No-Lose Theorems and the Pursuitworthiness of Experiments

Enno Fischer[*]

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Abstract

No-lose theorems state that—no matter what the result of an experiment will be—there will be a relevant epistemic gain if the experiment is performed. Here I provide an analysis of such theorems, looking at examples from particle physics. I argue that no-lose theorems indicate the pursuitworthiness of experiments by partially decoupling the expected epistemic gain of an experiment from the ex-ante probability that the primarily intended outcome is achieved. While an experiment's pursuitworthiness typically depends on the ex-ante probability that the intended outcome is realized, this is not the case if there is a no-lose theorem in place. I argue that this works only if (1) the theorem's win condition is attainable with reasonable effort, (2) the theorem's underlying assumptions are plausible, and (3) all potential experimental outcomes are epistemically relevant. I also explore the consequences of no-lose theorems for considerations of scientific pursuitworthiness. First, no-lose theorems can play an important role in assessing the risk associated with investing into a research project. Second, no-lose experiments can enhance scientists' agreement about the pursuitworthiness of experiments. My analysis also shows that no-lose theorems can face a number of limitations in these contexts.

Keywords: pursuitworthiness, experiment, particle physics, Higgs boson

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[*] Institute of Philosophy, Dresden University of Technology. `enno.fischer@tu-dresden.de`

1 Introduction

Experiments at the frontiers of fundamental physics often involve expensive research facilities, and they need to be planned over very long time periods.[1] For example, the Large Hadron Collider (LHC) at the CERN (European Organization for Nuclear Research) was approved with an initial budget of several billion dollars in the 1990s and it is expected to be operated until 2041. There are studies for a follow-up collider called the Future Circular Collider (FCC) with a project planning extending over 70 years (Abada et al., 2019; Benedikt et al., 2020). Decisions regarding the planning of such experiments have a major impact on the development of particle physics as a discipline.

In this context, physicists sometimes invoke no-lose theorems (NLTs) to justify their decision making. No-lose theorems indicate that—no matter what the result of an experiment will be—there will be a relevant and guaranteed epistemic gain if the experiment is performed. Examples from particle physics have concerned Higgs searches (e.g., Lee et al. (1977a,b); Chanowitz and Gaillard (1985); Chanowitz (1986)), searches for low-energy supersymmetry (e.g. Barbieri and Giudice (1988)) and, more recently, the physics of potential muon colliders (Capdevilla et al., 2022).

However, our understanding of the function of NLTs and criteria for assessing the strength of NLTs is limited. Specifically, the discovery of the Higgs boson indicates that the corresponding NLT was successful, while the success of similar argumentative strategies in the context of supersymmetry (SUSY) searches is at least questionable (Fischer, 2024). This raises the question what makes NLTs work and what are the general features of successful NLTs.

In this paper I provide an analysis of NLTs, looking at examples from contemporary particle physics. I argue that NLTs indicate the pursuitworthiness of experiments by partially decoupling the expected epistemic gain of an experiment from the ex-ante probability that the primarily intended outcome is achieved. This works only if (1) the theorem's win condition is attainable with reasonable effort, (2) the theorem's

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underlying assumptions are plausible, and (3) all the experiment's potential outcomes are epistemically relevant.

Moreover, I will highlight two consequences of my analysis. First, NLTs are important for the planning of experiments because they reduce the risk of investing research funds into experiments that do not deliver any significant results. Yet, whether experiments should necessarily be supported by NLTs is a question that cannot be answered on a general level.

Second, NLTs can play an important role by enhancing the agreement of scientists about the pursuitworthiness of a research endeavor. Ex-ante considerations of scientific pursuitworthiness are typically affected by uncertainty regarding the achievability of intended result. Moreover, scientists may have different views on how uncertain the intended result is. Usually, this means that they will also have different opinions on the overall pursuitworthiness of a project. This kind of disagreement may be dampened by NLTs. Provided that scientists agree on an NLT they can also agree on the promise associated with a research project even if they have widely different views on how likely it is that the primarily intended result of the experiment is achieved. The potential to promote agreement on the pursuitworthiness is important when the planning and conducting of experiments involves coordinating the research efforts of scientists with potentially diverging background beliefs. Thus, it is particularly important in the context of contemporary particle physics because experiments such as those performed at the LHC require the coordinated efforts of many researchers from various scientific subcommunities.

