a large energy desert?[9]

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[8] Similar considerations apply to other large collaborative projects, such as projects in astrophysics. See, e.g., discussions about upgrading the Event Horizon Telescope (EHT) to the next generation ETH (ngEHT) (Johnson et al. 2023).

[9] As an illustration of this situation consider arguments regarding a potential end of the particle era (Harlander et al. 2023) and particle physicists' reactions to results that they hope 'disrupt' theoretical expectations, as discussed by Ritson (2020).

In what follows we will look at more concrete issues of experimental pursuitworthiness in this context. First (5.1), we will see that questions of experimental pursuitworthiness are sometimes addressed quite independently of research questions, when it comes to comparing the performance of experimental facilities. Second (5.2), we will discuss the Future Circular Collider as an example of an experimental pursuit that is currently being discussed. This example will highlight that it is not only individual research questions that matter for considerations of experimental pursuitworthiness: it is also the capacity of experimental facilities to combine work on those questions, and it is the track record of foregoing experiments.

5.1 Comparing future colliders

Pursuitworthiness assessments can be made quite independently of concrete research questions. This can be seen by looking at comparative studies of future colliders. As an exemplary study consider Roser et al.'s (2023) report of the Snowmass'21 Implementation Task Force.[10] The study provides a detailed comparison of 25 proposals for future colliders, including circular electron-positron colliders (e.g., FCC-ee, CEPC), linear electron-positron colliders (e.g., ILC, CLIC), energy recovery colliders (e.g., ERLC, ReLiC, CERC), muon colliders, and hadron and hadron-lepton colliders (e.g., FCC-hh, SPPC). More precisely it develops "metrics to compare projects' cost, schedule/timeline, technical risks (readiness), operating cost and environmental impact, and R&D status and plans" (ibid., 2). The individual items of comparison can be mapped onto the economic framework suggested above.

First, the study identifies a collider's luminosity and center of mass energy as important features that are required for achieving particle physics goals. The study provides a comparison of the expected performance of various proposed colliders regarding these quantities. While, arguably, increased luminosity and center of mass energy of themselves do not represent or guarantee relevant epistemic gain, the report promotes them to relevant proxies to make the potential epistemic gains of proposed colliders comparable. It should be noted, though, that while the report prioritizes luminosity and center of mass energy, there are other features that play a role in assessing potential epistemic benefits as well. For example, the report discusses the colliders' potential to act as Higgs factories, where the Higgs serves as a tool for new discoveries.

Second, the study includes detailed discussions about the likelihood of achieving these goals. This includes assessments of the technical readiness and risk of collider proposals and their complexity. For example, for electron-positron colliders the report discusses

[10] The "Snowmass" is the United States particle physics planning exercise and it is organized by the American Physical Society (for details see, e.g., Garisto (2022)). It should be emphasized that the cited report only is part of an overarching multi-pronged strategy that besides large experiments promotes a "healthy breadth and balance of physics topics, experiment sizes, and time scales" (Butler et al. 2023, xi).

five “critical enabling technologies” including superconducting radio frequency cavities (SRF cavities), cryomodules, positron source, nanometer spot size and stability at interaction point, and damping rings. To each of these the report assigns scores for (among other things) associated risk factors, technology validation, and performance achievability and subsumes the scores by calculating the average of squares. The average of squares gives an overall comparison of the technical readiness and risk associated with future colliders.

Third, there are detailed discussions of cost prediction. For this the report proposes a 30-parameter model including items from civil engineering, power infrastructure, vacuum systems, physics infrastructure, magnets, radio frequency, cryo, plasma design, and controls. This model is then employed to provide a cost range for each of the 25 proposed projects.

So, once certain overall quantities such as luminosity and center of mass have been agreed upon as relevant goals, comparisons between concrete experimental setups can be made at a considerable level of specificity and sophistication. Ultimately, the value of achieving certain luminosity and energy goals will of course be determined by the research questions that can be addressed with them. What matters for our purposes, however, is that concrete research questions are to a large degree backgrounded in this comparison. Thus, what matters for discussions of pursuitworthiness is not exhausted by a look at research questions. What matters is the pursuitworthiness of the experimental facilities and activities which require an assessment in their own light.

5.2 The Future Circular Collider

Let us now zoom in on discussions about one such proposed collider. As a follow-up collider to the LHC, the European particle physics community is currently studying the prospects of a “Future Circular Collider” (FCC). The FCC is designed to be built in a new tunnel with a circumference of about 91 km and has two planned phases, the FCC-ee and the FCC-hh. The FCC-ee is an electron-positron collider designed to provide collisions with high luminosity with collision energies between 90 and 365 GeV. The FCC-hh will reuse the FCC-ee infrastructure to a large degree, and is designed to collide protons with protons, but also offers the potential to collide ions with protons and ions with ions. The goal of the FCC-hh is to push the energy frontier up to 100 TeV. The project planning for the FCC spans over 70 years. It includes a preparation and construction phase for the FCC-ee of about 20 years followed by 15 years of operation, and then a 10-year period of construction, installation and commissioning of the FCC-hh, which will then be operated for 25 years.

