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Discovery Beyond the Standard Model of Elementary Particle Physics

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Chapter 1
Discovery Goals and Opportunities: A Defense of BSM-Oriented Exploration over Signalism

Abstract Discoveries come through exclusions, confirmations or revolutionary findings with respect to a theory canon populated by the Standard Model (SM) and beyond the SM (BSM) theories. Guaranteed discoveries are accomplished only through pursuit of BSM exclusion/confirmation, and thus require investment in the continual formation and analysis of a vibrant theory canon combined with investment in experiment with demonstrated capacity to make BSM exclusions or confirmations. Risks develop when steering away from BSM-oriented work toward its methodological rival, "signalism," which seeks to realize SM falsification or revolutionary discoveries outside the context of any BSM rationale. It is argued that such an approach leads to inscrutable exertions that reduce prospects for all discovery. The concepts are applied to the European Strategy Update, which seeks to identify future investments in forefront experiment that bring a balance of guaranteed and prospective value.

1.1 Introduction

The practice of science includes a wide range of activities, ranging from theoretical speculations to experimental analysis. These activities are all in the pursuit of scientific discovery—securely knowing something of science value that we did not know before. In this essay a formalization of the language of discovery is put forward that articulates common ambient notions in high-energy physics. From this, an argument is made that persistent and guaranteed discovery, as well as enhanced prospects for discovery of every kind, are accomplished through the co-work of beyond the Standard Model (BSM) theory and experimental work focused on BSM exclusion/confirmation. Signalism is the main methodological rival to BSM-centered exploration. It proposes to achieve SM falsification or revolutionary discoveries without any reference to BSM theories. However, it will be argued that signalism is an inscrutable and non-rational methodology for science discovery and puts at risk all types of discovery as conventionally conceived.

The thesis introduced above should not be interpreted to imply lower value or lesser status for other activities such as SM theory work, SM experimental analysis,

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formal theory, or detector/experimentation development. These, as we shall see, are indispensable activities ultimately in the service of discovery when done well. Nevertheless, it is argued that positioning BSM as the central attractor of theory work and experiment discovery is what guarantees, vindicates, and gives meaning to those other efforts.

This essay is admittedly long. The impatient reader can go straight to the summary (Sect. 1.10) to read a listing of the main points developed. The full essay aims to give context, justification and nuance to those claims. Sections 1.2–1.5 set up the conceptualization of discovery, with arguments and illustrations for the BSM-centered approach peppered throughout. Section 1.6 addresses the methodological rival "signalism" more directly, and suggests that it comes up short compared to BSM-centered work. In some sense, Sect. 1.6 is the culmination of the main thesis of the essay that BSM-centered work is superior to signalistic approaches for the pursuit of discovery. Sections 1.7 and 1.8 illustrate the main points of the essay through discussion of recent discoveries of gravity waves and the Higgs boson, and also through discussion of the European strategy update, which aims to make possible more discoveries in the future. Section 1.9 discusses the risks and signs of discovery ending, and their antidotes. Section 1.10 summarizes the essay.

1.1.1 Theoretical Versus Experimental Discovery

Let us continue the introduction by first discussing a little more on what is meant by "discovery" in this essay. Colloquially we refer to discoveries mainly within the experimental realm. There are exceptions, such as speaking of Einstein having discovered General Relativity, whereas Eddington only confirmed it experimentally, or rather discovered a unique predicted feature of the theory (bending of light). However, the majority of cases where the appellation discovery is applied is reserved to experimental work: Thomson discovered the electron; Rutherford discovered the proton; Chadwick discovered the neutron; Anderson and Neddermeyer discovered the muon; Richter's and Ting's collaborations discovered the J/ψ; the Gargamelle collaboration discovered neutral currents; the CDF and D0 collaborations of Fermilab discovered the top quark; Atlas and CMS collaborations of CERN discovered the Higgs boson; etc.

