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Physics in Perspective

The End of the Theory-Driven Era: Five Decades of Particle Physics

Nurida Boddenberg,* Martin King** and Michael Stoeltzner***

For decades, research on the Standard Model dominated the field of elementary particle physics and searches for new physics beyond it were driven by the predictions of particular models, among them supersymmetry. These predictions have not borne fruit at the Large Hadron Collider, and as such physicists are increasingly turning to experiment for guidance. In this paper, we provide a philosophical analysis of the change, diagnosing it as a shift in consensus on where the field of particle physics expects the most progress and by defining general criteria whether a field is driven by theory or experiment. We base our analysis of the history of particle physics on programmatic documents issued by the large experiments, summary reports at the annual conferences assembling nearly all particle physicists, and on expert interviews and questionnaires conducted by us over the past decade.

Key words: Scientific progress; Expectations of progress; Elementary particle physics; Searches for new physics; Drivenness and pursuitworthiness; Philosophy of experiment; Empirically informed philosophy of science; Scientific consensus and central metaphor.

1 Introduction

There is a palpable shift underway in the field of high energy physics. For decades, the Standard Model (SM) has been the core of particle physics research: discovering the particles it predicted, measuring the values of their key parameters, and testing at what energy the theory might fail defined key parts of the field's major research programmes. The SM has passed all tests at unprecedented precision and become cemented as an accepted theory. However, the SM is incomplete, as there

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* Nurida Boddenberg is a philosopher of science at the University of Bonn. She was previously a doctoral researcher in the DFG project "Epistemology of the LHC," as part of the subgroup "Model Building and Dynamics."
** Martin King is a postdoctoral fellow at the Munich Center for Mathematical Philosophy at Ludwig Maximilian University of Munich and is a member of the Lichtenberg Group for the History and Philosophy of Physics at the University of Bonn. He is working on the DFG-funded Project "Deep Learning in Particle Physics: A Philosophical Analysis."
*** Michael Stoeltzner is professor of philosophy at the University of South Carolina. He has been a founding member of the interdisciplinary research unit "Epistemology of the Large Hadron Collider."

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are physical phenomena in nature that are not explained by the theory, e.g. gravity, dark matter, and the observed extent of matter–antimatter asymmetry. Physicists have formulated many new models and theories that go beyond the SM (BSM), such as supersymmetry and technicolor, that attempt to explain these phenomena. Despite the attractiveness of the theories and the expectations for physics beyond the SM, there have been no hints of new physics in the data generated at the Large Hadron Collider (LHC) or any other high-energy experiments since the discovery of the Higgs boson in 2012.

As a response, the field is shifting its approach. It has gone from being driven by testing theoretical predictions of available models (and combinations thereof) to expecting new directions from experiment. In our paper, we provide a philosophical analysis of this shift in what drives the field by looking at where physicists are expecting most progress. We establish this by a combination of empirical studies (three questionnaires sent out to the community of particle physicists in 2011, 2012, and 2018, each followed by expert interviews) and the summary reports presented at major conferences, through interviews and questionnaires about BSM model choice in physics conducted in 2018, as well as via official documentation that lay out where large research groups expect progress. We take this diverse evidence to demonstrate a consensus in the field about the expected progress at-large that is based on comparatively assessing a plurality of research programs, both experimental and theoretical, that are deemed promising on their own terms. We will also show that the fact that elementary particle physics is largely concentrated around a large experimental facility, the LHC, and the research center European Organization for Nuclear Research (CERN), provides several institutional structures for establishing such a consensus, among them the mentioned summary reports and the Moriond conferences.

The general shift to an experiment-driven era is related to a couple of other developments that have been discussed in the literature, such as the shift towards model-independent searches, which has been identified and partly characterized in recent studies.[1] We do not aim to describe the methods in any detail, but rather we are attempting to dive deeper into the dynamics of the change across the field and present the change in its historical context. For many decades, the field was being driven by theory—by the standard model research programme and by programmes to amend or test it. Now theory has become more focused on experimental applications and physicists are increasingly looking to experimental outcomes as a way forward.

In this paper, we aim to understand this development in terms of a shift in what is *driving* the field of research; a shift in being driven by theory to being driven by experiment. We will introduce the general notion of *drivenness*, elaborate what this entails, and identify two major modes of a field: *experiment-driven* and *theory-driven*. The drivenness of a field characterizes how the field is structured in terms of prioritized research programmes and we can identify it by where the most progress is expected to come. Although the *drivenness* of a field can be determined retrospectively it is a forward-looking notion, shaped by the circumstances and available resources at a time. This approach is distinct from the theory dynamics discussed by Lakatos[2] and Laudan,[3] who focused on confirmation and falsification as the motors of progress. Rather than rational reconstruction, we deal in expectations of progress that concern theory, experiment, or technology. To put it simply, when a field expects more progress from experimental research programmes it is what we call 'experiment driven.' As research unfolds, what drives the field of research may change. Scientific fields can shift, for example, from being driven by new experimental opportunities to being driven by work on a theory once that theory becomes more established. By looking at some examples in the history of particle physics, we will show that there are times when the character of the field is clearly recognizable and changes in the character can be seen. Our approach is also distinct from that of pursuitworthiness, which focuses on the qualities of particular hypotheses, models, or experimental research programmes, and provides a less overarching and more local concept. We are also not providing an overall assessment of the field—either in the form of an optimistic extrapolation to a final theory by Weinberg[4] or a pessimistic plea to abandon the post-war accelerator-based particle physics altogether by Hossenfelder.[5]

The paper is structured as follows: in section 2, we introduce our notion of consensus and outline our empirical methods that we draw on in diagnosing consensus on what drives the field at-large. In section 3, we will briefly describe the current state of elementary particle physics and the shift in modes of research that we aim to understand. In section 4, we draw on the philosophical literature to show that in a scientific discipline like particle physics, experimentation and instrumentation are not always subordinate to theory, but can actually be central research programmes in their own right. We also argue in favor of a broad conception of progress that recognizes distinct kinds of goals and allows us to speak about expectations of progress in section 5. With the discussion of expected progress in hand, we turn to the concept of drivenness itself in section 6. In sections 7 and 8, we present two case studies that exemplify the theory-driven and experiment-driven modes of research. Section 7 goes back to the time after the so-called "November revolution" of 1974 and the discovery of the J/Ψ. This crucial evidence for the SM kicked off a period of theory-driven research from which we only recently emerged.*[6] Section 8 takes up the discussion of section 3 and characterizes the present mode where the SM research programme has in many respects been completed and particle physicists are expecting less progress from theoretical research programmes and more from experimental research instead.