The FCC is clearly motivated by research questions. In particular, the four main goals that the latest feasibility study report (Benedikt et al. 2025) mentions are (i) to “map the properties of the Higgs and EW [electro weak] gauge bosons”, (ii) to “sharpen our knowledge of already identified particle physics phenomena with a comprehensive and accurate campaign of precision electroweak, QCD, flavour, Higgs, and top measurements, sensitive to tiny deviations from the predicted Standard Model behaviour and probing energy scales far beyond the direct kinematic reach", (iii) to "improve by orders of magnitude the sensitivity to rare and elusive phenomena at low energies [...] in particular, the search for dark matter should seek to reveal, or conclusively exclude, dark sector candidates belonging to broad classes of models", and (iv) to "improve, by at least an order of magnitude, the direct discovery reach for new particles at the energy frontier" (ibid., 1). These physics opportunities can be identified as the epistemic gains that particle physicists hope to reap from the project.

Note, however, that the FCC program is a controversial program even within the particle physics community (Castelvecchi 2025). Some argue that a key problem of the project is that, unlike the LHC, there is no clear hypothesis to be tested. In a response to this objection Massimi has forcefully challenged the underlying view of science as hypothesis testing as "factually inaccurate." Instead, particle physicists perform "an open-ended explorative kind of research" that is aimed at an "exploration into the realm of physical possibility", in particular, by excluding such possibilities (Massimi 2019).

I agree with Massimi's assessment. The underlying claim here is that experiments can be pursuitworthy without being aimed at testing a specific hypothesis. This also corresponds to how the physics opportunities are characterized in the feasibility study report: the goals are to "map" properties, to "sharpen knowledge", "improve" sensitivity and discovery reach. What makes these projects of mapping, sharpening, and improving pursuitworthy?

Conditions of pursuitworthiness for hypothesis testing and exploratory experimentation differ in important ways. What matters for hypothesis testing is (1) a clear indication that the hypothesis is epistemically relevant and (2) that the test will warrant a clear verdict on the hypothesis at (3) reasonable costs. What matters for exploratory experimentation is (1) some initial motivation that the surveyed parameter space will yield relevant insights and (2) that the experimental facilities are sufficiently versatile to cover a wide range of parameters and can be adapted to novel insights that are gained during inquiry at (3) reasonable costs.

So, arguably, an important advantage of the FCC is the *combination* of goals by first building the FCC-ee and then repurposing extant facilities to build the FCC-hh as a follow-up.[11] Accordingly, the feasibility study does not detail the costs of addressing individual research questions but rather the costs of the facilities. Thus, my claim is that one should not assess an experiment's pursuitworthiness by looking at individual research

[11] It should be noted, though, that this would be an advantage only over other large experiments without the potential to combine research questions. Generally speaking, one should expect series of small experiments to be more versatile.

questions. An exclusive focus on individual research questions and associated physics opportunities would clearly mischaracterize the project's pursuitworthiness.

Note also that the relative importance of research questions and experimental facilities depends on the state of the field.[12] If there is a *crucial research question*, then experiments will be primarily judged regarding their abilities to address that question.[13] Consider, for example, the muon *g-2* experiment, a high-precision measurement of the positive muon anomalous magnetic moment performed at Fermilab (Aguillard et al. 2025). The pursuitworthiness of this experiment comes down to one specific research goal: determining the muon anomalous magnetic moment with highest precision. If there is no such crucial question, then the experiment's broadness and versatility will be more central to assessments of pursuitworthiness, as in the case of the FCC. Thus, broadness and versatility increase the pursuitworthiness of an experiment. However, depending on the state of the field, they may not be necessary conditions for pursuitworthiness. Moreover, broadness and versatility may stand in tense trade-off relations with other aspects of experimentation, e.g., the cost of the facility.

Next, consider the idea of facility-first pursuitworthiness. It has been argued that a clear driver for building further circular colliders like those envisioned by the FCC program is the success of past circular colliders (Myers 2021). In particular, the idea to first build an electron positron collider and then repurpose part of the facilities for a hadron collider is preceded by CERN's repurposing the LEP tunnel for the construction of the LHC.[14] There are two aspects of this. First, the FCC would constitute a continuation of foregoing collider projects. Insofar as these projects are seen as successful, they may be seen as inductive support to expectations that also a continuation may be successful. Second, and more concretely, past experiments are an important basis for the feasibility of follow-up experiments because of the experience that the community has gained. Whether an experiment is pursuitworthy depends on how skillfully it is pursued. The fact that the particle physics community has performed large experiments of this kind makes it more likely that relevant skills are present, including such skills as required for managing human and financial resources in an international research environment (Myers 2021).

