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ORIGINAL RESEARCH

The promise of supersymmetry

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Abstract
Supersymmetry (SUSY) has long been considered an exceptionally promising theory. A central role for the promise has been played by naturalness arguments. Yet, given the absence of experimental findings it is questionable whether the promise will ever be fulfilled. Here, I provide an analysis of the promises associated with SUSY, employing a concept of pursuitworthiness. A research program like SUSY is pursuitworthy if (1) it has the plausible potential to provide high epistemic gain and (2) that gain can be achieved with manageable research efforts. Naturalness arguments have been employed to support both conditions (1) and (2). First, SUSY has been motivated by way of analogy: the proposed symmetry between fermions and bosons is supposed to 'protect' the small Higgs mass from large quantum corrections just as the electron mass is protected through the chiral symmetry. Thus, SUSY held the promise of solving a major problem of the Standard Model of particle physics. Second, naturalness arguments have been employed to indicate that such gain is achievable at relatively low cost: SUSY discoveries seemed to be well in reach of upcoming high-energy experiments. While the first part of the naturalness argument may have the right form to facilitate considerations of pursuitworthiness, the second part of the argument has been problematically overstated.

Keywords Supersymmetry · Pursuitworthiness · Naturalness

1 Introduction

Many physicists expected that supersymmetry (SUSY) would solve important challenges faced by the Standard Model (SM) of particle physics, our best theory of the physics of matter. A central argument for the importance of SUSY is based on considerations of naturalness. In order to explain the mass of the Higgs boson, one apparently has to assume an exceptionally large degree of fine-tuning. Physicists have been aware

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of this problem since the early 1980s, but it has become particularly pressing since a Higgs boson at 125 GeV was experimentally confirmed in 2012. The naturalness problem is a strong motivation for SUSY because the fine-tuning could be prevented if SUSY was realized in the energy regime currently being probed by collider experiments.

However, none of the promises have been fulfilled so far. Experiments at the Large Hadron Collider (LHC) have reached and surpassed the energy levels where many physicists expected SUSY to be realized, but to date no supersymmetric particles could be confirmed. While SUSY remains an active field of research today, the non-discovery has significantly dampened particle physicists' enthusiasm.

In this paper, I analyse the promises that have been associated with supersymmetry. More specifically, I will address promises arising from naturalness arguments, employing a concept of pursuitworthiness. A research program like SUSY is pursuitworthy, if (1) it has the plausible potential to provide high epistemic gain and (2) that gain can be achieved with manageable research efforts. Naturalness arguments have been employed to support both conditions (1) and (2). First, SUSY has been motivated by way of analogy: the proposed symmetry between fermions and bosons is supposed to 'protect' the small Higgs mass from large quantum corrections just as the electron mass is protected through the chiral symmetry. This analogy has made plausible the promise of SUSY as potentially solving a major problem of the SM. Second, naturalness arguments have been employed to indicate that such gain is achievable at a manageable research effort: for a long time, SUSY discoveries seemed to be well in reach of upcoming high-energy experiments because naturalness arguments were taken to impose relatively low upper bounds on the mass of superparticles.

I will show that the two arguments have played different roles in motivating low-energy SUSY: the analogy motivated SUSY as an upcoming proposal for beyond the Standard Model (BSM) physics at a stage where it had not had the chance to prove its worth. The 'upper bounds' argument, by contrast, has been particularly relevant in defending the further pursuit of SUSY at a time when SUSY had already reached the status of a well-established research program that had not paid off in experiments despite high expectations.

While the first part of the naturalness argument has the right form to facilitate considerations of pursuitworthiness, there are problems with how the second part of the argument has been employed to motivate SUSY research. Problems arise because further pursuit is defended by stipulating a win-win situation. The promise is that a search for SUSY within the upper bounds derived from naturalness will advance the field either way: either we find superparticles (which would clearly be a major advancement for particle physics) or we do not find superparticles (which would also be a major advancement for particle physics because this means, it is argued, that low-energy SUSY as an important research program can be rejected).

This stipulation of a win-win situation is problematic. It trades on an ambiguity regarding the function of naturalness arguments. Naturalness is typically advertised as an argument concerned with the pursuitworthiness of SUSY. SUSY is a promising theory because—among other things—it has the potential to solve the SM naturalness problem. If SUSY turned out to be unnatural, then the expectations towards SUSY should be discounted accordingly. This would mean that SUSY would turn out to be less pursuitworthy than initially thought. The stipulation of a win-win situation, however, makes a stronger assumption. It presupposes that SUSY's violating naturalness would be reason enough to reject or "disprove" SUSY (Barbieri & Giudice, 1988). In hindsight, we see that this assumption is clearly too strong. Instead of a straightforward rejection of SUSY, the surpassing of expected upper-bounds has led to a series of adjustments to the naturalness principle.

While the main goal of this paper is an analysis of pursuitworthiness claims in the context of SUSY, the case of SUSY also highlights issues that are relevant for overarching epistemological discussions of promise and pursuit. First, pursuitworthiness arguments can be given at different stages of pursuit and may have different primary targets (expected epistemic gain or effort). Second, pursuitworthiness claims are typically contrasted with judgements of theory acceptance. But there are also important questions regarding delineating pursuitworthiness and rejection of scientific theories.

In Sect. 2, I will provide relevant context for the following discussions regarding SUSY. In Sect. 3, I will discuss in more detail the naturalness argument in support of SUSY. More specifically, I will point out two strands of the argument. The first strand is based on an analogy, and the second strand is concerned with establishing upper bounds. In Sect. 4, I will relate claims about the promise of SUSY to philosophical discussions of the pursuitworthiness of scientific theories. In Sect. 5, I will argue that both parts of the naturalness argument can be understood as indicating the pursuitworthiness of SUSY, but in slightly different ways. While the analogy argument is primarily employed as an initial motivation, the upper-bounds argument is primarily concerned with the further pursuit of an already established research program. In Sect. 6, I will discuss this second part of the naturalness argument critically and show in what sense and why it is problematic.