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* Although the SM research programme dominated for decades, a history of this entire period is far beyond the scope of this paper. We will restrict ourselves to the first decade, which is in any case sufficient to demonstrate that the field was in a theory-driven mode of research. For a more detailed historical view, see the endnote (ref. 6).

2 Methods and Consensus

Research in a scientific field is driven by where progress is expected. To reach such a diagnosis, there needs to be some form of consensus among the individuals and groups within the field. This raises three questions about the nature of the expected progress. First, what is the kind of consensus we are talking about, is it just what a majority of the group expects, or is it a group level property? Second, does it have to be a unanimous consensus and what about dissenting opinions? Third, how can one determine what the group expectations are? To answer these questions, we turn to recent work on the philosophy of consensus by Miller[7] and Dellsén.[8] Miller develops an account of knowledge-based consensus as distinct from mere accidental agreements. He introduces three conditions that concern any consensus whatever, and are not only about consensus in science where many institutions and standards are in place.

1. A *social calibration condition* that the parties involved have a shared formalism and ontological schemes, which is surely satisfied by members of the particle physics community.
2. A *social diversity condition* requiring that the group be diverse. The field is in some respects not representative of the broader population and much work remains to be done to make it genuinely socially diverse. In other respects, the field is quite socially diverse, having members from different countries and abiding by basic fairness and equity in hiring, allowing for research submissions from anyone, and so on. The point of this condition is to ensure that different points of view have been sufficiently considered such that the decisions can be representative.
3. And lastly, an *apparent consilience condition* that varied lines of evidence seem to support the same conclusion. This condition is the sticking point since many may doubt that there is even agreement among the members of the field and it is for this reason that we introduce the empirical studies in sections 3, 7, and 8.

With regard to the first condition, the group's expectations are not identical to the expectations of the majority of its members, nor are they a shared cognitive belief. They reflect more of a strategic agreement and are not something determined epistemically, rather a kind of agreement or general sentiment at a group level. Though individuals may have subjective probability assessments about where progress is likely, the expected progress of a group or field is not to be calculated on the basis of these. This kind of consensus is not established by a kind of direct democracy. There are many groups of different sizes—both within larger collaborations and across the field as a whole, with differing procedures for peer review and distinct research aims. This pluralism allows for multiple research programs to coexist, each with its own view on what constitutes expected progress. This leads us to the answer to the second question, which is that expected progress is not unanimous. There will be dissenting opinions about directions for the whole field and even about what the consensus is. J. S. Mill famously went as far as to claim that dissent is required for progress and many have agreed on this point. Recently, Dellsén (ref. 8) has pointed out that disagreement between experts may give the nonexpert the reason to think that one of the experts has an unjustified opinion. But he argues that disagreement should not be taken as an undermining of our trust in the consensus. In fact, it is the contrary: disagreement of this kind provides us with a positive (although pro tanto and defeasible) reason to believe that the consensus theory is indeed epistemically justified. The existence of dissent is complicated by the further fact that not only are there individual members, but within a field there are also subgroups that have different focuses. A field of research is multifaceted and involves many different experiments, institutes, and research clusters at different scales with different aims, each with competing demands for funding and independent expectations for progress. One subgroup may be heavily geared towards theoretical research even if many other subgroups are more experiment oriented, and vice versa, but as mentioned this does not preclude consensus. There are institutional avenues for discussion and the exchange of opinion from individuals and across groups which can lead to the establishment of real consensus. Groups are diverse and will involve dissenting opinions, but as Dellsén points out, this is necessary for real consensus. It is important to note that the disagreements remaining within a broad consensus are not necessarily considered as preliminary, to be resolved by a certain experiment or calculation. Some disagreements are simply about the more promising methods.

For the third question, expected progress is mostly an implicit agreement among the members of the group and among the subgroups, but it can also be explicit, in that there are often documents of future research plans drafted by large organizations representing the field. Expected progress is not fully determined by a vote or majority of individual expectations of progress, but there are additional concrete (empirical) ways of getting a grasp on what the expected progress is, which we have undertaken and present here in this paper. Below, we will establish that there is such a consensus and that there is a shift in this consensus via three reinforcing lines of evidence: a broadly distributed questionnaire, expert interviews, and statements from summary presentations at major conferences. At major conferences, such as the Rencontres de Moriond and the International Conference on High Energy Physics (ICHEP), large collaborations and experiments send speakers from various groups to give presentations. In addition to this, certain speakers are asked by the conference organizing committees to provide summaries of the theoretical talks or the experimental talks at the conference. Quite unlike the situation at major philosophy conferences, there are speakers that function as representatives and their summary reports reflect the current situation in the discipline. They can highlight the most important results, the biggest open problems, the most popular and promising avenues of current and future research from among the conference presentations. The proceedings of these summary talks can then be taken as representing a broad consensus among those in the field and provide authoritative overviews, including perspectives on recent results, open problems, and future aims. In this paper, we attempt to characterize the mode of research of the field as a whole and so rely on the consensus identified by such summary reports as well as on the trading zones* at such conferences that enable the establishment of this consensus. It is of course true that the representative nature of these documents is a consequence of the fact that today's elementary particle physics is concentrated around a few large experiments. In this way, the pluralistic consensus can be observed annually, rather than referring to the reports of overarching panels, such as the consensus study reports of the National Academies of Sciences, Engineering, and Medicine that often cover more than a decade.[9]

To further establish the state of the field, we also include quotes from targeted interviews with several senior theorists and experimentalists conducted at CERN and at Deutsches Elektronen-Synchrotron (DESY) in 2018, as well as the results of broadly-distributed questionnaires from 2011, 2012, and 2018 (see the Appendix for charts of some of the relevant responses). The questionnaire was distributed to those who have submitted research article manuscripts to the physics preprint archive, arxiv.org. It had on average more than 1300 respondents globally each year, among which roughly 54% were theorists and 40% experimentalists. This large sample allows us to get broad snapshots of the field's views on various questions before, during, after the major discovery of the Higgs boson at the LHC** and how the field is changing in response to the lack of new physics discoveries. Each of the questionnaires was followed by a series of expert interviews (we will use this material below in section 3). Our own historical examination is buttressed by the statistics of our questionnaire and by our interviews with experts that help paint an overall picture of the field and its expectations of progress. In sections 4, 5, and 6, we will explain the epistemic nature of these general statements about drivenness and consider how they relate to an idea that Peter Galison has introduced about a central metaphor, and the concept of pursuitworthiness that has previously been employed in more fine-grained accounts of the history of elementary particle physics.