Note that with these claims I do not argue that the FCC is a pursuitworthy experimental facility. The point is a more general one. A fair assessment of the project should not be

[12] Thanks to an anonymous reviewer for pointing this out.
[13] Whether there *is* such a crucial research question is, of course, again a matter related to considerations of pursuitworthiness.
[14] Similar points can be made about other large empirical research endeavors such as in current gravitational wave astronomy. Since the 1990s, plans have been underway to build a gravitational wave detector located in space. The pursuitworthiness of current plans for LISA (Laser Interferometer Space Antenna) has been considerably boosted by a successful observation (the detection of gravitational waves in 2015 by the LIGO and Virgo collaborations) and the success of LISA Pathfinder, a project pursued to validate the technology for LISA (Armano et al. 2016; ESA 2017).

focused only on the costs and benefits of addressing individual questions that the project seeks to answer. Whether my general point speaks in favor of the FCC or not depends on whether there are other experimental facilities that make better use of their resources to address combinations of research questions. In particular, the argument provided here may be turned against the FCC. The FCC involves project planning ranging over 70 years. By agreeing on performing the FCC program the particle physics community would bind enormous resources for decades to come. Considering that scientific findings and advancements will give rise to novel experimental questions that we do not know yet, such commitments should be considered carefully.

Finally, arguing that the FCC constitutes a continuation of a successful research tradition is, by itself, certainly not sufficient for its pursuitworthiness. In fact, one might argue that it is a change of methodology (e.g., towards a muon collider) that is required to generate and address novel research questions. Moreover, one might object that the FCC-program is not a continuation of the LEP-LHC program precisely because, unlike these other colliders, it is not built with a similarly clear expectation towards the detection of a novel signal.

5.3 Consequences for other cases

An exclusive focus on research questions mischaracterizes experimental pursuits. What we need is a look at concrete facilities and activities because (1) for each question there are typically many ways of addressing it, (2) there is often no one-to-one mapping between questions and facilities/activities, and (3) questions can come up during experimentation that is simply motivated by past successes. To support these claims, I have drawn on particle physics as an example. Do the points that I have made here also hold in other cases?

In particular, one might worry that some of the points discussed here depend on the size of the research project that I have focused on. The FCC is an instance of "Big Science" involving the efforts of many thousands of researchers, enormous financial resources and very long timelines. Therefore, it may not be surprising that the FCC as an experimental program addresses a whole set of research questions rather than an individual question.

I have two points in response. First, I think that the observations made here should apply at least to other such cases of "Big Science." This is an important result because it shows that the assessments of pursuitworthiness in Big Science should not be focused merely on the questions that such programs address. Second, even on smaller scales it is likely that considerations of experimental pursuitworthiness take the form described here. When setting up a lab, scientists have to decide what kinds of instruments to buy with the limited funds that are available. Clearly, while some research questions may require single-purpose instruments, it is generally a matter of good lab management to invest into instruments that can be repurposed to address a variety of experimental questions.

Moreover, one might think that the case of the FCC is special because the FCC gives rise to new experimental pursuits, but the empirical methodology of high-energy physics is largely continued. The focus lies on pushing the luminosity and energy frontier with larger facilities. The FCC program in this regard continues past developments at the CERN, with the Large Electron-Positron Collider's (LEP) tunnel being repurposed for the LHC. Additional questions of pursuitworthiness arise when they are concerned with experiments that also employ new methodologies (in particle physics, e.g., the design of a muon collider). New and not-yet-accepted experimental methods will have to compete with extant accepted methodologies. Under what circumstances is it rational to develop these methods further such that they may become serious competitors for the accepted methodologies? While a detailed and systematic discussion of such indicators will have to be subject of future work, I believe a fruitful starting point are "experimental virtues" such as the uniqueness, precision and simplicity of a signature, the independence of systematic uncertainties from model assumptions, and the broadness of sensitivity (Mättig and Stöltzner forthcoming).[15]

6 Discussion

So far, my goal has been to establish a "context of pursuit" for the case of experiment, in analogy to Laudan's context of pursuit for theories. Note that in a sense it is obvious that there is a context of pursuit for experiments: experimental facilities need to be designed, paid for, and built, lab personnel need to be trained, testable theories and hypotheses need to be developed, and consequences of experimental results for extant theories and hypotheses need to be examined. The question here is not whether scientists *perform* these activities but (1) whether and to what degree these activities can be governed by rational considerations of pursuitworthiness, and (2) whether and to what degree they are autonomous from ideas of theoretical pursuitworthiness. With the economic model I have suggested one way to spell out what it means to speak of pursuitworthiness in contexts with resource scarcity. I have also argued that what matters for the assessment of pursuits are not simply research questions whose value is ultimately determined by theoretical background assumptions. It is the experiment itself and associated facilities and activities that should be at the center of the pursuitworthiness evaluation.