2 Problems and promises: from the SM to SUSY

The SM of particle physics is an exceptionally successful theory, but it is known to fail at arbitrarily high energy levels because it does not involve a description of gravity. As research in particle physics is going forward, a crucial question is: what lies between the scale of electro-weak symmetry breaking (EWSB, at about 10^2 GeV), where the SM is known to make accurate predictions, and the Planck scale (10^{19} GeV), where gravity is relevant, and the predictions are assumed to break down?

In principle there could be a large "desert" between these energy levels, meaning that no new physics is to be expected in the energy regime that we can hope to reach with our experimental facilities. However, there are a number of theoretical shortcomings of the SM that lead physicists to expect new physics not too far away from the energy regime that is currently being probed experimentally.

More specifically, there are three kinds of shortcomings of the SM that have been invoked for motivating SUSY.[1] First, the SM faces a problem of naturalness (also called the hierarchy problem or fine-tuning problem of the SM). For a long time physicists

[1] Work on SUSY has also been motivated independently of such shortcomings. See, e.g., Dardashti's (2021) discussion of early work on SUSY by Gol'fand and Likhtman (1971); Volkov and Akulov (1972); Wess and Zumino (1974).

have been aware that the scale of EWSB is located at around 10^2 GeV but that the quantum corrections to a scalar boson such as the Higgs boson are many orders of magnitude larger (Susskind, 1979; 't Hooft, 1980). This can only be the case if there is a delicate tuning of the parameters of the Standard Model. This naturalness problem has become even more pressing through the discovery of the Higgs boson at 125 GeV in 2012. What was a potential shortcoming of the SM up to that point had materialized through the discovery to an actual problem of the SM (Chall et al., 2021; Mättig & Stöltzner, 2019). SUSY holds the promise to prevent such fine-tuning if sufficiently light superparticles can be detected (Kaul & Majumdar, 1982; Veltman, 1981; Witten, 1981). In this paper, I will focus on this motivation for SUSY, but it will be helpful to point out two further motivations before we go ahead.

A second motivation for SUSY is gauge unification (e.g. Amaldi et al., 1991; Ellis et al., 1991). The SM provides a description of the electromagnetic interaction, the weak interaction, and the strong interaction. The strength of the interactions is quantified by gauge couplings, which depend on the energy scale. This dependence on the energy scale or 'running' of the gauge couplings is described by the renormalization group equations. In the SM the renormalization group equations predict that the gauge couplings will not meet at high energy levels. This is different in some supersymmetric models. According to the Minimally Supersymmetric Standard Model (MSSM), the gauge couplings meet at about 10^{16} GeV, meaning that the corresponding interactions can be understood as expressions of one underlying unified interaction.

Third, SUSY has been considered promising because of its potential to provide a dark matter candidate. According to the current concordance model of cosmology, the "Lambda Cold Dark Matter" model, dark matter accounts for 26% of the universe's energy density. But the exact nature of the hypothesized matter is unknown. One complication is that it is assumed that direct detections of dark matter are not possible because dark matter does not interact with the electromagnetic field. SUSY could involve Weakly Interacting Massive Particles (WIMP) that could be dark matter candidates (Ellis et al., 1984; Goldberg, 1983).

Finally, an advantage of SUSY research is that on the theoretical side it involved relatively modest efforts. Supersymmetric extensions of the SM can be treated in the effective field theory framework and can be analyzed using conventional Feynman diagram perturbative methods.[2]

So, while naturalness has been an important motivation for SUSY, it has certainly not been the only one. Moreover, the fact that SUSY promises to solve multiple major flaws of the SM can itself be taken as a kind of simplicity motivation for SUSY. Solving each of these problems with a different theory or model would certainly be far less elegant.

It should be noted that SUSY is not the only theory that has been motivated by naturalness considerations. First, technicolor models have been considered as a potential solution to the SM naturalness problem (Susskind, 1979; Weinberg, 1976). These models try to avoid an elementary scalar by dynamically generating W and Z masses. But technicolor models have faced difficulties well before the Higgs discovery and additional constraints arise because the Higgs is so light and has a width of less than

2 Thanks to an anonymous referee for raising this point.

a few GeV (Dine, 2015). Another important class of models that have been motivated by naturalness are models involving extra dimensions (Arkani-Hamed et al., 1998; Randall & Sundrum, 1999). Models with large extra dimensions aim to solve the naturalness problem by bringing the scale of gravity near to the scale of electroweak breaking. If two dimensions are added, large extra dimension models predict a modification of Newton's laws at millimeter scales, and the creation of new particles at the order of 1 TeV. But LHC experiments at 13 TeV impose severe constraints on such models (ATLAS collaboration, 2016).

3 Naturalness arguments for SUSY

3.1 Naturalness

The Higgs naturalness problem arises in the context of treating the SM of particle physics as an effective field theory (EFT).[3] According to this approach, the SM is a field theory that is predictively accurate below a high-energy cutoff (or equivalently above a certain length scale), and above that cutoff the SM is thought to break down. The squared physical Higgs mass $m^2_{H,p}$ can be written as follows:

$$m^2_{H,p} = m^2_{H,0} + \delta m^2.$$

The physical Higgs mass is the observable mass of the Higgs boson. The bare Higgs mass $m_{H,0}$ is the assumed mass of the Higgs at infinitely high energy or infinitely small distances. This assumed mass gets quantum corrections δm that arise from the Higgs interacting with the vacuum. To leading order the quantum corrections depend upon the top Yukawa coupling $y_{t,0}$, such that

$$m^2_{H,p} = m^2_{H,0} - \frac{y^2_{t,0}}{8\pi^2}\Lambda^2_{SM} + \cdots$$

The cutoff parameter can be pulled out of the sum, such that

$$m^2_{H,p} = \Lambda^2_{SM}\left(\tilde{m}^2_{H,0} - \frac{y^2_{t,0}}{8\pi^2}\right) + \cdots$$