3 Motivation: The Current Situation

A major turning point towards experiment drivenness can be seen in the crowning event of the SM research programme, the discovery of the SM Higgs boson. In 2011, 40 years after theories describing the most fundamental constituents and interactions came together as the SM, almost all ingredients of the SM had been found in agreement with its predictions. The Higgs mechanism, essential for explaining mass generation, was the only missing piece of the puzzle. The search for it, or more precisely, the search for the mechanism of electroweak symmetry breaking (EWSB)* dominated particle physics in the early 2010s.[10] There was a strong expectation that the LHC at CERN would find out how the electroweak symmetry is broken and confirm either the SM Higgs or some alternative model. After all, the previous experiments the Large Electron–Positron collider (LEP) and Tevatron had established certain lower bounds on the Higgs mass. However, our questionnaire reveals that there was not nearly as much consensus as is often supposed in retrospect (see figure 2 in the appendix). What we see is that the results are very evenly spread, with the same percentage (18.5%) strongly agreeing and strongly disagreeing that the SM Higgs would be discovered. In the second part of the question, 28% strongly agreed that the SM Higgs would be ruled out, indicating a more complicated Higgs sector with additional new physics. This also shows that questions of drivenness do not amount to a shared belief in a certain model or theoretical paradigm. Everybody agreed that the LHC would resolve the question posed by theory in one way or another. In parallel to dedicated searches for the Standard Model predictions, other BSM models were developed that proposed answers to the SM's remaining open questions—such as accounting for the matter–antimatter asymmetry, providing candidate particles for dark matter, or theorizing a possible quantization of gravity—as discussed by Ellis,[11,12] and Buchmüller.[13] These models introduce new particles like 'lepto-quarks' and 'stops' as part of larger theoretical frameworks, such as Grand Unified Theories (GUTs) and supersymmetry (SUSY). GUTs propose to unify the fundamental interactions by identifying the SM symmetries as part of a larger symmetry group. Supersymmetry (SUSY) is an umbrella term for a set of theories that feature an additional spacetime symmetry that entails a host of new 'superpartner' particles, one for every SM particle.** Supersymmetry has many virtues and was heavily pursued, partly because it was a more unifying theory that could solve many of the SM's perceived problems and some models, such as the Minimal Supersymmetric Standard Model (MSSM), featured new particles in the range of 1 TeV/c^2, which was feasible for testing at the LHC.

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* In the sense of (ref. 29), which we will discuss further in section 3.

** For a philosophical analysis of the discovery and confirmation of the Higgs boson, see the paper by Chall et al. cited in (ref. 1).

The Moriond conference in March 2012—the last major conference before the actual observation of a Higgs candidate—was dominated by reports on the LHC's ability to probe the SM and on early indications of a Higgs particle. A departure from the SM was only briefly discussed, but signatures of supersymmetric particles were still considered part of the LHC's potential.[14] The observation of a new particle with a mass of 125 GeV in the summer of 2012 threw particle physics into excitement, as can be seen from Michelangelo Mangano's summary[15] of the Moriond conference in 2013. The focus of the conference was on the nature of the new particle, as well as on potential deviations from SM predictions. While admitting that "there is clearly room for deviations to be detected with future studies and larger statistics," Mangano emphasized suggestive evidence for the 125 GeV particle to be 'a' Higgs particle. The following years were dominated by a consolidation of the 125 GeV signal being the anticipated SM Higgs as well as the ongoing search for new effects in line with BSM models as we see in the following.

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* This is the mechanism needed to give rise to massive W and Z bosons while maintaining a massless photon.

** As a matter of fact, SUSY models often have five or more Higgs bosons compared to the SM's one.

At the ICHEP in 2014, Andrij Pich summarized[16] that the searches for exotic objects and the efforts to solve open questions still gave negative results by "pushing the new physics scale beyond the reached experimental sensitivity."[17] Pich noted that this is "challenging our previous ideas about naturalness" and "putting in trouble the most fashionable theoretical scenarios for BSM physics."[18] While according to the summary report, some hope for new physics discoveries remained, as the LHC was not operating at full power yet and much data remained to be taken, there was already a growing uneasiness that the LHC would not be able to see the effects predicted by BSM models.[19]

Once no BSM signal was observed in the 13 TeV data, doubts became more urgent. While Ian Shipsey[20] admitted that "the road to SUSY is fogbound, but the fog might clear at any moment,"[21] he diagnosed the beginning of a change in the direction of particle physics: "since 2012, we are in a situation where we are trying to recognize a Dali masterpiece, with little information to guide us [….] Without a roadmap we are dependent on bottom up information: we are in a data driven era. […] In consequence, our job is to, more than ever: 'measure what is measurable and make measurable what is not so,' (Galileo)."[22] In response to the situation, one strategy is to broaden the scope of empirical studies to better include systematic observations from astro(particle)physics and cosmology. Further, we find an increased emphasis on *tool development*. Quoting from Freeman Dyson's novel *Imagined Worlds*, Shipsey pointed out that "new directions in science are launched by new tools much more often than by new concepts,"[23] emphasizing the necessity of new detectors and/or new accelerators.

In 2018, the year the SM turned fifty, Paul Langacker[24] appreciated the success of the SM so far, especially its mathematical consistency, but also pointed out at ICHEP 2018 that the aforementioned problems still persist. SUSY searches were still giving negative results: "since no signals have been observed, most recent analyses have concentrated on more challenging possibilities."[25] Further, Langacker addressed the question of how future progress will be made. He mentioned explorations at the newly achieved higher LHC energies and the expectations of a large increase in luminosity within the next 20 years, allowing for more precision measurements or new technologies being pursued in terms of detectors, accelerators, and machine learning algorithms. He went on to say that numerous remaining questions in neutrino physics should be addressed with experiments, while he expected observations to tackle observational shortcomings in astrophysics and cosmology. Finally, he expressed his hope to establish a revised theory, a “New Standard Model of Nature,” but only through significant progress in experiment and observation.[26]

More recently, discussions have revolved around the planned Future Circular Collider (FCC)* at CERN, which would dramatically increase our sensitivity to BSM physics. However, “given the lack of BSM discoveries so far, it is critical that the model-focused search program of future colliders be complemented by a model-agnostic effort. Machine learning tools are well-suited for this task, as they can efficiently explore high-dimensional feature spaces.”[27]

We also examined this shift from theory- to experiment-oriented approaches for future physics through expert interviews with physicists conducted in 2018. When asked whether the field had become more experiment- or theory-driven, the physicists responded in similar terms: that experiment is leading the way, driving the field. The interviewees were not told in advance what was meant by ‘experiment-driven’ and ‘theory-driven,’ but immediately understood the question. This demonstrates that this change is already evident to those in the field and that an elaborated concept of drivenness could be a fruitful way to understand this change. What it means more precisely for a field to be experiment- or theory-driven, and what drivenness is in general will be developed throughout the paper.