Standard usage of discovery in science rightly puts the primary emphasis on experiment. Applying the word "discovery" for the invention of a theory, whenever it does happen, as in the case of General Relativity, often only takes place after experimental confirmation, which is the strongest form of discovery. Discovery has the sense of uncovering something that is true that was lying in wait for us to find. For us, theories will not be evaluated in our forthcoming discussion on whether they always existed or whether they are permanently true fixtures waiting to be found, but rather whether they are presently adequate in the face of all experimental results known. Thus, it would be preferable perhaps to replace phrases such as "she discovered the theory of X” with something less provocative, such as “she educed the theory of X”.

Colloquially we may also utilize the word “discovery” for the product of a “founder of discursivity,” as a Foucauldian might say, where a new work produces “the possibilities and the rules for the formation of other texts” [63], where “other texts” in our context are forthcoming scientific works made possible by the founding work. A key example of this in recent years was the “discovery” of warped extra dimensions by Randall and Sundrum [92], which resulting in a multitude of additional works that built upon their founding idea. When Randall and Sundrum were appropriately awarded the 2019 Sakurai Prize[1] their citation was for “in particular the discovery that warped extra dimensions of space can solve the hierarchy puzzle...” [96]. The word “discovery” is implicitly modified by “theoretical” by the context of the award being exclusively in the theoretical domain. However, there has been no experimental verification (not yet at least) of warped extra dimensions. Therefore, by the common implicit rules of scientific discourse one could not say in a contextless environment that “warped extra dimensions have been discovered.” Only after experimental verification could one presume to make such a grand statement. For this reason, the unmodified word “discovery” in a contextless sentence must necessarily refer to a result confirmed by experiment, such as “the discovery of general relativity”, or “the discovery of neutrinos,” etc.

Nevertheless, theory plays a significant role in the discovery process. Many times experimental discoveries are made because they constructed dedicated apparatuses to search in subtle places that theory suggested. The most celebrated recent example of that is the discovery of the Higgs boson, which required a multi-billion dollar experiment with special particle detectors designed primarily with the Higgs boson discovery requirements in mind. Thus, any full accounting of discovery must also make theory an integral part of the story.

1.1.2 Experiment as Transformations of the Theory Canon

The key construct through which we account for theory’s role in discovery is what can be called the *theory canon*. The theory canon is the collection of all theories devised, including the standard reference theory (i.e., the Standard Model in particle physics), that satisfy all the requirements that physicists believe make these theories good descriptions of nature. There are many such requirements. Some are uncontroversial (i.e., must satisfy all known experimental data, must be mathematically consistent), while others are controversial (i.e., must be natural, must be simple, must not be in swampland). It is not just theorists who decide what belongs in the canon, but all stakeholders that test such theories. For this reason what is admitted into the theory canon is a difficult community discussion.

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[1]The Sakurai Prize is the highest award given by the American Physical Society for work in theoretical particle physics.

More will be said about the theory canon later, but let us suppose we have one. Experimental discoveries are then made within the context of that canon. Confirmations are made when a theory or a key component of a theory within the canon is confirmed. Exclusions are made with respect to a theory in the canon. (One cannot exclude what one does not know.) Similarly, relegation or falsification of a theory to the dustbin of history (i.e., total exclusion) is an experimental discovery that can only be achieved if there is a theory canon within which the falsified theory had once lived. The existence of the theory canon enriches experiment and makes possible numerous discoveries that were otherwise inconceivable.

Of course, there are experimental discoveries that take place completely outside the context of the theory canon. Finding completely unexpected particles or interactions or signals that are unanticipated by any theory within the theory canon is revolutionary. Such revolutionary discoveries (e.g., discovery of the muon is thought to be one such discovery) are part of physics history and presumably should continue to be into the future. Nevertheless, it should be noted that what makes them spectacular, eye-popping, revolutionary and rare is the existence of an advanced theory canon that is exploded by the discovery.