[3] This original formulation goes back to Susskind (1979). Especially since the discovery of the Higgs boson and the subsequent non-finding of BSM physics, there has been an extensive theoretical debate about what the naturalness problem and the associated naturalness principle is (Bain, 2019; Borrelli & Castellani, 2019; Williams, 2015, 2019), and whether it is a legitimate problem or principle (Harlander & Rosaler, 2019; Hossenfelder, 2021; Rosaler, 2022; Rosaler & Harlander, 2019). In what follows, the details of these discussions will not be relevant. If the naturalness problem turned out to be a pseudo problem that would certainly undermine any pursuitworthiness claims based on SUSY being a potential solution to the apparent problem. But here I am interested in evaluations of the ex ante pursuitworthiness of SUSY, that is, evaluations of pursuit at a stage where it is not assumed to be known whether the naturalness problem is a genuine problem.

with $\tilde{m}^2_{H,0}$ denoting the dimensionless bare Higgs mass in units of Λ^2_{SM}. In 2012 experiments at the LHC have confirmed that the physical Higgs mass $m_{H,p}$ is at 125 GeV. This means that the quantity on the left-hand side of the equation is of the order of 10^4. Gravitational effects are assumed to become relevant only at the Planck scale at about $\Lambda = 10^{19}$ GeV, meaning that the quantity in front of the brackets is of the order of 10^{38}. Then the quantity in the brackets has to be of order 10^{-34}. But this means that the bare parameter $\tilde{m}^2_{H,0}$ and the contribution from quantum corrections have to be fine-tuned to an extremely high degree, which many physicists have taken to be implausible.

In what follows, I will explain how SUSY has been thought to solve the naturalness problem. The explanation has two parts. The first part is an analogy between corrections to the electron mass and corrections to the Higgs mass. The second part concerns the upper bounds on superparticles that can be derived from naturalness considerations. While both parts of this naturalness argument for SUSY are closely related to each other, I will argue later that they have played different roles in motivating SUSY as a promising theory of BSM physics.

3.2 The analogy

A standard diagnosis of the SM naturalness problem relates it to the quadratic divergence in Λ, which has been known to be associated with a fundamental scalar boson at least since Wilson who communicated such problems privately to Susskind (1979). A potential explanation of what is going wrong in the Higgs case can be given by a closer look at the analogous situation for fermions, where such a quadratic divergence does not occur.

Consider an electron in a vacuum according to classical electrodynamics.[4] This electron has a Coulomb field with the energy $\Delta E_{Coulomb} = \frac{1}{4\pi\epsilon_0}\frac{e^2}{r_e}$. This energy is called the electric self-energy or Coulomb energy of the electron and together with the bare electron mass $m_{e,0}$ it constitutes the observable or physical electron rest energy:

$$m_{e,p} = m_{e,0} + \Delta E_{Coulomb}$$

Since the electron radius is assumed to be smaller than 10^{-17} cm, the self-energy is very large (at around 10 GeV). In order to obtain the electron's observed rest energy of 0.511 MeV one would therefore have to assume that the bare mass delicately cancels the electron's self-energy:

$$0.511\,\text{MeV} = -9999.489\,\text{MeV} + 10000.000\,\text{MeV}.$$

This degree of fine-tuning is much lower than the degree of fine-tuning required in the Higgs case. Yet it still seems implausible. Why should the bare electron mass match the self-energy so closely? The fine-tuning can be prevented, however, if we restrict the theory to length scales above

[4] The following exposition follows closely that given in Murayama (2000) and Aitchison (2007).

$$r_{cutoff} = \frac{1}{4\pi\epsilon_0}\frac{e^2}{m_{e,p}} = 2.8 \times 10^{-13}\,\text{cm}.$$

The fine-tuning required above may be taken as an indication that below this cutoff length scale the theory no longer applies. And indeed, at small length-scales quantum electrodynamics (QED) predicts additional contributions to the self-energy. The classical self-interaction described above can be understood as arising from an electron emitting a virtual photon which is later absorbed by the electron. In QED there are additional vacuum fluctuations such that a pair of an electron and a positron is produced out of nothing together with a photon. The original electron in the vacuum may be annihilated together with the positron and the photon from vacuum fluctuations. Then the electron from the vacuum fluctuation will replace the original electron. The contribution of this process to the electron self-energy cancels the leading contribution of the Coulomb-energy, an observation already made by Weisskopf (1939). As a result, the electron bare mass gains only a logarithmic contribution from self-energy, rather than one that scales with $1/r_e$:

$$m_{e,p} = m_{e,0}\left(1 + \frac{3\alpha}{4\pi}\log\frac{1}{m_{e,0}r_e}\right).$$

So, the divergence related to going to very small distances (or high energy levels) in the classical case is reduced to a logarithmic dependence. This, in turn, means that we no longer need to assume a high degree of fine-tuning when applying QED to very small distances (or very high energy levels). Thus, the apparent naturalness problem of the electron mass is solved.

But why is it that the correction to the electron mass is proportional to $m_{e,0}$? This is because the Lagrangian of QED has an additional symmetry as the fermion masses go to zero: the chiral symmetry. This chiral symmetry implies that all radiative corrections to the bare electron mass vanish as the bare mass goes to zero. Therefore, the correction needs to be proportional to the bare mass. The small electron mass is in this sense 'protected' by the chiral symmetry.

Let's go back to the Higgs case. The Higgs mass receives a large contribution from the Yukawa interaction with the top quark, the main contributor to the Higgs quantum correction δm^2. This is why we needed to assume that the bare Higgs mass is extremely small, such that it cancels out the quantum corrections and we can account for the observable Higgs mass at 125 GeV.

But maybe the high degree of fine-tuning is, just as in the case of classical electrodynamics, simply a result of applying the SM to an energy regime where it no longer applies. So, as in the case of the electron we should expect that above a critical energy scale new physics comes up that cancels or at least reduces the overly large corrections.