The following quotes from both experimentalists and theorists are typical of the sentiment shared by the physicists interviewed.

> “It is certainly experiment driven. Experiment is back to playing a key role of guiding theoretical thinking. By having killed, or any way, reduced, the range of interesting theories, it’s doing precisely that. It’s leading the way.”
>
> “Just before the Higgs discovery, I was just doing model building for model building’s sake. And I don’t do that at all any more. I’m much more connected to experiment, and I think that’s true for most people.... To me, the slogan would be ‘Experiment, experiment, experiment.’ I think that’s the only way we’re going to make progress.”
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> “We’ve been in this sort of phase where the standard model has pretty much been there for some tens of years...Theorists have had a long time to think about the next stage, based on dark matter, and fine tuning, and whatever the different arguments are. So now we need a breakthrough in experiment to give us a possible direction, I would say.”
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> “It’s not the theory that drives the data, the data drives the theory. And it is an experimental thing particularly since we don’t know where we are going from here...”

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* FCC is going to be an electron positron collider with an $\approx$ 90 km long tunnel, which will allow searches for new particles. The FCC is considered in two stages (e^+e^- collisions and pp-collisions).

> "Experimental data now force us to think, or rethink, about the foundation of the theory... It is the experimental data that really tell us what are the theoretical questions that you should ask yourself."

In summary, we observe the following changes during the last decade: from the original excitement with the start of the LHC operation, through the momentum gained by the Higgs discovery and the further establishment of the Standard Model by experimentally confirming the SM-like nature of the Higgs boson to a rather pessimistic outlook on finding BSM physics by testing concrete models. Different and novel strategies are envisaged, which involve less hypothesis testing and require more input from experiments and observations, as well as new tools and technologies. We follow many other philosophers who have argued that experiment and theory are independent, that science is not simply about theory testing, that experiment is not always subordinate to theory, but instead can become the dominant driver during some periods. As we explain in section 5.3, our notion of drivenness is related to, but distinct from pursuitworthiness. Pursuitworthiness operates at the level of individual research projects, be they models or experiments. Questions of pursuitworthiness figure prominently in the Moriond report and are part and parcel of the deliberations of the committees of large collaborations. However, although they are influential in the development of the field, they do not characterize the mode of the consensus as our concept of drivenness does. We are interested in the large-scale trend across a whole field of research.

4 Experimental Research Programmes

It is now commonplace that the traditional philosophical debates about theory dynamics, be it in the framework of Lakatos or Kuhn, have neglected the role of experiment. As Hacking famously expressed the central creed of the new experimentalism, "experiments have a life on their own."[28]

Another aspect of experimentalism pointed out by Peter Galison[29] is that rather than there being just one line of experimental and instrumental knowledge, there are breaks and discontinuities of experiment and instrumentation that occur at different times than theoretical discontinuities. Experimentalists work under a combination of theoretical and experimental constraints that can be long term, middle term, and short term. This brings, for instance, a general principle like gauge theory on a par with particle accelerators and a specific theory like the SM with a specific device like LHC. Galison argues for an understanding of science where different instances of experiment, instrumentation, and theory can each take the place of a central metaphor. The central metaphor is a unit in a periodized structure of an experiment, theory, or instrumentation (see figure 1). Central metaphors can exist at the same time, can partially overlap, and can vary regarding

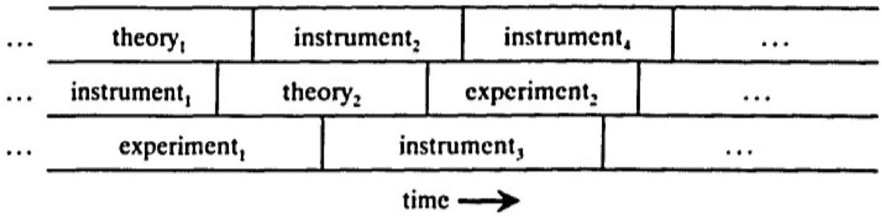

Fig. 1. Schematic depiction of a periodization of physics. The brick-like structuring shows how breaks can occur at different levels and different times, but the intercalation of the breaks provide an overall continuity across replacements. Source: Peter Galison, “History, Philosophy, and the Central Metaphor,” *Science in Context* 2, no. 1 (1988), 197–212, p. 209

their impact and duration. Galison’s model entails a plurality of partially autonomous layers, which are still cooperating on a global scale.

We have two related aims in the following discussion of Galison: first, to use his notion of central metaphors as a way of talking about communities as wholes; and second to expand on his notion of trading zones as a way to characterize how research programmes communicate and form consensus such that they can be meaningfully treated as wholes despite diversity and dissent, and a plurality of research agendas. Galison’s schematic depiction of periodization (figure 1) shows that theoretical, experimental, and instrumental* research can be central to a field. No structure is necessarily imposed over the other and there are visible discontinuities, which are bridged by experiments, theories, or instrumentation. This shows three things we wish to highlight: 1) experiments/observation and instrumentation are not always motivated by, or subordinate to, a specific theoretical framework; 2) there are different kinds of experiments and instrumentation, and there is not necessarily one continuous experimental or instrumental language. Rather, there can arise hybrid languages, which Galison dubs ‘creoles’, in which speakers of different languages can communicate; 3) there can be continuity of the field as a whole as the different central metaphors are overlapping on a larger scale, though it remains to be clarified how the communication and interaction between them looks. Galison motivates his view by elaborating on the so-called dynamic of subcommunities.[30] These subcommunities are part of the larger discipline, e.g. particle physics, and follow their everyday research regardless of any theoretical discontinuities. As Galison writes: “by granting a measure of autonomy to the practices of experimentation and instrumentation, we recognize what has become obvious to laboratory workers: the daily activities of instrument builders and experimentalists and their perspective on their discipline often differ strikingly from each other."[31]

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* For this paper we consider instrumentation to be a very broad field, including tools (also mathematical tools), algorithms, detectors, including their material design and the social context of their use.