In the following, three broad categories of experimental discovery are described: confirmation, exclusion, and revolutionary. There are important further distinctions and subcategories that will be made within these broad categories, which includes SM confirmations, BSM confirmations, falsifications of the SM, falsification of a BSM theory, or falsification of the entire theory canon. As stated at the top, it will be argued that perpetual and guaranteed discovery passes through the focusing gateway of BSM theory and BSM-centered experiment.

1.1.3 The Work of Assured Discovery

It is hoped that articulating the concepts, categories and paths of discovery will contribute to assessing valuable activity in high-energy physics enterprise, especially as we plan for its future. As we contemplate all the aspects of guaranteed discovery, we see that the effort that gives rise to it can be organized into three core discovery activities that must be healthy for high-energy physics to be healthy:

- "model building": constructing a vibrant and motivated BSM theory canon.
- "theory analyzing": connecting theory canon ideas with phenomenological implications.
- "experimental work": translating phenomenological implications of the theory canon into experiments with assured confirmation/exclusion capability.

All three of these are necessary, and require intense, focused and unique knowledge and skill sets.

The categories above are based on action-oriented work, not static labels of individuals, since a scientist can in principle participate in any combination of these three activities, although he/she most often has hard-won primary expertise in only one. Often a physicist can have substantial overlap in nearest-neighbor activities. For example, a physicist can contribute to "model building" and "theory analyzing" and yet another can contribute to "theory analyzing" and "experimental work". It is hoped that it will become clear after the argument is presented that if any of these activities dwindles, guaranteed discovery ends.

The above list may give the reader the wrong impression that more formal theoretical work is viewed here as less relevant to discovery and less important. Formal work includes many areas of active research including string theory, AdS/CFT theory, amplitudes theory, finite temperature field theory, black hole conundrumology, information theory, etc. Although formal work looks far removed from discovery it is recognized by most to contribute as feed-in fuel to model building and theory analysis. For example, the proof of renormalizability of the weak interactions was critical to progress in concretizing the SM into a fully calculable theory (see Veltman's and 't Hooft's essays in [73]). As another example, the work of dualities in supersymmetric Yang-Mills theory [99] led to significant developments in BSM model building [47]. Likewise, AdS/CFT correspondence [80] has given much deeper and fruitful correspondences between theories of warped extra dimensions and walking technicolor [15], which on the surface looked unconnected. The recent development in the theory of amplitudes (for reviews, see, e.g., [43, 59]) is hoped to one day provide a significantly better approach to theory analysis, and perhaps even model building. Similarly, in the past, the mathematical physics work of group theory, topology, differential geometry, etc. also could not have been spoken of directly as "model building" or "theory analyzing" as discussed above, yet they ultimately have played central roles in both.

One could then interpret formal work as vital "pre-discovery" work in the service of model building and theory analysis which is, in turn, in the service of (experimental) discovery. It is no less an important activity as any of the others for a healthy and vibrant field that wishes to continue making discoveries far into the future. Nevertheless, if a particular activity of formal physics cannot be plausibly argued to have some possible connection to the three more direct discovery activities (model building, theory analyzing, experimental work), then it is at risk of being a less relevant activity. It is a subtle task to evaluate formal theory work's ultimate relevance to discovery. That topic will be taken up elsewhere. For the purposes of this essay we need merely acknowledge that the "pre-discovery" work of formal physics is crucial and contributes fuel to sustained progress in model building and theory analysis.

Lastly, just as formal work within theory gives fuel to future advances in model building and theory analyzing, so does "pre-discovery" work in experimental physics. Detector R&D, accelerator physics research, computational and electronics hardware advances, and analysis software tools, all contribute toward and seed progress in experimental work. In some sense this is the experimental analogue to theory's "formal work," which is less direct and proximate to actual discoveries, but is vital work that enables more direct discovery activities to realize themselves consistently into the future.