What kind of 'new physics' could that be? In the QED case the large corrections where ultimately reduced because of the chiral symmetry which relates electrons to positrons. If there was an additional symmetry that protected the Higgs mass in an analogous fashion as in the QED case, then the large corrections could be avoided, and fine-tuning would not be required. This suggested symmetry is supersymmetry: a symmetry that stipulates fermionic superpartners for bosons and bosonic superpartners for fermions. With a contribution from the stop particle (the supersymmetric and scalar partner of the top quark) the leading contribution of the top interaction could be cancelled, and the dependence on the cutoff could be again logarithmic—provided that the yet unknown stop mass is not too high.

In summary, SUSY is motivated by an analogy to QED. QED solves an apparent fine-tuning problem of classical electrodynamics by introducing the positron as a new degree of freedom. This is related to the electron through a broken symmetry: the chiral symmetry that gets restored as fermionic masses go to zero. In analogy, SUSY could solve the fine-tuning problem of the SM by introducing superpartners as new degrees of freedom. These superpartners would again be related to known particles by a new symmetry that is broken at energy levels that have so far been probed: supersymmetry.

3.3 Upper bounds on SUSY

The analogy discussed in the foregoing section works only for a limited range of stop masses. The higher the stop mass the more fine-tuning is required, up to a point where SUSY will no longer prevent the naturalness problem. This idea has been employed to derive upper bounds on the masses of supersymmetric particles. The key idea is as follows: first, set a permissible threshold value of fine-tuning and then apply the fine-tuning measure to a specific model of SUSY. This should give an upper bound on the permissible mass of superpartners.

Barbieri and Giudice (1988), among the first to suggest such upper bounds (see also Ellis et al., 1986), argue that such upper bounds can be "an objective criterion to test (or disprove) the idea of low-energy supersymmetry, as implemented in supergravity models" (p. 63). More specifically, Barbieri and Giudice look at the dependence of some output parameter of the supersymmetric model at the electroweak scale M_Z on changes to input parameters a_i and the top Yukawa coupling y_t (which was not known by the time of Barbieri and Giudice's contribution). This dependence, according to their approach, should not be overly sensitive, that is, slight changes to the input parameters should not have dramatic effects on the value of the output parameter. The sensitivity is quantified by the derivative with respect to the respective input parameter and should not exceed a certain threshold value Δ:

$$\mathcal{N}_i = \left| \frac{a_i}{M_Z^2} \frac{\partial M_Z^2(a_i; y_t)}{\partial a_i} \right| < \Delta .$$

Barbieri and Giudice set the threshold of permissible fine-tuning to $\Delta = 10$. There is no specific justification for this value, other than the idea that cancellations among physical parameters should not exceed one order of magnitude. But Barbieri and Giudice also note that this choice of threshold value can be accounted for, considering that the upper bounds scale with $\sqrt{\Delta}$.

The result of Barbieri and Giudice's study are very specific recommendations for where potential new particles are most easily found. Weakly interacting superparticles should be found in the regime 100–200 GeV while strongly interacting superparticles may become heavier than 1 TeV. Given these arguments by Barbieri and Giudice, measurements at the Large Electron Positron Collider (LEP) and the Large Hadron Collider (LHC) should have discovered supersymmetric particles or at least they should have been sufficient to “disprove” low-energy SUSY.

But this is not how many physicists have perceived SUSY. Instead of a disproval of SUSY on the basis of upper bounds from naturalness, it seems that naturalness considerations had to be adjusted. By the late 1990s the permissible amount of fine-tuning had been moved up to a threshold value of $\Delta = 20$ (see e.g. Chan et al., 1998). Moreover, there has been a shift away from employing strict thresholds on the permissible amount of fine-tuning. Instead, physicists have employed fine-tuning measures to compare the naturalness of different BSM theories and models (Grinbaum, 2012) or to simply quantify the required fine-tuning (Wells, 2023).

Nevertheless, naturalness constraints have continued to be an important tool, even after the first run of the LHC. Feng’s (2013) discussion gives an idea of the more cautious use of such constraints. Feng emphasizes the “subjective” (p. 365) character of a number of choices that have to be made in order to evaluate the naturalness of SUSY. To begin with, Feng points out that naturalness can only be quantified with regard to specific supersymmetric models and choices of particular sets of fundamental parameters within such models. Moreover, there are some delicate choices to be made regarding the concrete naturalness measure. While Feng refers to the Barbieri and Giudice measure explained above as one standard way of quantifying naturalness, there are other possibilities. Even if the results of these measures only differ by a factor of up to four this can easily overturn the perception of a parameter being fine-tuned to a degree of $\mathcal{O}(1)\%$ to $\mathcal{O}(10)\%$, or so Feng argues. Finally, one has to determine the overall measure of naturalness from the sensitivities of the individual parameters a_i. Options include taking just the maximum $\mathcal{N}_i$, adding up the $\mathcal{N}_i$ in quadrature, or normalizing the individual $\mathcal{N}_i$ before taking them into account. While Feng still employs upper bounds on superpartners to evaluate SUSY proposals, he cautions against “grand conclusions based on hard cutoffs in naturalness measures” (p. 367).

In summary, naturalness does not only provide positive motivation for SUSY through an analogy. Naturalness can also be employed to derive upper bounds on the masses of superpartners. Beyond these upper bounds superpartners may still be realized. Yet they would come at the cost of new fine-tunings and thus the initial motivation of preventing such fine-tunings would be undermined. While the analogy is a qualitative argument in favor of naturalness, the upper-bounds claims are quantitative. These quantitative claims have to be taken with care, though, because a number of choices have to be made in applying fine-tuning measures to SUSY.

4 From promise to pursuit

The impact of naturalness considerations on the development of SUSY seems to be at odds with the relatively weak support that naturalness provides or should be taken to provide to SUSY. First, naturalness considerations are vague, as the discussion of the various choices in applying naturalness measures illustrates. Moreover, on the conceptual level there are various formulations of the naturalness principle such as those relating naturalness to the autonomy of scales and those relating the concept to a prohibition of fine-tuning. While there certainly are strong conceptual connections between these formulations, there are also significant differences (Borrelli & Castellani, 2019; Rosaler, 2022; Williams, 2019).