Here is an overview of the paper. In Section 2 we will have a closer look at a paradigmatic example of an NLT: the NLT associated with the discovery of a Higgs boson. In Section 3 I will introduce the concept of expected epistemic gain. This concept will be employed to specify the basic structure of NLTs in Section 4. Based on this analysis I will formulate three important success conditions for NLTs in Section 5. In Section 6 I will explore some of the consequences for using NLTs as arguments for pursuing particular experiments.

2 The Discovery of a Higgs Boson

The discovery of a Higgs boson in 2012 was a major advancement for particle physics and represents an important confirmation of the Standard Model (SM) of particle physics, our best fundamental theory of matter today. The discovery was highly anticipated by the particle physics community. The Higgs mechanism, responsible for the mass generation of W and Z bosons, is known since the 1960s (Englert and Brout, 1964; Higgs, 1964). Yet, the Higgs mechanism has not been without theoretical problems. Specifically, the associated mechanism of spontaneous symmetry breaking arguably had an ad-hoc character (Friederich et al., 2014), and the stipulation of the Higgs as a scalar boson had been assumed to give rise to a naturalness problem for the SM of particle physics since the late 1970s (e.g., Susskind (1979)). Moreover, the SM does not predict the mass of the Higgs boson. As empirical research on particle physicists' expectations regarding LHC experiments indicates, there was no overwhelming agreement that the Higgs would be discovered at LHC experiments (Mättig and Stöltzner, 2019)).

Yet, there were strong arguments for expecting *something* new to be observed at LHC experiments—even if a Higgs boson would not be observed. High-energetic proton-proton collisions lead to scattering of W-bosons with W-bosons. The cross section of some such scatterings, specifically the longitudinally polarized, diverges linearly with energy (Lee et al., 1977a,b). With increasing energy, the probability would increase until it would eventually exceed the value 1. Probabilities exceeding 1, however, would violate the principle of unitarity, a key tenet underlying quantum mechanics. When one includes additional contributions involving the Higgs boson, however, the linear divergency is cancelled, restoring unitarity. Lee et al. (1977b) use this argument to derive an upper bound for the Higgs mass of 1 TeV. If this upper bound is surpassed, they argue, one would find that "[e]ither a light Higgs boson will exist or weak interactions will approach the richness of low-energy strong interactions" (885).

What's important here for our argument is that, according to this line of reasoning, new insights are guaranteed if experiments reach the threshold of 1 TeV: either the effects of the Higgs boson materialize, or an apparent violation of unitarity would indicate something else entirely new. Physicist Sally Dawson (2022) notes that on the basis of this argument physicists in the 1980s were convinced that "[*t*]*he LHC and the SSC [Superconducting Super Collider] had to find something*: If the Higgs boson weren't found, the thinking went, then there would be supersymmetry, technicolor, or something new at the energy scales accessible to the LHC and SSC that would restore perturbative unitarity. It is impossible to over emphasize the impact of this reasoning as it triggered serious thinking about how the Higgs boson could be observed at a hadron collider."

While the idea of a guaranteed advancement of particle physics through such experiments goes back to the arguments by Lee et al., the concept of an NLT has been invoked only more recently (see Espinosa and Gunion (1999); Forshaw et al. (2008); Kanemura and Nagai (2022); Capdevilla et al. (2022) for examples that explicitly use the concept of NLT). Researchers have also referred to such arguments as a "no-lose corollary" (Chanowitz, 1986) or the "'no-lose' capability" of certain experiments to allow relevant observations (Chanowitz and Kilgore, 1994). In what follows, I will focus on no-lose theorems (a term that emphasizes the theoretical and formal character of such arguments), but the main lessons of the analysis I provide here should carry over to no-lose reasoning regarding experiments that is not provided in the strictly formalized way of a theorem.