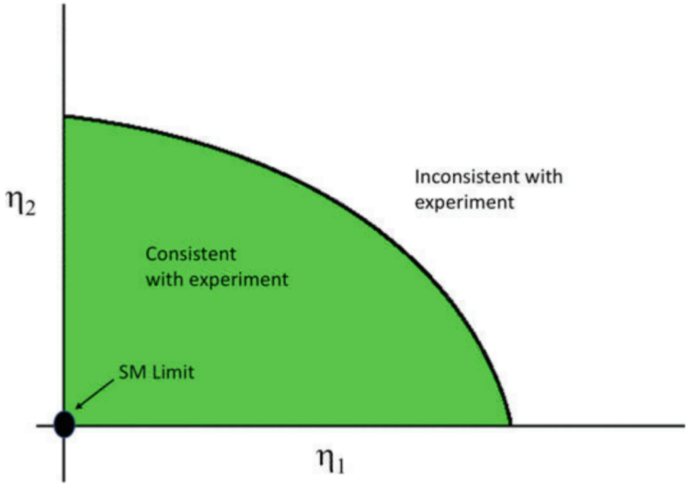

Fig. 1.1 Green is the currently allowed parameter space of a BSM theory that decouples to predictions for observables indistinguishable from those of the SM when $\eta_{1,2} \to 0$

Another example is "sequential hypercharge Z'" theory. This is a new Z' boson that couples to the SM in exactly the same way as the hypercharge gauge boson except that its mass $M_{Z'}$ and overall coupling strength $g_{Z'}$ are free parameters. This is arguably not within the theory canon since its motivation may not be high enough, but it demonstrates visualization in an especially simple way, which is analogous to many theories that are within the theory canon, such as dark photon dark matter theories. The ξ-variables for this representation are $g_{Z'}$ and $m_Z/M_{Z'}$ which gives decoupling (i.e., SM predictions) at the origin. It is also true that the entire $g_{Z'} = 0$ and $m_Z/M_{Z'} = 0$ axes are in the decoupling limit as well. It is this reason that experimental constraints on the parameter space will push closer and closer to the $g_{Z'} = 0$ and $m_Z/m_{Z'} = 0$ axes but can never get there. As the exclusion capability increases it nevertheless does open the opportunity for a signal to develop in that previously unexplored region of parameter space. That would constitute an important discovery.

As alluded to above, there may be other theories that have no decoupling limit at all to the SM. For example, the case of minimal no-scale supergravity theories with neutralino dark matter LSP do not allow superpartner masses to decouple [55]. This theory is similar to the standard minimal constrained supersymmetric standard model except that $m_0 = 0$ is required, which puts an upper bound on $m_{1/2}$, otherwise the LSP is no longer a neutralino and so cannot be the dark matter. The upper bound on $m_{1/2}$, and thus lower bound in $\xi_{1/2} = m_Z/m_{1/2}$, prevents reaching a decoupling limit within the theory.

One might object that minimal no-scale supergravity is just a subset of the parameter space within the more expansive minimal supergravity theories that do not require $m_0 = 0$ and thus should not be consider as an additional theory within the theory canon. However, landmarks of experimental progress are powerfully stated as total exclusion of coherent, self-contained BSM theories with specifically motivated theoretical structures and phenomenological targets (such as dark matter, $g-2$ explanation, etc.). Recognitions of BSM falsifications are powerful milestones, which can in turn also impact views of the community on the larger category of theories, such as whether minimal supersymmetry should still occupy high table in the theory canon.

We have discussed minimal supersymmetry and minimal Z' models within this discussion of the theory canon. But there are many more ideas of high interest to the high-energy physics community including warped extra dimensions, twin Higgs theories, little Higgs theories, minimal scalar-extended dark matter scenarios, superlight vector dark matter, low-scale baryogenesis sector theories, etc. The stature of various theories within the theory canon is not the subject of this essay, yet it must be recognized that various ideas are promoted and others relegated as their strengths and weakness are revealed in the intense theoretical and experimental scrutiny they experience. In this sense there is value in some "group-think" activity to promote, criticize and explore ideas. A thousands scientists in a thousand attics working on a thousand totally distinct ideas are unlikely to make the progress needed for discovery. Likewise, a thousand scientists in one attic working on only one idea is also unlikely to engender a healthy flow of ideas and discovery. As with most such endeavors, a balance between these two extremes toward constructing and analyzing the theory canon is likely to most useful. However, balance in this sense is not to be recommended to exist within every individual, but rather across the field, since individuals must focus to make impact. Partly for this reason, banishing an idea from the theory canon that has many invested proponents is not easy. Nevertheless, theory ideas die regularly, albeit it quietly with few visiting the graves (minimal technicolor, minimal non-supersymmetric $SU(5)$ GUTs, supersymmetric electroweak light-stop baryogenesis, minimal conformal SM, etc.).