What is of particular interest to us in Galison's account is the interpretation of a class of experiments, theories, or instrumentation that can coexist in a field as what we refer to as *central research programmes*. Research programmes can be theoretical or experimental, e.g. the development of BSM models or the hunt for new particles in the 1950s, but also finding a specific particle, studying a specific signature, improving detection methods. Building a new detector can be an instrumental research programme, with many different subtasks that relate back to experiment.

We find Galison's brick-wall metaphor helpful, but we will not adopt his framework wholesale as there are a number of points on which we wish to improve. For example, it is not clear when an experiment or an instrument is *independent enough* to constitute its own central research programme. Sometimes experiments are employed to test the immediate consequences of a theoretical prediction, or at least they are developed with theory-testing in mind. The question of centrality of the metaphor seems to be closely related to a question of autonomy. Further, and importantly for our discussion here, however, we find the retrospective and static character of the model to be a shortcoming. The determination of the different levels and layers is only assigned to periods in a historical analysis, which does not seek to explain any forward-looking character of science, e.g. groups and committees initiating experimental or instrumental research projects. Simply stating that experiments and instrumentation have been central does not explain why research programmes start, stop, increase, diminish, or generally why the bricks are structured in some given way—that is to say, there is no sufficient account of which exploratory research strategies are worthy of pursuit. These aspects will be explored within the next sections.

In order to resolve some of the issues regarding communication of the subcommunities, Galison recognized that the local coordination between various subcultures of science with different languages bore a resemblance to "trading zones"[32] in which communication proceeds in hybrid, "creole" languages. Nevertheless, "trading" comes with so-called "rules of exchange,"[33] with each side having different views on the exchanged objects and findings or even the exchange itself. A trading zone can involve shared concepts, but different ways of approaching them. Periods of radical changes for one subculture, e.g. a research programme, are not necessarily radical changes for the other: "moreover, the relative rigidity and foreignness of one subculture from another does not make crosstalk between the strata impossible; rather, it insures that as the trading domains become established, the structure of the enterprise as a whole has a strength that the antipositivists denied."[34] Coming back to the brick-wall model, we see that the partially autonomous bricks can co-exist and scientists can communicate from one to the other. Research programmes may depend on each other to a great extent in the sense that there are shared objects in the trading zone. In summary, we can use the brick-wall model and trading zones as a tool for the description of a scientific discipline like particle physics since it reflects that research programmes of different kinds can co-exist, communicate, and be to various degrees interdependent. There is not always a need for guidance by a theory, rather experimentation and instrumentation survive theoretical breaks, gaps, and can exist without theoretical targets, as long as trading is possible between existing research programmes, such that questions about progress and pursuitworthiness can be discussed.

5 Kinds and Expectations of Progress

The previous reflection showed that experiments can potentially act as central research programmes in a field like particle physics and can bridge gaps between theories. Galison provides us with a background against which we can further investigate these research programmes, by looking into more specific issues, such as how various configurations of the field are triggered. We argue that the changes in the brick-wall model are often about expectations of scientific progress. We therefore want to elaborate on a conception of progress that reflects that progress stems from various kind of research activities and allows us to expect, or to aim for, progress from all kinds of research programmes. This is central to our understanding of drivenness to which we turn in the following subsection.

5.1 Kinds of Progress

There are many theories of what progress consists in and how it is measured. Progress has been cashed out as empirical success,[35] an increased verisimilitude or truthlikeness,[36] an improved ability to solve problems,[3] unification,[37] increased explanatory power,[38] the accumulation of scientific knowledge,[39] or improved understanding.[40]

Many notions of progress are tied to comparisons of theories: a new theory is progress over an old theory if it solves more problems, offers more true predictions, provides a more unifying description of nature, etc. There is progress in science that is more connected to empirical and instrumental endeavors. For example, knowledge-how rather than knowledge-that is something that is frequently glossed over: “According to most philosophers, experiments are run in order to promote theory (…). Thus, according to most philosophers, improved theories account for the progress of scientific knowledge. (…) This asymmetry is a mistake. Technicians, engineers and experimenters (…) Are able to make devices work. (…) Only blind bias would say that such scientists do not *know anything* about nature. Their knowledge consists in the ability to *do* things with nature, not *say* things about nature.”[41] This conception allows *practical* scientific progress caused through experimentation or instrumentation. This is also something pointed out by Mizrahi,[42] who argues for an expanded knowledge-based account of scientific progress that explicitly allows for experimental knowledge and *know-how*.

We want to recognize *instrumental* progress, since improving instruments, data-analyzing algorithms, and/or detection capabilities also constitute scientific progress. Realistically, a certain amount of theory and experimentation is always required.* There are experiments, which were inconclusive as regards to theories or that had results which were difficult to interpret that were nevertheless the first to feature highly effective techniques of detection, data analysis, or simulation.[43]

If one regards science only as an endeavor to come closer to the truth, this kind of progress may be passed over, even if it facilitates other kinds of progress. Experimental knowledge can also be crucial when it comes to obtaining knowledge that something does not work or to uncover dead-end paths in research. For example, LEP and Tevatron increased our knowledge about the Higgs boson prior to its discovery by ruling out certain energy ranges. Similarly, we are learning about what can and cannot be the case when it comes to BSM models, like supersymmetry, by probing energy ranges and not finding particles. We consider this to be progress for a number of reasons, including that more targeted searches can be conducted, and that it frees up research so that other more experimental/theoretical paths can be pursued instead.

Excluding options can also solve existing problems in terms of making them less relevant or leading to the dissolution of problems. Experimental progress can also be achieved in terms of improved understanding as the ability to correctly explain and reliably predict relevant phenomena, or to simply accumulate more facts, independently of predictions from a theory. The fact that there are experimental, theoretical, and instrumental research programmes, allows us to speak of experimental and instrumental progress as well. Each kind of research programme can be progressive by its own standards and measures.