The above-mentioned no-lose theorem associated with the Higgs boson is maybe the most important recent example of no-lose reasoning in particle physics. But there are other cases in which similar considerations have been put forward to motivate the search for new physics. Another notable example are the expectations that have been formulated with regard to the discovery of Beyond the Standard Model (BSM) physics at the LHC. In particular, there have been attempts to formulate no-lose arguments with regard to the discovery of supersymmetric particles in the TeV regime or below. These arguments are typically based on the naturalness principle: the SM Higgs as a scalar boson would require excessive fine-tuning unless 'new physics' is discovered in the TeV regime. Assuming a certain threshold of allowable fine-tuning one can thus derive upper bounds for the discovery of supersymmetric particles (e.g., Barbieri and Giudice (1988); Dine (2015)). While the NLT regarding the Higgs boson was a success, the situation is much less clear in the example of naturalness arguments for SUSY because to date conclusive evidence for such physics has not been provided and neither did the absence of such results have the kind of immediate effects on SUSY's perceived viability that were suggested by proponents of upper-bounds arguments. Instead, the underlying naturalness principle has been adjusted multiple times and interest in SUSY has seen a steady decline (see Fischer (2024) for a discussion of pursuitworthiness claims of SUSY).

3 Expected Epistemic Gain

NLTs are concerned with the pursuitworthiness of experiments. There is a rich philosophical literature on the pursuitworthiness of scientific theories or research programs (Laudan, 1978; Nickles, 1989; Whitt, 1992; Šešelja and Straßer, 2014), and especially there are discussions of examples from contemporary fundamental physics (Camilleri and Ritson, 2015; De Baerdemaeker and Boyd, 2020; Chall et al., 2021; Cabrera, 2021; Wolf and Duerr, 2023; Fischer, 2024). The pursuitworthiness of experiments, however, has received comparatively little attention (see Layman and Franklin (2022) and DiMarco and Khalifa (2022) for recent exceptions).

Among extant approaches to pursuitworthiness economic accounts are particularly suited for an analysis of NLTs (Peirce, 1976; McKaughan, 2008; Nyrup, 2015). The basic idea here is that expected epistemic gains of performing a research project are set off against the costs associated with the research effort. Epistemic gains of a research effort are achievements that improve the researchers' knowledge or their ability to improve knowledge. For example, epistemic gains may include the discovery of new phenomena, new theories and models, new mathematical methods and measurement technologies. Costs are the resources required for performing the research. This includes, for example, the intellectual resources required for developing new theories, the material resources required for building up research facilities, and managerial resources for coordinating large groups of scientists. In what follows, I will focus on the concept of expected epistemic gain to shed some light on the basic structure of NLTs.

The expected epistemic gain (EEG) of an experiment (E) with two potential outcomes

O_1 and O_2 can be expressed as follows[2]

$$EEG(E) = Pr(O_1) \cdot EG(O_1) + Pr(O_2) \cdot EG(O_2).$$

Here $Pr(O_1)$ denotes the probability that the experiment's primary outcome is achieved (conditioned on the experiment being performed) and $EG(O_1)$ the epistemic gain associated with that outcome. Analogously, $Pr(O_2)$ describes the probability of the alternative outcome (conditioned on the experiment being performed) and $EG(O_2)$ describes the epistemic gain associated with that alternative outcome. To keep things simple, we discuss scenarios with two potential outcomes. The considerations can be generalized to experiments with more than two possible outcomes by including additional terms in the formula for the expected epistemic gain. In our example $Pr(O_1)$ represents how likely experimenters take it to be that they will observe a Higgs boson if certain collision experiments are being performed, and $Pr(O_2)$ represents how likely they take it to be that the Higgs can be excluded through suchcollision experiments. Then $EG(O_1)$ and $EG(O_2)$ correspond to the epistemic gain associated with observing or excluding a Higgs boson.[3]

A few comments are in order. First, the formula might give the impression that precise expected epistemic gains can be calculated. However, I believe that such concrete calculations cannot be performed. Yet, the formula still facilitates certain comparisons: suppose two experiments have primary outcomes with the same associated probabilities and epistemic gains. Then the formula recommends pursuing the experiment whose secondary outcome has the larger associated probability or epistemic gain.