Second, considering the recent non-findings it is also unclear whether naturalness is actually realized. While the non-findings are a recent development and thus could not affect naturalness arguments at the time when they were initially invoked, it seems that even in the beginning, naturalness was seen at least by some (e.g. 't Hooft, 1980) as a speculative guiding principle (Fischer, 2023).

Finally, even if naturalness considerations had strong theoretical grounds, there would arise questions as to how far purely theoretical arguments could carry a theory such as SUSY. Dawid (2013) has argued that non-empirical theory assessment can play an important role in supporting theories of fundamental physics, such as in the case of String Theory. Similar thoughts may have an impact on the status of SUSY. For example, SUSY may be understood as receiving non-empirical support through unexpected explanatory coherence in virtue of providing a potential solution to the SM naturalness problem (Dawid, 2013, p. 87). However, whether the naturalness problem is actually solved by SUSY remains an empirical matter. As long as the stipulated superparticles have not been detected in collider experiments, the corresponding theory of SUSY remains just that: a stipulation.

A better understanding of the role of naturalness arguments in the context of motivating SUSY is available by a closer analysis of the promise that is implied by the arguments. This is a conditional promise: SUSY may turn out to be confirmed in the not so far future *if* further research effort is invested. And what's so special about the naturalness argument is that it also appears to allow a preliminary estimate of the effort that is needed, because we can calculate tentative upper bounds for the lightest superparticles.

The promise of SUSY, I will argue, indicated that SUSY was pursuitworthy. One of the first systematic discussions of pursuitworthiness is provided by Laudan (1977).[5] Laudan distinguishes two modalities of scientific appraisal. In the context of acceptance scientists are concerned with the question whether a scientific theory is to be treated as if it were true. Such treating of a theory is particularly important in contexts where consequences of the theory find application. The context of pursuit, by contrast, concerns whether a scientist or a scientific community should invest research effort into a scientific theory. Such appraisals of pursuit are crucial for scientific advancement. Already Feyerabend (1975) had argued that in the early stages of its development almost every theory is less acceptable than its rivals. So, without a distinct mode of appraisal regarding pursuit, it would be impossible to ever generate and develop new theories.

The promise of naturalness is concerned with the ex ante pursuitworthiness of SUSY. Evaluations of the ex ante pursuitworthiness concern a project before the project

[5] The concept of pursuitworthiness and associated concepts of fertility and fruitfulness have been of philosophical interest at least from the 1970s (Kuhn, 1977; Laudan, 1977; McMullin, 1976; Whitt, 1990, 1992), and they have recently attracted renewed interest (Nyrup, 2015, 2020; Šešelja & Straßer, 2014, and contributions in Shaw and Šešelja, 2021). Recent discussions of pursuitworthiness in fundamental physics have been particularly concerned with String Theory (Cabrera, 2021; Camilleri & Ritson, 2015).

is pursued, with a limited overview of the actual outcome of the research project. This should be distinguished from post hoc pursuitworthiness, which concerns judgments that are made in hindsight with an eye to the actual outcome of a research project. Ex ante and post hoc pursuitworthiness can come apart, when the research project does not achieve what was expected.

If we distinguish the context of pursuit as an additional mode of appraisal, there also need to be distinct standards for that mode. There has been an extensive debate about what such criteria for theory pursuit could be. Laudan (1977), for example, refers to the rate of progress of a research program: while a new theory may be less effective in solving extant scientific problems, the new theory's pursuitworthiness may be indicated by its solving new problems at a high rate. Other criteria include empirical fertility and conceptual viability (Whitt, 1992), potential explanatory power, potential inferential density, potential consistency, and programmatic character (Šešelja & Straßer, 2014). What the best set of criteria is, and whether at least a minimal set of criteria can be fixed at all (Shaw, 2022), however, remains a contested issue.

Whatever the concrete indicators of pursuitworthiness are, the overall structure of pursuitworthiness arguments involves two kinds of considerations that will be relevant in what follows. First, the expected epistemic gain and, second, the research effort needed to achieve that epistemic gain. Such an "economic" approach to considerations of pursuitworthiness can be traced back at least to Peirce (1976) who relates it to abductive reasoning. More specifically, Peirce argues that "the better abduction is the one which is likely to lead to the truth with the lesser expenditure of time, vitality, etc." (37f, see also the discussion by McKaughan (2008) and Nyrup (2015)).

In economic models of pursuitworthiness one would simply deduct the investment from the expected gain in order to see if a project pays off. In research contexts this does not seem to work so easily because comparing epistemic gain with invested effort is more difficult. But the overall reasoning still employs the same two main criteria: a project is pursuitworthy if (1) it has the plausible potential to provide high epistemic gain and (2) that gain can be achieved with manageable research efforts. An important consequence of this approach is that there are two ways in which one research project can be more pursuitworthy than another. First, one project is more pursuitworthy than another if a higher epistemic gain can be achieved with the same research efforts. Second, a research project can be more pursuitworthy if the same epistemic gain can be achieved with lower efforts.[6]

In what follows, we will apply this account to get a better understanding of naturalness arguments in support of SUSY, and we will see that naturalness arguments address both the expected epistemic gain and the required effort. For this, we need a clearer idea of what an expected epistemic gain is and what a manageable research effort is. These conditions, I assume, are highly dependent upon the specific research context,

[6] Some of the more concrete indices of pursuitworthiness can be related to this overall structure of pursuitworthiness arguments. Consider Laudan's (1977) rate-of-progress criterion. Assuming that the amount of time invested in a research program is a measure of the efforts invested, a high rate of progress means that many scientific problems are solved with relatively little effort. But Laudan's account works only as an account of ex ante pursuitworthiness if it takes the past rate of progress to be indicative of a future rate of progress. This does not have to be the case, as we will see in the discussion of SUSY below.

and it will be difficult to provide general criteria. But within the specific context of particle physics there are a few important constraints to be considered.