5.2 Goals and Expected Progress

Progress is a goal-relative concept. It may be impossible to foresee which scientific aims will actually be achieved, but it is possible to formulate goals that are more likely to be achieved by one kind of research rather than another. One can formulate a plurality of goals, as Mizrahi summarizes: “On the semantic account of scientific progress, the aim or goal of science is to get closer to the truth. On the epistemic account of scientific progress, the aim or goal of science is to accumulate knowledge. On the noetic account of scientific progress, the aim or goal of science is to increase understanding.”[44] It seems unwarranted to take a monistic stance when it comes to the goals or aims of science. Our picture, rather, is reminiscent of positions that state that cognitive aim of scientific inquiry has to be defined as a weighted combination of several different, and even conflicting, epistemic utilities (each with many paths to them).[45] Such goals may be to refine equations, to build a new facility, to prove a prediction, to detect a particle, and so on. Real progress means that these goals become "effectively recognizable," meaning that there are routines or tests for showing that the goal has been reached or approached. This is in line with a functionalist-internalist account of scientific progress[46], where achieving goals in a certain time-frame within a scientific discipline is considered progress. It seems to us that in such a field a plurality of different measures of progress exists. While we are not defending this claim here in detail, and thus grant the possibility that in philosophical reconstruction, different episodes of progress can be subsumed under a uniform notion, it seems to us very plausible that deliberations about expected progress will be necessarily pluralistic because participants in such discussions cannot look back at a well-defined historical period, but are embedded in ongoing research programs and their internal criteria of progress, and are forced to reach a broad consensus about which group of programs are the most promising ones at the moment. This is in line with "science progresses if and only if more useful exemplary practices are proposed," which are "defined as a particular way of problem defining and problem-solving, typically by means of problem-proposing, problem-refining, problem-specification, conceptualisation, hypothesisation, experimentation, and reasoning."[47] This expresses a micro-scientific consensus while the larger traditional units of Lakatos and Laudan were based on a macro-scientific consensus to assess progress. Shan is right to argue that it "is often difficult to identify the content of a macro-scientific consensus"[48] and that the relationship to the microconsensus is non-trivial. We agree; and this might anyhow be too tall an order for a pluralist on progress. But it seems to us that in a field as structured as particle physics one can at least identify central metaphors and assess the overall drivenness.

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* While in industry one may consider an exclusively instrumental driving force, such as launching a new commercial product, in science and in fundamental physics, this kind of progress seems always the stepping stone for experimental or theoretical progress (see Section 6).

Of course, no one can predict what groundbreaking discoveries will occur that could lead to rapid revolutions, or which scientific activities will endure long periods without significant progress. Still, scientific communities do pursue some research programmes more than others; there are hopes or expectations that these programmes will be successful. Expectation can be "future-oriented abstractions (that) are among the most important objects of enquiry for scholars and analysts of innovation. Such expectations can be seen to be fundamentally 'generative,' they guide activities, provide structure and legitimation, attract interest and foster investment."[49] Therefore, expectations can help us to understand scientific changes.

We can identify what are broadly the expectations of the field concerning the most promising goals from summary reports and strategy updates. Since we allow for experimental, theoretical, and instrumental progress, we can also *expect* *progress* from pursuing or initiating specific research programmes. Ultimately, the long-term goal of particle physics may be to have a new experimentally confirmed higher energy theory, but as highlighted before, there are intermediate goals, some of which set the stage for the achievement of future goals. While major changes to new theoretical frameworks certainly mark moments of significant progress, we want to stress that physics also makes progress along the way by achieving intermediate goals and this forces us to rethink the philosophical notion of scientific progress.

5.3 Pursuitworthiness

Having endorsed a pluralist notion of progress, it is important to distinguish our notion or drivennness from pursuitworthiness that in itself may support a plurality of research programs at a given point in time (or justify them in retrospect). While pursuitworthiness reaches back to the classical debates about theory dynamics[50] and played a role in the philosophy of particle physics experiments,[51] Laymon and Franklin have recently proposed a much broader notion and exemplified it in a series of seminal experiments in elementary particle physics from the last century. Different from acceptance or rejection, pursuitworthiness is about the promise of success. Such promise can be shown by (i) "formal considerations such as simplicity, coherence, robustness,"[52] that is, not being ad hoc; (ii) explanatory power with respect to experimental phenomena; (iii) practical considerations, such as expense, availability of expertise, and the potential to mount an experimental test. The goal of their book is to complement these largely theory-oriented categories with genuinely experimental ones and exemplify them, and their interaction, in case studies. Some of these, such as Wu's discovery of parity violation or the double-beta decay, had a clear theoretical target even though this was not to confirm a specific model. Others, such as the analysis of the beta decay spectrum, did not.

In the end, Laymon and Franklin come up with a catalogue of twelve lessons that they dub "The Many Flavors of Pursuit."[53] Among them are the value of negative results; the combination of partial acceptance and pursuit, especially when it comes to the model-independent exclusion of confounding factors after resolving the theoretical question; the value of replication; and that "experimental programs sometimes take on a life of their own even after their theoretical motivations have been abandoned and continue to be pursuitworthy."[54] While Laymon and Franklin exemplify the last point considering the fifth force hypothesis—interestingly this was also discussed at some Moriond meetings—one may see some research programs in present particle physics along similar lines. As discussed in section 3, the search for new physics has today moved away from being steered by searches for explicit BSM models or simplified models.[55] We are thinking here of precision studies undertaken to place bounds on the parameter space for new physics by investigating properties of the SM. But there are also other strategies with more specialized analysis, among them SM-EFT or SM + SUSY EFT, or to an increasing extent machine learning models.

Laymon and Franklin also show that a research program can change its character between being theoretically motivated and experimentally motivated, and that there can be open discussions about pursuitworthiness in the scientific literature including overall assessments, such as Rutherford's textbooks on radioactivity. The latter come closest to the level on which our notion of drivenness operates—albeit for a much larger community of more than 10,000 researchers. On that level characterized by committees, strategy papers, and large conference, the idea of a central metaphor becomes significant because it expresses what at a certain point becomes considered the prevailing mode of research.

To be sure, considerations of pursuitworthiness figure prominently in these discussions because they are not simply governed by majority rule. But pursuitworthiness operates on different scales, it is applied to small and large projects and programs, maybe to a large program by the funding agencies. Thus the question of drivenness cannot be reduced to a sum of the pursuitworthinesses of the programs making up the field of particle physics, but is based on a pluralistic consensus about what kind of progress is most likely that is cast in the kind of central metaphors that we found in the interview statements of the scientists.