[15] Mättig and Stöltzner broadly describe these virtues as playing a role "in planning the detector, devising a strategy of analysis, and for accepting a result." The current analysis differs in that it puts a focus on the context of pursuit, which is delineated from the acceptance of experimental results. Further analysis will have to show in what sense the mentioned virtues can play a role in that context of pursuit as ex-ante indicators of successful experimentation, e.g., (how) can we assess the precision of a signature in advance?

I can anticipate two worries regarding this line of reasoning. First, one may be worried about the applicability of the economic model of pursuitworthiness, especially its ambition to work as the basis for quantitative comparisons. The foregoing example, however, should show that this worry does not apply in general. On the level of comparing concrete experimental setups there *are* ways to quantify the output of experiments as well as associated uncertainties and costs even in a field of foundational research.

The second worry concerns the foreseeability of research results. One may think that the gap between *ex-ante* and *post-hoc* considerations is simply too big. If experiments regularly have unforeseen outcomes, what is the value of ex-ante pursuitworthiness considerations? There are three reasons to take this worry very seriously.

First, experimental discoveries are often surprising, such as in the case of the discovery of the Cosmic Microwave Background or the discovery of X-Rays. In particular, the history of particle physics includes many examples of surprise discoveries, such as the catalysis of nuclear reactions by muons and other results from bubble chamber experiments (see, e.g., Alvarez (1969) and Perovic (2011)).

Second, as pointed out above, experimentation often takes exploratory forms (Steinle 2016), especially in particle physics (Karaca 2013; 2017; Mättig 2022; Beauchemin and Staley 2024). One may be worried that assessments in the context of pursuit rely too heavily on theoretical expectations as to give sufficient space for exploratory experimentation, the results of which may be less foreseeable.

A related point concerns science funding. According to Haufe (2013) funding agencies favor hypothesis testing because the risk and the significance of the results of hypothesis-driven research can be assessed more easily. Introducing a context of pursuit (in the way I have suggested here) may bear the risk of *unduly* favoring hypothesis-driven research to the disadvantage of more open and exploratory forms of research.

These are relevant concerns for setting up a "logic of pursuit" (Achinstein 1993) for experiments. They do not, however, speak generally against establishing a context of pursuit or even the specific economic approach employed here. Even if an experiment's results are often a surprise, that does not mean that research endeavors should not be planned to a certain degree such that surprises may be facilitated. Also, in the case of exploratory modes of experimentation scientists typically do not 'just explore' in an arbitrary fashion. In most cases there will be assumptions about what parts of the parameter space should be prioritized when exploring—and such issues of prioritization clearly depend on issues of pursuitworthiness.

The underlying worry here may be that speaking of the pursuitworthiness of experiments gives theoretical expectations too much weight in the planning of experimentation and, thus, could implement a "theory first" (Galison 1988) view that threatens the idea that experimentation has "a life of its own."

I hope to have countered such worries with the foregoing discussion. Besides theoretical expectations expressed through research questions, one needs to take into consideration also aspects on the side of experimental facilities and activities. These aspects concern, for example, how questions can be usefully combined to create an attractive research program and whether one may expect the experiment to *work*, that is, to produce any results at all. These latter expectations are rarely generated on the basis of theory alone. What matters here, for example, are experiences from other experiments.

7 Conclusions

I have argued that the pursuitworthiness of experiments merits a philosophical reflection of its own: extant discussions of theoretical pursuitworthiness depend on experimental pursuitworthiness, and we cannot hope that extant discussions simply carry over into the case of experiment. More specifically, in the case of experiments there is competition between various ways of achieving novelty. This has consequences for the kinds of questions that arise in experimental pursuitworthiness: the focus lies on allocating scarce resources according to what novelty is most relevant. The economic model puts this in terms of expected epistemic gain that can be achieved by investing one's efforts and resources.

Moreover, I have argued that we should distinguish between theoretically motivated research questions, on the one hand, and experimental facilities and activities, on the other hand. We have seen that experimental questions play an important role in motivating experimental pursuits. However, an adequate picture of experimental pursuitworthiness cannot be achieved by looking at experimental questions alone. As the example of future particle colliders illustrates, scientists are often concerned with the pursuitworthiness of experimental facilities and activities. First, there are typically multiple ways of addressing an individual research question. Second, an experimental facility may be particularly pursuitworthy because it addresses more than a single research question. Third, experimental research may give rise to new experimental questions while being guided by past experimental successes. The pursuitworthiness of experiments, thus, often is related to the availability of pursuitworthy questions. Yet it is far from being dominated by theoretical considerations. Experimentation has a life of its own, also in assessments of its pursuitworthiness.

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