First, epistemic gains in particle physics can be many things: the development of a new theory or model, the development of new measurement technologies and algorithms for data analysis, for instance. But in the current situation of BSM physics, the discovery of a new phenomenon or particle that would show the limitations of the SM and give hints as to what theories beyond the SM should look like is the main goal.[7] In ex ante considerations of a theory's pursuitworthiness it is, of course, not known whether that theory will be realized. The *expected* epistemic gain accounts for this by factoring in the probability of achieving the gain. The more likely we think it is that supersymmetric particles can be detected, the higher the expected epistemic gain of SUSY. The estimate of that probability, in turn, depends on the initial plausibility of SUSY as a theoretical framework.[8]

Second, research efforts in particle physics can also take many forms: theoretical efforts in the development of theories and models, efforts in the development of mathematical and computational techniques used to extract predictions from models, efforts in building experimental facilities, efforts in generating and interpreting collision data, and organizational efforts in managing experiments that involve thousands of scientists. For our discussion an important kind of constraint is that of reaching certain energy levels at collision experiments. Due to energy conservation and energy-mass equivalence, the upper bound on the mass of particles that can be produced through collisions is limited by the energy of the colliding particles. In circular colliders this energy, in turn, is limited by the loss of synchroton radiation which increases with the fourth power of the beam's energy and is inversely proportional to the collider's radius. Thus, what is a manageable experimental effort in this context, very sensitively depends upon the order of magnitude of energy at which new phenomena are to be expected. The Large Electron-Positron Collider (LEP) with a circumference of 27 km, for example, reached a peak collision energy of 209 GeV. The LEP tunnel has been repurposed for the Large Hadron Collider (LHC), which reached 8 TeV in the first phase and 13 TeV in the second phase. A planned Future Circular Collider (FCC) at the CERN could achieve a collision energy of 100 TeV at a circumference of 100 km.

5 Naturalness and the pursuitworthiness of SUSY

Let's look more specifically at the two parts of the naturalness argument and see how they may indicate the pursuitworthiness of SUSY in the proposed framework. The first part of the argument, I have argued, takes the form of an analogy. This analogy indicates the pursuitworthiness of SUSY in two ways: first, by addressing the expected epistemic gain; and second, by addressing the effort.

[7] Considerations of pursuitworthiness can also be guided by considerations of non-epistemic gain, such as the technological advances brought about by collider and detector physics. Such arguments are often invoked to justify the high research expenses in particle physics. Yet these kinds of arguments are too unspecific to motivate any particular BSM theory over its rivals.

[8] In most cases, assigning concrete values to this probability will not be possible. But comparisons between proposals are still possible: all other things being equal the more probable epistemic gain should be targeted.

The primary role of the analogy is to indicate the initial plausibility of SUSY as a viable theory of BSM physics. While such initial plausibility arguments may not suffice to establish a new theory, they can be vital in motivating the investment of research efforts into the further development of a theory (Bartha, 2010; Hanson, 1958; Salmon, 1967). In the framework of pursuitworthiness proposed here such initial plausibility increases the expected epistemic gain because it makes it more likely that the epistemic gain of discovering supersymmetric particles is achieved.

More specifically, the initial plausibility is provided as follows. In the analogy's source system of classical electrodynamics, the high degree of fine tuning is eliminated by restricting electrodynamics to a certain energy regime. Beyond that energy regime new interactions occur between the electron and fluctuations of the vacuum described by QED. The new degrees of freedom accounting for these phenomena are related to the electron by a symmetry: the chiral symmetry. This symmetry explains why the initially divergent contributions to the electron self-energy are reduced such that no fine tuning is required in QED.

In the analogy's target system of the SM there is a high degree of fine-tuning required to account for the physical Higgs mass. This can be prevented if the SM is limited to a certain energy regime. Beyond that energy regime new physics becomes relevant. Prima facie, this could be any kind of physics not predicted by the SM. But since such physics was related to symmetry considerations in the QED case, one might think that a similar solution is realized in extensions to the SM. One symmetry that has been considered particularly relevant here is a symmetry between bosons and their fermionic superpartners and between fermions and their bosonic superpartners.

Providing initial plausibility to SUSY is certainly the main function of the naturalness analogy in the context discussed here. But analogies can play another role for pursuitworthiness claims when they can reduce the expected effort required for achieving an epistemic gain—at least on the side of theoretical progress. Nyrup (2020) argues that such reduction of effort is possible because "trying to adapt an already existing modelling strategy to a new domain is typically easier and less time consuming than developing a new one from scratch" (p. 897). Scientific understanding of a phenomenon (understanding-why) often requires understanding of a model or theory of that phenomenon (understanding-with, de Regt, 2017; Strevens, 2013). So, in order to understand a new (potential) phenomenon, scientists need a prior grasp of the new models and theories. This grasp is facilitated if the model or theory is similar to a model or theory that has been employed to understand known phenomena.

The analogy argument draws a parallel between SUSY as a novel phenomenon and well-understood phenomena of QED. If realized, supersymmetric models could thus benefit from the understanding-with that physicists have gained in the context of applying symmetry concepts in QED. This in turn, may indicate that the theoretical efforts needed for an understanding of SUSY are decreased through employing that analogy. Such understanding-with can also be achieved through other links to known physics. Yet the analogy to QED seems to be particularly important because it is employed in many pedagogical introductions to SUSY (see e.g. Aitchison, 2007; Murayama, 2000). It should be noted, though, that the efforts affected by the analogy are purely theoretical. Promoting the theoretical understanding of SUSY does not by itself contribute anything to decreasing the efforts needed on the side of experiment.

In summary, the analogy supports pursuitworthiness claims regarding SUSY in two ways. It indicates the initial plausibility of SUSY as a potential solution to the naturalness problem and thereby boosts the expected epistemic gain of pursuing SUSY. Second, it may reduce the theoretical effort because it indicates that 'symmetry protection' as an existing modelling strategy can be transferred to a new domain.