Second, the formula is concerned with the expected epistemic gain of specific experimental outcomes. Considerations of pursuitworthiness, however, should also be affected by the broader epistemic consequences of performing experiments such as

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[2] The formula closely follows Nyrup's (2015) approach but refers to experimental outcomes instead of acceptance and rejection of hypotheses. The reason is that the epistemic gain of an experimental outcome can go beyond the acceptance or rejection of a specific hypothesis. For example, an experiment may be designed to test multiple hypotheses or to probe a large parameter space for new phenomena.

[3] There is a separate question of what kinds of inferences (e.g., regarding the existence of a Higgs boson) are justified by a certain experimental outcome. This question will not be addressed here since we are only concerned with a model that connects scientists' subjective expectations with their (likewise) subjective considerations of pursuitworthiness.

newly developed methods and research questions. So, in what follows, we will keep in mind that the provided model reflects only a very specific kind of epistemic gain.

Third, $EEG(E)$ is concerned with ex-ante considerations. The question whether an experiment should be performed typically arises at a stage at which knowledge about potential outcomes of the experiment is limited. Thus, the probabilities should reflect the subjective epistemic state of the researchers or decision-makers reflecting on whether it is worth performing the experiment.[4] This will depend on theoretical assumptions and knowledge from previous experiments. Also, the assignment of an epistemic gain to an outcome may take into account only the subjective epistemic state of the decision-makers. Sometimes researchers revise their views on epistemic gain in the light of new evidence. For example, one might fail to confirm one's hypothesis but later find out that the null-result implies the more valuable advancement of the field, as illustrated by the Michelson-Morley null-result and its relation to the subsequent confirmation of Special Relativity.

An aspect that matters specifically for the following discussion is that researchers may have differing views regarding both the ex-ante probabilities and the associated epistemic gains. As pointed out above, before experiments at the LHC had been performed there was considerable disagreement about whether a Higgs boson would be found. Likewise, there were and still are different views regarding the promise associated with SUSY discoveries. Disagreement also arises about assessments of epistemic gain. Such assessments may depend on various contextual factors and individual scientists' preferences, especially if they are members of different communities within one research area. For example, while the discovery of the Higgs was a great success for experimentalists, theorists rather have hoped for a result that points more clearly towards potential theories of BSM physics. Likewise, it has been argued that current assessments of potential gains in supersymmetry research vary across the phenomenology-theory divide (Gautheron and Omodei, 2023). This means that also assessments of the corre-

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[4] In a Bayesian framework such subjective prior probabilities affect the degree of belief in a hypothesis and thus questions of whether a hypothesis should be *accepted* (see, e.g., Dawid (2017) for a Bayesian perspective of the discovery of the Higgs boson). Note, however, that here we are not concerned with accepting or rejecting specific hypotheses based on experimental evidence but with the question whether certain experiments should be performed in the first place.

sponding considerations of expected epistemic gain will be affected by such contextual and community-dependent factors.

Finally, it should be mentioned that there may be interactions between considerations of ex-ante probability and epistemic gain. Surprising experimental results may be considered to be particularly valuable because they have potentially large consequences for our theories and background beliefs. Likewise, experimentally excluding a result that was unexpected in the first place may constitute a relatively low epistemic gain.

4 The Basic Structure of NLTs

With the concept of expected epistemic gain in place, let's address the basic structure of NLTs. An NLT promises a high expected epistemic gain if the experiment is performed. More specifically, the situation described by an NLT is that where the expected epistemic gain is high because

(1) the epistemic gain of both the primary outcome ($EG(O_1)$) and the epistemic gain of the secondary outcome(s) ($EG(O_2)$) are large, and

(2) the sum of the probability of the primary and secondary outcomes approaches 1: $Pr(O_1) + Pr(O_2) \approx 1$.