Let us also remark that it is entirely reasonable to be cautious of theory talk about such lofty aims as "elucidating the true structure of space and time" and "constructing deeper reformulations of the laws of nature," etc. A new, improved language is not particularly transformative if one cannot order a good dinner with it, as every speaker of Esperanto can attest. Less controversial is a more instrumentalist appraisal of knowledge gain and theory development, which assesses the ability to predict that "if I do A, then I know B will come next", where, of course, B can be a collection of probabilistic outcomes. This power of prediction is worth more than any fancy subtle theory or "deep insight" into the soul of nature.[4] However—and this can never be forgotten—powerful workhorse predictive theories are often given birth by lofty theory/mathematical parents (e.g., non-abelian gauge symmetries, general coordinate invariance, supersymmetric theories, conformal theories, etc.). Thus, erring on the side of inclusive acceptance to theory development is in order, but researchers in theoretical high-energy physics should be able to articulate how their work is (or

[4] Distinguishing true science from mere visionary pronouncements has been a difficult problem for millinia. Nevertheless, as scholars frequently note, "we have come to realize that the best proof that our knowledge is genuine is that it enables us to do something" [61].

at least “might be”) connected to the construction of new BSM theories that answer outstanding problems in nature (i.e., ability to make predictions or to explain “histories”), or they should be able to explain how their work enables (or at least “might enable”) more effective analysis of the SM and BSM canon theories that enlarges capacity for exclusion/confirmation discoveries. Theory work that can do neither is unlikely to contribute to genuine discovery.

Finally, our purpose here is not to develop an evaluative theory of what should and should not be in the theory canon, or a praxis theory of how some theories get promoted and others banished among empirically adequate alternatives, or other such philosophical concerns. The purpose here is mainly to point out that a theory canon does indeed exist, as any high-energy physicist recognizes. They are the theories that many people continue to work on. They are the theories that experimentalists aim to find or constrain. They are the theories that end up in technical design reports motivating new experiments. Furthermore, the theory canon exists even though individual physicists might differ on what the community views as being contained within it, especially some theories on the “edges” of the canon (somewhat fewer practitioners, less experimental interest, remaining allowed parameter space is extremely “small” compared to prior motivated assessments, etc.). Criticisms, promotions, additions and deletions of the theory canon will always be a part of high-energy physics. Nevertheless, discovery is and should be made with respect to that canon, as will be developed more fully below. These discoveries are confirmation, exclusion and revolutionary, to which we now turn.

1.3 Confirmation Discoveries

With respect to the theory canon, there are three kinds of confirmation discoveries. The first kind of confirmation, SM feature confirmation, is experimentally verifying a feature of the SM that hitherto had not yet been established, or even was viewed by many as highly uncertain. The second type of confirmation, SM locus confirmation, is confirming by experiment the empirical adequacy of a narrowly carved locus of points in SM parameter space motivated by additional principles that go beyond the SM definition (i.e., BSM motivated). And a third type of confirmation discovery, BSM confirmation, is verifying a feature of a BSM theory by which the SM is eliminated from the theory canon and the BSM theory is elevated to the new SM.

1.3.1 SM Feature Confirmation

Let us first consider a SM confirmation. Throughout the history of particle physics there are many such examples. Notable ones in recent years include discoveries of the charm quark [18, 20], of the *W* boson in 1983 [16, 25], the top quark in 1995 [7, 8], and the Higgs boson in 2012 [3, 41]. The charm quark and Higgs boson