Unlike the analogy argument, the upper-bounds argument does not concern the expected epistemic gain of SUSY directly. Imposing upper bounds on superparticles, based on naturalness considerations, does not make it more plausible that such particles are realized. Neither does it increase the epistemic gain of confirming such particles. Instead, it is primarily concerned with the research effort required for achieving that epistemic gain. More specifically, the upper-bounds argument has been taken to indicate that empirical confirmation of supersymmetric particles is well in reach of current or upcoming collider experiments.[9] This has been taken to imply that the research efforts required for testing SUSY are manageable. Moreover, large parts of the research efforts associated with reaching experimentally that upper bound were motivated by another research goal: testing the Higgs sector of the SM at the LHC.[10]

This is an important factor in pursuitworthiness considerations. Earlier I have argued that a major constraint on experiments in high-energy physics is the collision energy. A concrete idea of the energy scale at which new phenomena are to be expected, thus, is vital in motivating the push to higher energy scales. Otherwise, one may invest huge efforts in increasing the energy limits without coming close to the scale where the expected epistemic gain may be realized.

In this regard the naturalness argument also differs from other motivations given for SUSY (Feng, 2013). The potential for gauge unification also depends on the mass of the superpartners because they have an impact on the renormalization group equations. But the dependence on the superpartner masses is only logarithmic, meaning that even large differences in the masses only have a slight impact. This is why even if gauge unification is an important motivation for SUSY, it is not considered a very useful tool for deriving upper bounds on superpartner masses. Likewise, considerations of SUSY as providing a dark matter candidate, are not a useful tool in this regard. The upper bounds imposed on superpartner masses that can be derived from this constraint are far beyond the reach of current colliders.

Judgements of pursuitworthiness can come into play at different stages of a research program. Most accounts of pursuitworthiness have focused on the early evaluation of research programs, at a stage where the research program still needs to get an opportunity to prove its worth. At such an early decision point one might not see very clearly the conditions and potential consequences of success. The focus lies on exploring the potential epistemic gain and the initial plausibility of it. Moreover, the efforts that need to be invested at such an early stage may be primarily theoretical. This is where the kind of initial support provided by analogical reasoning discussed above can be relevant.

[9] The situation changes, of course, when the upper bounds are surpassed without new particles being found. Then the upper-bounds argument should indicate a decrease in the probability of the epistemic gain being realized and, thus, a decrease in the pursuitworthiness.

[10] Thanks to an anonymous referee for raising this point.

Questions of pursuitworthiness, however, can also arise with respect to research programs that are quite developed (Chall, 2020; de Baerdemaeker & Boyd, 2020; Laymon & Franklin, 2022). Here, it may be well-established that the potential epistemic gain of a research program is very high, even if it is less clear whether the *expected* epistemic gain (factoring in the probability of the potential gain being realized) is high. Instead, the main question arising in this situation is whether further investment of research efforts will make a difference to whether the epistemic gain will actually be achieved or whether such investment just adds to the sunk costs. The upper-bounds argument has been taken to play an important role in motivating such further investment of research efforts into SUSY because it nourished the hope that soon there will be clarity regarding whether SUSY is realized or not. And this is a motivation for investing not just theoretical but also experimental research efforts.

6 Empty promises?

The upper-bounds argument is problematic if further pursuit is defended by stipulating a specific kind of win-win situation regarding research directed at low-energy SUSY.[11] Here, the promise is that a search for SUSY within the upper bounds derived from naturalness will advance the field either way, because once the upper bounds are reached experimentally, there remain only two options. The first option is that we will then have found superparticles. This would clearly be a major advancement for particle physics. The second option is that we won't find superparticles. But this would also count as a major advancement for particle physics because this means that low-energy SUSY as an important research program can be rejected.

This framing of the upper-bounds argument is evident, for example, from Barbieri and Giudice's (1988) seminal paper that introduced the naturalness measure discussed above. In the abstract of their paper, they advertise the upper bounds derived as giving "an objective criterion to test (or disprove) the idea of low-energy supersymmetry, as implemented in supergravity models" (p. 63). There are also more careful formulations in that publication. For example, Barbieri and Giudice argue that "[i]f no supersymmetric particle is found below the limits that we have given, the case for low energy supersymmetry gets, in our opinion, extremely weakened" (p. 73). But the overall gist stays the same: the upper-bounds argument is not just taken to provide a positive motivation for expecting SUSY in a certain energy regime. Beyond this, the argument is taken to undermine SUSY as a research project if no superparticles are found within the energy regime. There are further instances of this framing. Even Feng (2013), for example, who is much more careful in advocating naturalness measures as providing strict upper bounds, concludes that as of 2013, weak-scale SUSY is neither "unscathed, nor is it dead", but that in light of then upcoming upgrades to the LHC "patience is a virtue" and that "in the grand scheme of things, we will soon know" (p. 378).

[11] The discussion focusses on promises and potential disconfirmations of low-energy SUSY. The viability of low-energy SUSY has important implications for the pursuitworthiness of SUSY as a general property. But claims regarding a potential experimental rejection of SUSY in current and upcoming collider experiments only concern low-energy SUSY.

Note that, to a certain extent, any proposed experiment can be framed as a win-win situation. If a certain experiment is performed with the goal of discovering a new phenomenon, there is a sense in which any negative result can be described as an epistemic gain: the experiment has excluded possible conditions under which the new phenomenon could have been realized. In many cases, however, the epistemic gain of the negative result will be small compared to the invested effort. The claims associated with the upper-bounds argument for SUSY appear to invoke more than this minimal form of a win-win situation. The upper-bounds argument is not concerned with excluding individual proposals of low-energy SUSY but is framed as a threat to low-energy SUSY in general.

The stipulated win-win situation, however, trades on an ambiguity regarding the function of naturalness arguments. Earlier I have argued that we should distinguish between (1) accepting SUSY as true and (2) claims that SUSY is pursuitworthy. The main reason is that even if we are not (yet) convinced that SUSY is true, there may be good reasons to pursue SUSY. Moreover, I have argued that naturalness is an argument for (2), the pursuitworthiness of SUSY but not for (1), that SUSY should be accepted.