A consequence of (1) and (2) is that the expected epistemic gain of the experiment is high—to a certain degree independently of the probabilities of the individual outcomes. So, for example, the expected epistemic gain of Higgs searches was high because, (1) even if the Higgs had not been observed, the possibility of excluding the Higgs still would have been an important epistemic gain, and (2) there were strong theoretical reasons to expect either a Higgs discovery or exclusion upon reaching certain energy levels. So, even if one was not convinced that a Higgs would be observed, the NLT recommended performing the experiment because there was a high overall expected epistemic gain.

It should be noted that, given the present analysis, NLTs may—in the ideal case—-support arguments for the pursuitworthiness of an experiment. It is a different question whether they are necessary for an experiment's pursuitworthiness. Generally, I do not think that this is the case. As noted above, experiments may be pursuitworthy because of other epistemic consequences apart from those associated with the specific experimental outcomes mentioned by an NLT.

5 Evaluating NLTs

Given this characterization of an NLT, there are a few general considerations that can be made regarding the quality of NLTs. These concern the attainability of the NLT's win condition, the plausibility of the NLT's underlying theoretical assumptions, and the epistemic relevance of all potential outcomes of the experiment. I will address these considerations in turn.

Attainability of win condition. NLTs are formulated in a conditional form. If the experiment is performed, then some kind of epistemic gain is guaranteed. Of course, not any attempt at performing an experiment will do. What is required for the promise to be fulfilled is the successful performance of the experiment. In what follows, such successful performance of the experiment is what we will refer to as the *win condition* of the NLT.

The win condition of an NLT needs to be formulated carefully. The formulation needs to be independent of the intended outcome of the experiment, or any of the alternative outcomes that count as epistemic gains. Otherwise, the requirement for an experiment to create a no-lose situation would be overly demanding and would potentially render the NLT trivial. The win condition of experiments at the LHC, for example, should not be the observation of a Higgs boson or of 'new physics'. The win condition may only refer to considerations on the side of experimental performance, such as certain center-of-mass energy boundaries and intensities of proton-proton collisions. It is the NLT's work to provide a theoretical link between such considerations and potential experimental results.

At the same time, the win condition may not be too permissive, otherwise there is a risk of performing the experiment without achieving the no-lose situation. This can be the case if the evidence turns out to be insufficient to decide whether the relevant hypotheses are to be accepted or rejected. It might also turn out that less than satisfying epistemic gains will be achieved.

There are also pragmatic considerations regarding the evaluation of NLTs. The win condition must be attainable with reasonable effort—or at least an effort that is justified in the light of the expected epistemic gain. In the Higgs example the importance of the argument is clearly related to the relevant experiments having been in reach of experiments at then upcoming colliders. This was also assumed in the case for the win condition of the SUSY argument. It was assumed that the win condition could be achieved with relatively small additional effort over and above the efforts invested for testing the Higgs hypothesis.

Such considerations of attainability are particularly important in contemporary particle physics. The SM of particle physics is assumed to be predictively adequate up to about 10^2 GeV and it is assumed to break down at the Planck scale at 10^{19} GeV where gravity becomes relevant. Between those energy scales there could in principle be a 'large desert'. Whether and at which energy scale there are new phenomena to be expected is an important question since moving only a few orders of magnitude up on the energy scale requires vast experimental resources.

Plausibility. An important prerequisite for a situation to count as a no-lose situation is that the summed probability of the primary and secondary goals being realized upon meeting the win condition must approach 1: $Pr(O_1) + Pr(O_2) \approx 1$. This is related to the NLT's stating a true conditional: if the experiments win condition is fulfilled, then the probability that either the primary or one of the secondary goals will be achieved approaches 1.[5]

NLTs are supposed to provide ex-ante reasons for performing an experiment. Whether an NLT will in fact turn out to be true, however, may be difficult to see in advance. In particular, if we take the experiment to increase our knowledge of the research object, we cannot assume that that knowledge is already available at a stage prior to the experiment

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[5] In general, the higher the summed probability the better is the NLT. Probability 1 would be too demanding in contexts of ex-ante pursuitworthiness considerations.

when the NLT is being evaluated.