6 Drivenness: A Global Characterization

Expecting progress in certain areas can motivate work on relevant theories, experiments, or instrumentation, each of which can then form central research programmes. One example is the theoretical work done by Gell-Mann, Zweig, and Ne'eman to organize and theoretically understand the so-called 'particle zoo' of the many particles and resonances that had been discovered up to the 1960s. The progress sought after was largely theoretical since it involved developing either a theory that provides a classification and ordering scheme, or a more unifying theoretical framework that reduces the number of elementary particles. Research along those lines was abundant and of high relevance to the field in general, so one could describe it as a *theoretical research programme.* Similarly, we can imagine experimental searches to uncover new physics constituting an *experimental* research programme.

That a scientific field can be theoretically or experimentally driven can be explained by the fact that the goals that are formulated in each case seem each to be best achieved by theoretical, experimental efforts. Such assessments on the level of individual research programs often lead to debates about pursuitworthiness, which as explained above may pit an experimental strategy against a theoretical one. Our question of drivenness is about when such a prevailing tendency is apparent on a larger scale. As discussed, the concept of a central research programme grants instrumentation, experimentation, and theory at least a partial autonomy, "without granting anyone the sole legitimate narrative standpoint."[56] We have now elaborated that if certain goals are in place, some research would be motivated more than others for some period of time, because more progress is expected from them.

In concordance with our aforementioned aim to find an underpinning for a notion of drivenness, Galison himself described this possibility as "(constituting) the central *driving power* of the field [cursive for emphasis]."[57] He states that even though there is a heterogeneity in the field, the time-specific arrangement (importance and abundance) of central research programmes and their motivation by local goals still allow for a more global characterization of the field as being driven by experiment or theory. We can now say that *drivenness* consists of at least two aspects:

1. Expectations of progress of a certain kind, namely more theoretical or more experimental, and
2. Central research programmes motivated by these expectations

Drivenness is then the dominant mode of research the field is in and hence constitutes a global characterization of the field. Connecting expectations of progress and the respective brick-wall structure of the field in this way allows us to identify the motor, or the driving force, of a field of research.

This driving force is typically constituted by experiment and theory. Despite being able to constitute a central metaphor (in Galison's sense), instrumentation also in particle physics is typically understood as occupying a middle ground between science and engineering—similar to what has been defined as "research-technology."[58] This means that instrumentation operates across the boundaries that typically separate disciplines or research groups. Although discoveries in high-energy physics (HEP) are often made possible by improvements in particle detectors, accelerators, and measurement techniques, instruments are also characterized by their *genericity*: they are not designed for a single application but rather consist of a variety of components suitable for a broad range of uses. This gives rise to a process of *dis-embedding* and *re-embedding*, where an instrument initially developed for one context can later be adapted to various scientific or industrial settings.[59]

Thus, while instrumentation in this broader sense can serve as a central metaphor or infrastructure within scientific practice, it does not necessarily function as the primary driver of a specific scientific field, such as particle physics. Instrumentation works more as a *creole*—an additional language that enables communication and exchange between research programmes.

As mentioned in section 4, there are theoretical breaks and experimental or instrumental discontinuities. Periods of drivenness can extend over time and may transition into one another due to important discoveries and achievements. However, it is not always the case that goals or expectations of progress clearly favor experiment over theory or vice versa, nor is it always easily determined which research programmes are more central. This situation could describe a balanced or “hybrid” mode. Moreover, it is by no means excluded that in an experiment-driven era theoretical approaches are pursued or vice versa.

A remaining question is how to figure out in practice what is driving the field at a given time. We will proceed in the following way: i) determine which goals are important for the field and what kind of progress is expected from their achievement; and ii) determine the research programmes at that time. For the first, we can draw on summary reports from major conferences, statements from spokespersons or questionnaires regarding the goals and the directions from which progress is expected. For the latter, we can draw on the research projects that were proposed and undertaken during a specific period according to reports or retrospective summaries.

It is important to mention that drivenness as such is a forward-looking concept, which describes the efforts of physicists to pursue a certain type of research. Retrospectively, deviations in the actual outcome can certainly be found, e.g. by unexpected discoveries or detours that were required for instrumental reasons. Unexpected discoveries can shape the field and often overshadow the previous structures of the field that may have been designed for a very different kind of discovery. Nonetheless, we want to take a closer look at these structures here and reveal the field’s mode of research—how it was being driven. For this purpose we employ two case studies. First, we consider the time between the mid-1970s, after the discovery of neutral currents and the *J/*Ψ, which marked an increasing importance of the Standard Model, up to the mid-1980s, where many of the SM’s predictions have been confirmed. The decade, which seems intuitively closely intertwined with the progress of a theory, the SM, also required experiments and encouraged the construction of accelerator facilities. Nonetheless, we will show why the field should be identified as theory-driven. Next, we look at the decade following the Higgs discovery in 2012, with large scale high-energy experiments. This “experiment driven” phase, which was the motivation for our paper (cf. section 3), will now be re-examined with the new concepts in mind and we will argue for the current experiment-drivenness of the field. The procedure in the case studies is as follows: we describe the initial situation, which discoveries and activities shaped the discipline beforehand, and then examine the goals in the field during this time based on a consensus and identify the predominant nature of expected progress associated with these goals. Finally, we determine what the most central research efforts are and how the experimental, theoretical, and instrumental programmes are related.

7 Case Study 1: A Theory-Driven Era Follows the November Revolution 1974

For our first case study, we look at the field of particle physics following the so-called *November Revolution*, triggered by the discovery of the *J/*Ψ particle in November 1974 by two different experiments (SLAC and Brookhaven). As a bound state of the charm and anticharm quarks, its observation provided concrete evidence for the quark model and reinforced its status as more than a mathematical framework. While the charm quark's existence had been theoretically anticipated, the discovery itself was unexpected, leading to a re-evaluation of experimental and theoretical priorities, reinforcing not only the quark model but the theory of strong interaction, quantum chromodynamics QCD, as part of the Standard Model. At the time, *Physics Today* described the discovery as setting "the world of high-energy physics afire"[60] due to the particle's remarkably narrow width at 3.1 GeV. Other major discoveries were also made in that time period, among them neutral currents in 1973 that supported the predictions regarding the electroweak theory developed by Glashow, Weinberg, and Salam (1959–1967). The theory was shown to be renormalizable in 1971,[61] meaning that the values of observables remain finite as the energy scale increases to infinity. The τ lepton discovered in 1975 was not a direct prediction of any given model and interest in heavy leptons was very weak. Van Hove went as far as to say of the muon neutrino "Nature cannot be so stupid to have two neutrinos to do the same thing."[62] However, the generational structure of leptons that the discovery suggested fitted well with the SM. It implied a third generation of leptons and thereby motivated the search for the bottom and top quarks, which were discovered in 1977 and 1995 respectively. While the SM had no explanation of why there are three and only three generations, all these discoveries were in line with its predictions.