An analogous distinction has to be made between (1) rejecting SUSY and (2) claims to the effect that SUSY is no longer pursuitworthy. Even if we are not convinced that SUSY is false, there may still be good reasons against further pursuit of SUSY. Even if some form of SUSY is ultimately realized, it may be a waste of theoretical and experimental efforts to pursue SUSY under the current circumstances. SUSY may simply be out of reach for current and upcoming experiments, or there may be other BSM theories that are simply more pursuitworthy.

Now, naturalness is typically advertised as an argument concerned with the pursuitworthiness of SUSY. SUSY is a promising theory because—among other things—it has the potential to solve the SM naturalness problem. If SUSY turned out to be unnatural, then the expectations towards SUSY should be discounted accordingly. This would mean that SUSY would turn out to be less pursuitworthy than initially thought, because it simply does not solve one of the problems of the SM. Moreover, one may see the naturalness principle as actually counting as an argument against SUSY if SUSY, after all, requires the same kind of fine-tuning as the SM.

The stipulated win-win situation, however, makes a stronger assumption. It presupposes that SUSY's violating naturalness would be reason enough to reject or "disprove" low-energy SUSY. Naturalness has for a long time been considered among the strongest motivations for SUSY. But none of these arguments have ever been considered to provide sufficient basis for accepting SUSY as a true theory of BSM physics. This has a number of reasons. First and foremost, this is related to the primary role of empirical tests in the evaluation of theories. Moreover, even as a theoretical concept naturalness arguments have always had the role of a guideline or guiding principle rather than strict criterion. But given this comparatively weak role of naturalness in the context of theory *acceptance* it is unclear, why naturalness should be able to play such a powerful role in a potential context of theory *rejection*.

I should clarify, though, that this criticism does not concern the upper-bounds argument as such. The upper-bounds argument has been an important positive motivation for searching for new particles. In so far as it has predicted superparticles within the reach of current or near-future experiments it has increased the expected epistemic gain to be achieved with manageable research effort. Even though such particles have not turned out to be discovered, this was a legitimate preliminary reasoning in support of the pursuit of SUSY. My target of criticism here is more specific: I am critical of the promises derived from framing the situation as a specific kind of win-win situation. This is based on the additional (negative) assumption that upon reaching certain upper bounds derived from naturalness without finding supersymmetric particles we should have been in a position to reject a whole research programme.

A potential objection against my criticism is that this is cheap criticism: it is always easy (too easy!) to criticize pursuitworthiness arguments in hindsight. Doesn't such criticism as advanced here just conflate backward-looking (post hoc) pursuitworthiness with forward-looking (ex ante) pursuitworthiness? Even if we think now that the upper-bounds argument was problematic it may still have been rational ex ante. But again, the criticism does not target the upper-bounds argument as such but merely its framing as leading to a win-win situation. And that this framing problematically conflates naturalness as a ground for pursuitworthiness considerations and as a ground for potential rejections of SUSY could have been recognized ex ante.

Another potential objection is that low-energy SUSY is in fact rejected, now that certain energy levels have been reached, so the framing of the upper-bounds argument in terms of a win-win situation may not have been problematic after all. It is true that LHC searches have imposed considerable constraints on a large class of low-energy SUSY models. These constraints certainly are important epistemic gains, and one could make an argument to the effect that the associated research costs have been relatively modest.

However, the kind of win-win situation stipulated by the upper-bounds argument involves the stronger claim that low-energy SUSY could be disconfirmed upon reaching a certain upper bound derived from fine-tuning measures. This claim is clearly too strong.[12] SUSY remains an active field of research today, as evidenced by a quick search on inspire-HEP which registers about 50 papers per year with "SUSY" in the title in the early 2020s as opposed to an average of about 150 such papers at the peak around the early 2010s (see also the empirical data on model-groups in Chall et al. (2021)). Moreover, even though research interests in SUSY have been decreasing, the research dynamic does not display the kind of sudden decline that one would have expected if experiments had crossed a clearly disconfirming upper bound.

7 Conclusion

The promise that was associated with SUSY can be understood as a conditional promise: if further research efforts are invested, then SUSY will turn out to provide a large epistemic gain. I have argued that this promise can be spelled out in terms of pursuitworthiness: SUSY was taken to be pursuitworthy because (1) it was taken to have a high expected epistemic gain that (2) is associated with a manageable research effort. In this context, naturalness arguments have played a double role in motivating

[12] Buchmuller and de Jong conclude that the absence of new phenomena at the LHC "place significant constraints on SUSY parameter space," but that the constraints can "weaken considerably" depending on the assumptions made on the underlying SUSY spectrum (p. 23).

SUSY. The analogy to the protected electron mass primarily provides initial plausibility and, thus, increases the expected epistemic gain. The upper-bounds argument derived from naturalness, by contrast, is primarily concerned with the effort needed to achieve that epistemic gain.

But the upper-bounds argument has also been employed to frame the situation of SUSY research as a win-win situation. Once we reach the upper bound, it has been argued, we will either have found SUSY or will be in a position to reject SUSY. While the first (positive) part of this motivation seems prima facie legitimate, the second part is problematic because it trades on an ambiguity regarding the function of naturalness arguments. If SUSY turned out to be unnatural, then the expectations towards SUSY should be discounted accordingly. This would mean that SUSY would turn out to be less pursuitworthy than initially thought. The stipulation of a win-win situation, however, makes a stronger assumption. It presupposes that SUSY's violating naturalness would be reason enough to reject SUSY. But this is not the case. As a consequence, there remains the scenario in which research efforts are invested into experimental tests but no clear verdict regarding SUSY can be made. And, unfortunately, this appears to be the scenario that current particle physics is in.

My analysis indicates two lessons to be drawn for more general considerations of pursuitworthiness. First, pursuitworthiness arguments, can be given at different stages of pursuit and with different targets. Especially, there is a distinction to be made between the expected epistemic gain from pursuing a theory and the effort that is required to achieve that gain. Second, pursuitworthiness claims are typically contrasted with judgements of theory acceptance. But there are also important questions regarding delineating pursuitworthiness and rejection of scientific theories.

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