Thus, there need to be independent ways to assess the plausibility of an NLT. Specifically, NLTs typically involve a range of theoretical assumptions about models, theories, and scientific principles. These need to be plausible. The NLT concerned with the Higgs boson, for instance, involves quite far-reaching theoretical assumptions on the level of the SM of particle physics but also on the level of overarching physical principles. Most importantly, the relevant constraint is imposed by the principle of unitarity.

A similar role is played by the naturalness principle in SUSY searches. If low-energy SUSY is not found below a certain energy cutoff this severely limits the theory's potential to solve the SMs naturalness issue. However, while unitarity is deeply engrained in quantum theories, the status of the naturalness principle is much less clear (Fischer, 2023). There are arguments in favor of naturalness, e.g., those pointing to other instances where naturalness is fulfilled and those highlighting its potential significance for the Effective Field Theory framework (Williams, 2015). But there is little agreement what kind of naturalness concept should be required to hold. Non-findings of BSM physics have led to repeated adjustments to the naturalness concept, specifically regarding the permissible amount of fine tuning (Grinbaum, 2012) and there is a growing skepticism with regard to the naturalness principle being a useful guiding principle at all (Rosaler and Harlander, 2019; Hossenfelder, 2021).

Epistemic Relevance. All potential consequences of meeting the win condition need to be epistemically relevant. This is an important prerequisite because to a certain degree any experimental situation could be framed as a win-win situation. Think of the case of a failed experiment. Suppose parts of the detector are not correctly connected and the resulting measurements conflict with the experimenters' expectations (e.g., the apparent discovery of superluminal neutrinos at the OPERA (Oscillation Project with Emulsion-tRacking Apparatus) experiment (Strassler, 2012)). The unexpected result may initiate a search for explanations of that result. The scientists will double-check the experimental apparatus and find out that the detector is not correctly connected. The discovery of the miswiring certainly constitutes an advancement of inquiry since the scientists found out something that they did not know before. So, even in such a case of failed experimentation one may be able to identify an epistemic gain.

But this kind of advancement is not what is intended by those who bring forward NLTs. Instead, the idea is that even if the experiment has not the intended outcome (such as a particle discovery) the alternative outcome still counts as a particularly relevant result. In the given example of failed experimentation, the result seems to have limited relevance because it is merely concerned with the experimental setup itself and not with the wider consequences of the experiment. Even if finding problems in one's experimental setup is often crucial for experimental success, finding such problems is usually not the primary goal of pursuing the experiment in the first place (except, e.g., in proof-of-concept experiments when they try to establish that certain experimental problems can be avoided in principle).

In the foregoing section I have emphasized that there are context sensitive and community dependent factors that may influence what epistemic gain EG a researcher ascribes to achieving a particular experimental outcome O_1 or O_2. Consequently, there may be significant disagreement among scientists about how valuable a particular NLT is. Those researchers who ascribe a high value to the experiment's outcomes will also ascribe a higher value to the NLT, while those who are skeptical about the epistemic gain associated with one or more of the potential experimental outcomes will also take the NLT to be less valuable.

To see the importance of considerations of epistemic gain, one can once more contrast the Higgs case with attempts to formulate no-lose arguments regarding SUSY. The Higgs case, supposedly, is such a prominent case of an NLT because it realizes the prerequisite that a set of mutually exclusive and jointly exhaustive potential outcomes (either discovery or non-discovery of a Higgs boson) have high epistemic relevance. By contrast, one might argue that the case of SUSY is less clear-cut. Research on low-energy supersymmetry appears to describe a case where the non-discovery still represents some relevant epistemic advancement because some SUSY models have been excluded experimentally. However, the epistemic gain in this regard seems to fall short of the overarching goal of either confirming or unequivocally rejecting natural low-energy SUSY as a research program, and this overarching goal is what seemed to be intended by at least some of those physicists who formulated no-lose arguments regarding SUSY (e.g., Barbieri and Giudice (1988)).

