The Pursuitworthiness of Experiments

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Abstract: Scientists decide to perform an experiment based on the expectation 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. A key question that arises is: what exactly is being evaluated when we assess experimental pursuits? I argue that, beyond the 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) pursuitworthy experiments typically address a combination of research questions, and (3) experimental pursuitworthiness can be boosted by past experimental successes. My claims are supported by a look into ongoing debates about future particle colliders.

Keywords: Pursuitworthiness, experiments, particle physics

1 Introduction

Scientists decide to perform an experiment based on the expectation 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 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

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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 a puzzle how and why new theories emerge, 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 is being evaluated when we assess experimental pursuitworthiness? In what follows I will argue that we should distinguish between experimental questions, on the one hand, and experimental facilities and activities, on the other hand. Experimental questions arguably play an important role in motivating experimental pursuits. However, an adequate picture of experimental pursuitworthiness cannot be achieved by looking at experimental 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 typically addresses more than a single research question. Third, experimental research may be motivated by previous instances of successful experimentation and may thus give rise to new experimental questions in the first place.

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. In section 3 I will argue that this is an unfortunate lacuna. However, a central question is whether experimental pursuits can at all be evaluated from an *ex-ante* perspective. To address this question, we first need to spell out what such evaluations would ideally amount to. In section 4 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 5 I employ this approach to introduce and clarify a distinction between experimental 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 experimental questions. In section 6 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 7 I will discuss some potential objections against the applicability of the "context of pursuit" to experiments.

2 Context of acceptance and context of pursuit

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).

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 more pursuitworthy than T2 if the rate at which solutions are produced by T1 is higher than T2's rate of problem solving.

The project of identifying a context of pursuit has two mutually related goals. Laudan's primary goal is to address the "problem of innovation", first raised by Paul Feyerabend: "if one insists [...] that standards for accepting a theory should be pretty demanding epistemically, then how can it ever be rational for scientists to utilize *new* theories which (in the nature of the case) will be likely to be less-well tested and well-articulated than their older and better-established rivals?" (Laudan and Laudan 1989, 222f). According to Laudan, this is solved by acknowledging that the development of new theories is simply governed by a set of distinct criteria: those of appraising pursuits.

Second, criteria of pursuitworthiness can be seen as a guideline to answering the more specific question of what the most pursuitworthy projects among a set of new theories are. Here criteria for pursuitworthiness are consulted for actual guidance, in a context in which it is not clear which one of the new theories or research programs will eventually succeed.

Criteria of pursuitworthiness typically fall far short of providing such guidance. Consider Laudan's criterion of rate of progress. In general, it is plausible that a high rate of progress indicates that further efforts will be rewarded with quick results. Yet, it is unclear how it could be applied to provide guidance in concrete decisions between novel theories. Such decisions require more specific directives that tell us how the rate of progress is to be quantified and how the relevance of specific empirical and conceptual problems that a theory faces are to be weighed. As an *ex-ante* heuristic for selecting a promising project Laudan's criterion is hardly helpful.

Other accounts of pursuitworthiness face similar problems. For instance, DiMarco & Khalifa's (2022) "apokritic" account of pursuitworthiness characterizes a research question's pursuitworthiness in terms of obligations and prohibitions. These, in turn, refer to the bugs and features of a research question and the scientists' capabilities to address the question. While the account argues convincingly that bugs, features and capabilities play an important role in assessing pursuits, it does not provide details about how they are to be weighed. An account suited for deciding between novel theories, however, would have to explain such weighing (see Duerr and Fischer (2025) for further discussion). It will be useful to keep in mind that approaches to theoretical pursuitworthiness have had such limitations as we go on to discuss the pursuitworthiness of experiments.

3 The pursuitworthiness of experiments

Laudan mentions theories and research traditions as objects of appraisal. A theory, according to Laudan, is "a very specific set of related doctrines (commonly called "hypotheses" or "axioms" or "principles") which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena" (71). As examples of theories Laudan lists Maxwell's theory of electromagnetism, the Bohr-Kramers-Slater theory of atomic structure, and Einstein's theory of the photoelectric effect.