6 Consequences

6.1 NLTs and Risk Assessment

If NLTs succeed, they partially decouple the expected epistemic gain of an experiment from the ex-ante probability of the intended experimental result. This is important for the planning of experiments because it reduces the risk of investing research funds into experiments that do not deliver any significant results. Thus, NLTs have consequences for the pursuitworthiness of research endeavors.

However, there arises a question of *how important* NLTs should be taken for considerations of pursuitworthiness. Suppose a researcher may choose between two experiments E_1 and E_2 whose primarily intended outcomes are equally valuable and equally likely. Suppose also that the projected costs of both experiments are equal. Moreover, an NLT can be formulated for E_1 but not E_2. Given the economic approach to pursuitworthiness, E_1 would clearly have to be favored under such circumstances. Yet the situation is less clear if E_1 is associated with higher costs than E_2 or if the epistemic gains associated with the potential outcomes of experiment E_1 are significantly lower than the gain associated with the potential outcome of experiment E_2.

There arise interesting questions of how risk-loving or risk-aversive scientific decision-makers should be. Should they, for example, always opt for the highest potential epistemic gain (maximax)? Or should scientists prioritize experiments with NLTs, at the cost of building experiments that have a smaller potential highest epistemic gain (minimax)? Such questions will certainly have to be answered on a case-by-case analysis.

For example, there has been some discussion over the pursuitworthiness of a potential Future Circular Collider. In particular, the project has faced worries that huge investments are made with no clear results in sight. Reacting to such worries in a newspaper article, Michela Massimi (2019a) argues that such worries are related to an outdated Popperian view of scientific method as based on conjectures and refutations—the supposed problem being that there are no specific conjectures to be tested at the FCC. Against this she argues that high-energy physics is to be portrayed as an "open-ended explorative kind of research." Moreover, Massimi (2019b) argues that exploratory searches and associated modelling practices in high-energy physics are directed at delivering modal knowledge, that is, they aim to explore what is possible in nature. This, according to Massimi, implies that physics often progresses by means of excluding possibilities.

I agree with Massimi's view regarding the importance of exploratory modes of experimentation and the relevance of excluding possibilities. So, even without an anticipated discovery or a strong conjecture to be tested one may argue that the FCC is pursuitworthy. Yet, the reasons Massimi gives are weaker when addressing worries concerned with the absence of an NLT. Consider Massimi's emphasis on 'excluding possibilities.' This is convincing only if she is referring to epistemically relevant possibilities, such as theories and models that have at least some initial plausibility. This, however, is exactly what is at stake in no-lose reasoning: the idea of NLTs is that even if the primarily intended experimental outcome does not materialize, the experiment has significant epistemic gain that makes the overall experimental effort pursuitworthy. So, Massimi's arguments may work as a criticism of a view that sees clearly testable conjectures as a necessary criterion for the pursuitworthiness of experiments like the FCC. It appears to be weaker, however, against calls for the FCC to be supported by an appropriate NLT.

Another question is that of the relation between exploratory modes of experimentation and NLTs. On the one hand, an NLT for the FCC would certainly provide support for exploratory forms of experimentation such as discussed by Steinle (2016) and Karaca (2013). In exploratory experimentation one should focus on exploring or at least prioritize exploring those regions of parameter space and those theoretical possibilities that promise the largest epistemic gains. Appropriate NLTs would help identify the relevant regions in parameter space and the relevant theoretical possibilities. On the other hand, it should be emphasized that a general plea for the necessity of NLTs would be problematic. Making NLTs a necessary condition for the pursuitworthiness of any experiment may unduly favor experiments with strong theoretical background assumptions. In so far as exploratory experiments work with no or at least fewer such assumptions they may be unduly disfavored in scientific decision making.

6.2 Agreeing on the Pursuitworthiness of Experiments

It has been argued that scientific disagreement often concerns whether theories should be pursued or not rather than whether they should be accepted as being true (Lichtenstein, 2021; Cabrera, 2021). But scientific disagreement also concerns the pursuitworthiness of experiments. Such disagreement is particularly fierce when it concerns the pursuitworthiness of costly experiments as scientists compete for limited resources and the decision to support a large experiment may shape a research field for many generations to come. For example, when the construction of the Superconducting Super Collider (SSC) was cancelled in the 1990s it had also faced strong criticism from within physics, notably from solid state physicists who saw their own research underfunded (Martin, 2015).