A research tradition, by contrast, is "a set of general assumptions about entities and processes in a domain of study, and about the appropriate methods to be used for investigating and constructing the theories in that domain" (81). A research tradition is a broader framework that (1) is exemplified and partially constituted by specific theories, that (2) exhibits certain metaphysical and methodological commitments, and (3) has an extended history in which it experiences substantial changes. Examples of research traditions are "Darwinism, quantum theory, [and] the electromagnetic theory of light" (78).

In Laudan's account the primary object of appraisal are research traditions, which explicitly include methodological commitments 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.[2]

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; 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.

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 2024c),

[2] 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.).

and cosmology (De Baerdemaeker and Boyd 2020; Wolf and Duerr 2023; 2024).[3] 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). In particular, 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. 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", can we hope to make systematic *ex-ante* assessments of pursuitworthiness at all?

4 The economic approach

In what follows, I will limit the discussion to one promising approach to pursuitworthiness and apply it 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.

It is prima facie plausible that 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)).

[3] A notable exception is Laymon & Franklin's (2022), which discusses a series of intriguing case studies of experimental pursuits but does not provide an overarching account of the pursuitworthiness of experiments.

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 (2024) for examples of such formalizations). Here we will limit the discussions to qualitative considerations (for a detailed discussion of such qualitative considerations of theory promise see, for example Duerr and Fischer (2025)).

From a normative perspective the economic perspective is *prima facie* plausible. 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. [4]

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.

5 Experiments and experimental questions

What exactly is being evaluated when we assess experimental pursuitworthiness? Since most of the pursuitworthiness discussions have focused on theories, a natural starting point would be to argue that what matters for an experiment's pursuitworthiness is the pursuitworthiness of the theory that is being tested. For example, experimental searches for Dark Matter are pursuitworthy because of the theoretical pursuitworthiness of Dark Matter. However, not all experimentation aims at the testing of theories (Hacking 1983; Franklin 1986). For example, experiments may be employed to provide evidence for a new phenomenon or to articulate a theory. Moreover, there can be important mismatches between experimental and theoretical epistemic gains. An experiment's potential gain can be larger than that associated with the theory because of new instrumentation and experimental technology being developed (such as new data processing routines that can be reused in other contexts). The experiment may also have a smaller epistemic gain when the theory under consideration is pursuitworthy for reasons not related to its

[4] 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.

empirical predictions (such as the mathematical methods devised in the context of developing the theory).

In what follows I will argue that a more fruitful approach is to distinguish between experimental questions, on the one hand, and experimental facilities and activities, on the other hand. Both questions and facilities/activities are associated with relevant aspects that are not covered if we leave either one of them out of the assessment of a project's pursuitworthiness.

Let us begin with experimental questions. DiMarco & Khalifa (2022) argue that when we evaluate scientific pursuits, we need to address the pursuitworthiness of research *questions* (see also Barseghyan 2022). It is natural to assume that this extends also to the realm of experimentation.

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

Experimental questions can also arise from more general guiding principles (Fischer 2024a). These can be employed to formulate an experimental question that creates a no-lose situation (Fischer 2024b). A no-lose situation is achieved when the confirmation and the rejection are both associated with large potential epistemic gains. Under such circumstances addressing the experimental question will produce high epistemic gain no matter what the actual outcome is (as long as the outcome decides the initial question). For example, before the discovery of Higgs boson at the Large Hadron Collider there was no overwhelming consensus that a Higgs boson would be found. There was, however, a strong consensus that even the exclusion of the Higgs boson would advance the field, with some arguing that it would have been the more interesting result.[5]

In what follows, we will see that experimental questions are often central to evaluating experimental pursuits. However, we will also see that an exclusive focus on experimental

[5] For example, Sean Carroll commented the discovery of the Higgs as follows: "It's a bittersweet victory when your theory turns out to be right, because it means, on the one hand, you're right, that's nice, but on the other hand, you haven't learned anything new that's surprising" (Heilprin 2013).

questions can be problematic, especially when it gives too strong a priority to theoretical expectations as is often the case in hypothesis testing.

Evaluations of pursuitworthiness may also address the experiment itself, that is, the concrete facilities and activities that are employed to address research questions. 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 research question there is typically a variety of ways of addressing it. If one agrees on the pursuitworthiness of an experimental 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 experimental questions are important to set the goal of an inquiry, there are additional questions of the pursuitworthiness of experimentation that need to take into account specific experimental facilities and activities.

Second, often there is no one-on-one mapping between research questions and experimental facilities. In fact, pursuitworthy experimental setups are typically the ones that can be reused for further experimental questions, in particular, for questions that come up only during inquiry. 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, sometimes research questions only come up during inquiry. That does not mean, though, that experimentation is without guidance as long as such questions have not been formulated. Instead, experimentation is often guided by experience from other successful instances of experimentation. The fact that an experimental routine has been successful in the past can inductively support expectations regarding future success.

The upshot here is that to assess experimental pursuitworthiness both dimensions need to be considered: experimental questions *and* experimental facilities/activities.

6 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.[6]

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. 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 theoretical arguments. Among them is the so-called naturalness principle (Susskind 1979; Williams 2015; Fischer 2023): currently the Higgs mass can only be explained if certain Standard Model parameters are finely tuned. Many physicists have argued that this calls for an explanation. More precisely, it has been taken as an indication that new physics should be within the reach of collider experiments. Unfortunately, these expectations have not been fulfilled such that there are strong doubts as to whether the naturalness principle and other arguments were legitimate arguments in the first place. In the “post-naturalness era” (Giudice 2018) 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?[7]

In what follows we will look at more concrete issues of experimental pursuitworthiness in this context. First (6.1), 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 experimental *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. Second (6.2), 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.

6.1 The Future Circular 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

[6] 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).

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

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 which 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. In particular, the idea that dark matter models will be either confirmed or excluded mimics earlier no-lose arguments discussed above, stipulating that also the exclusion of the models amounts to epistemic advancement.

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 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? Arguably, a key selling point of the FCC is the *combination* of these goals by first building the FCC-ee and then repurposing extant facilities to build the FCC-hh as a follow-up. 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 questions. An exclusive focus on individual research questions and associated physics opportunities would clearly mischaracterize the project's pursuitworthiness.

Another point concerns the track record of preceding experiments. 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.[8] 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 lend 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 aim to defend the claim that the FCC is a pursuitworthy experimental facility. The point is a more general one. A fair assessment of the project should not be 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. Moreover, arguing that the FCC constitutes a continuation of a successful research tradition assumes that it is similar to past colliders in relevant respects. An opponent of the FCC could argue that the absence of a clear question to be addressed is exactly what distinguishes the FCC from earlier endeavors.

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[8] 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).

6.2 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. 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 easily mapped onto the economic framework suggested above.

First, the study identifies a collider's luminosity and center of mass energy as the 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.

Second, there are 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 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.

6.3 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, it is a matter of good lab management to invest into instruments that can be repurposed to address a variety of experimental questions.

The case of particle physics is also special in that it involves a high degree of theorizing. Even if there are no concrete theories to be tested, key motivation for pursuing particle searches derives from the theories and models that have been developed. This is different in other disciplines, where considerations of experimental pursuitworthiness may even depend much more strongly on other instances of successful experimentation. For an example outside of foundational physics consider pilot studies. A pilot study is a small-scale study that is performed to prepare a larger scale “parent” study. The pilot study employs methods and procedures that are like those of the parent study. The goal is to test the methods and procedures and to foresee possible problems that may come up in the conduct of the parent study. Thus, the pilot study helps estimate the expected epistemic benefits to be reaped from the parent study as well as associated considerations of costs and benefits.

Finally, 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). It is to be expected that a close analogue of Laudan's problem of innovation may come up in such situations. As in the case of theories, new and not-yet-accepted experimental methods may 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? What are indices of such promise? A discussion of the pursuitworthiness of such methodologies goes beyond the scope of the paper, but promising indicators are criteria of "epistemic superiority" and "experimental virtues" such as discussed by Boyd & Matthiessen (2024), Boge (2025), and Mättig & Stöltzner (forthcoming).

7 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 whether and to what degree these activities can be governed by rational considerations of 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 experimental 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.

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 (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; Massimi 2019; 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" 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 experimental 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.

8 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, I have pointed out a key difference between theoretical and experimental pursuitworthiness: in the case of experiments there is competition between various ways of achieving novelty, whereas traditional discussions of theoretical pursuitworthiness focus on competition between upcoming and established theories. 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 experimental 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 typically 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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