A potential function of NLTs is that of enabling agreement among scientists about the pursuitworthiness of experiments. In the presented model, considerations of ex-ante pursuitworthiness are typically affected by the ex-ante probability of particular outcomes. I have also pointed out that scientists may come to quite different assessments of these probabilities. Usually, this means that they will also have different opinions on the overall pursuitworthiness.

This is different if there is an NLT in place that the scientists agree on. Then the scientists can also agree on the promise associated with a research project even if they have widely different views on how likely it is that the primarily intended result of the experiment is achieved. And this is the case because the NLT decouples the overall pursuitworthiness assessment from the ex-ante probabilities of the potential outcomes.

In the Higgs case the NLT could thus promote agreement on the pursuitworthiness of collision experiments at the LHC among those scientists firmly believing in the attainability of a Higgs boson and those scientists who were more careful in their expectations. Likewise, the naturalness argument proposed, e.g., by Barbieri and Giudice had the potential to promote agreement on the pursuitworthiness of the SUSY tests between scientists strongly hoping to confirm SUSY and those scientists who were more skeptical and waited for SUSY to be rejected.

The potential to advance agreement is an important consequence for experiments in general in so far as they require the joint efforts of scientists with potentially diverging background beliefs about the experiment's outcome. It is particularly important in the context of contemporary particle physics because experiments such as those performed at the LHC require the coordinated efforts of many researchers from various scientific subcommunities.

It should be noted, though, that an NLT's ability to dampen disagreements is limited to disagreements regarding the ex-ante probabilities. They do not extend to potential disagreements regarding the epistemic gain associated with the individual potential outcomes. If there is disagreement about how epistemically valuable the individual experimental outcomes are, then the NLT will do nothing to resolve that disagreement. Thus, the possibility of largely diverging views about the epistemic gains of an experiment somewhat restricts the potential of NLTs to create agreement among scientists. For example, the kinds of overarching disagreements about the value of foundational research at the smallest scales (Anderson, 1972) are unlikely to be affected by NLTs.

Another potential constraint are the above-mentioned interaction effects between ex-ante probability and epistemic gain. If divergent views on the ex-ante probabilities lead to divergent views about the epistemic gain associated with the corresponding experimental outcomes, and if NLTs do not alleviate disagreement over an outcome's associated epistemic gain, one might worry that the NLT is thereby problematically undermined. Yet considerations of epistemic gain depend on many other factors besides the associated ex-ante probability, for example, the fruitfulness of experimental results for further research. Thus, there may be significant agreement about epistemic gain even between scientists who ascribe different ex-ante probabilities to an experiments' potential outcomes. Therefore, the interaction effects between ex-ante probability and epistemic value will undermine the potential to create agreement only to a limited degree.

7 Conclusion

NLTs are sometimes invoked to motivate experiments at the frontiers of fundamental physics. In this paper I have characterized NLTs as partially decoupling an experiment's expected epistemic gain from the ex-ante probability of achieving the intended result. A closer look at examples from high-energy physics has shown that this works only if the NLT's win condition is formulated appropriately, the underlying theoretical assumptions are plausible, and all potential experimental outcomes are epistemically relevant. Under such conditions NLTs can play an important role for assessing the risk associated with funding large experiments and they can dampen the effects of potential disagreements regarding the pursuitworthiness of experiments.

The analysis has also highlighted important limitations of NLTs. NLTs may dampen only disagreement regarding the ex-ante probabilities of the experimental results. If there is disagreement about the epistemic gains of possible outcomes, then NLTs will not help scientists to arrive at an agreement. Moreover, NLTs are typically concerned with the expected epistemic gain associated with specific experimental outcomes. But pursuitworthiness assessments should also consider the broader epistemic consequences of experiments.

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