Gilman recapitulates ten years after the November Revolution: "we can now see that the different currents of research and ideas converged in those exciting days in November. And within a couple of years afterwards, no one [....] would be found writing about this part of physics without at least a perfunctory bow to the Standard Model."[63] Starting from the mid-1970s, the SM was incomparable in scope and explanatory power to any other theoretical framework in particle physics before and gained considerable credibility due to the findings of the first half of the 1970s. This motivated the construction of large-scale experiments to test these predictions.

7.1 Goals and Expected Progress

Many high-energy facilities were designed and constructed to test and investigate predictions of the SM in the 1970s and 1980s, such as the Fermilab Booster and Main Ring (Fermilab, start of operation 1970), the Standford Positron Electron Asymmetric Rings/SPEAR (SLAC, start of operation 1972), Positron-Elektron-Tandem-Ring-Anlage/PETRA (DESY, start of operation 1978), Positron–Electron Project (PEP) (SLAC, start of operation 1980), Super Proton Synchroton/ $S\bar{p}pS$ (CERN, start of operation 1981), Tevatron (Fermilab, start of operation, 1983), Transposable Ring Intersecting Storage Accelerator (TRISTAN) (KEK/ Japan, start of operation 1986), and The Large Electron–Positron collider/LEP (CERN, start of operation 1989). The construction and operation of these facilities up to the mid-1980s yielded instrumental progress in terms of methods and mechanisms, reaching new realms of energies and precision through stochastic cooling, refined electron storage rings, and higher energy of proton-antiproton collisions.

In order to learn more about other potential goals of physicists at that time, we are specifically looking into the expectations around a long-planned facility, namely LEP, largely motivated by the discoveries of the early- to mid-1970s. "As for European particle physics, options for the future major high energy machine have converged on LEP."[64] Summary reports during the construction of that facility help to understand what kind of progress was expected long-term. To extend our findings we will also have a brief look into expectations formulated in the proposals of the Tevatron and the $S\bar{p}pS$ accelerator facilities.

The summary report originating from the 1978 LEP summer study, emphasizes the great excitement in the field due to the discoveries of the 1970s.[65] The existence of a third lepton family consisting of a top quark t, a bottom quark b, the tau lepton τ and a tau neutrino ν_τ had been theoretically defined, and only the τ and the b had been experimentally established by 1978. One goal was therefore to find the missing particles to confirm the SM predictions, which would strengthen the theoretical framework. Generally speaking, the physicists wanted to confirm more predictions and confirm them more accurately, it was not about finding new mechanisms as the theoretical machinery was already in place. The expectation of progress is thereby an instance of expecting theoretical progress, since even if experiments are employed, the progress eventually consists in further establishing and developing (or refuting) a theory empirically—the theory is the aim and the primary motivation to embark on these experiments.

The expectation of more accurate predictions is also realized within studies of the weak interaction bosons, the Z_0 and the W: "LEP as presently envisaged is the ideal instrument for a detailed study of the Z_0 and more generally for the analysis of the expected merging of weak and electromagnetic interactions. The study of the W mesons and furthermore the WWZ coupling is also of extreme interest in this endeavor."[66] Physicists expected to find the W and Z_0 at $S\bar{p}pS$ and the interaction boson of the strong interaction—the gluon g—at PETRA.

A central goal of LEP was increasing the understanding of SM processes: "PETRA (and PEP) will provide many tests of QCD so it is not profitable now to work in great detail on QCD effects at LEP energies. However, assuming the tests of QCD at PETRA work, it is already clear that LEP will provide further insights into the theory."[67]

However, a small part of research was also intended to study new phenomena experimentally without a given target theory: "The possibility of discovering new heavy quarks has been investigated."[68]

Nevertheless, we find expectations concerning the SM to be the main theme, which continued for the following years, as it can be seen from the 1984 CERN

School of Physics proceedings: “The CERN proton-antiproton collider has proved to be an extremely rich source of physics. Not only has it fulfilled all the expectations based on the standard model of the electroweak interaction in the discovery of the W and Z particles with the predicted masses and properties and the probable discovery of the top quark, but has turned out to be an excellent testing ground for QCD as clear jet events are easily observed.”[69] While SM research dominated and drove large experimental efforts, other theory research drove purely theoretical efforts. The success of the SM with its gauge symmetries suggested to theoretical physicists by 1980 that a more encompassing theory unifying the strong and electroweak forces could be devised, possibly including gravity. In string theory “widely disparate themes from several decades of theoretical physics have (…) converged to become parts of a single story.”[70] The extensive push towards string theory in theoretical physics also shows that there were genuinely theoretical research programmes that had basically no connection to experiment at all. The goals here were to solve problems by, for example, integrating gravity into the framework as a more unifying approach, to accommodate fermionic strings, or generally to increase our understanding of the symmetries of nature.

The theory-drivenness was so dominant that the philosophical idea emerged that there were no alternatives to the SM.[71] On the other hand, there was an increasing separation between string theorists and theoreticians connected to the large experiments, the particle phenomenologists.

7.2 Central Research Programmes

We have shown that much of the expected progress from the mid-1970s to mid-1980s was theoretical, and we will now summarize the nature of the actual research programmes formed during the decade. First, we notice instrumental activity in providing superconducting cavities, which were important for electron beams up to 100 GeV and superconducting magnets for proton beams, and many more instrumental endeavors of that kind. There were also many experimental endeavors at accelerator facilities. Many of the goals were either in place to accurately confirm predictions or to increase the understanding of the SM particles and processes. Here, the SM is the central research programme and the experimental and operational projects are being driven by it. One can still argue that the accelerator facilities were not built exclusively to inform theory as there is always an openness to unexpected experimental discoveries. This is explicitly acknowledged by Ian Butterworth, chairman of the UK Science and Engineering Research Council’s Nuclear Physics Board in 1982: “(The study of elementary particle phenomena) requires a broad attack producing a wide variety of results which have to be described in a consistent way by theory. Experiment has often led the way by uncovering unexpected phenomena which, though at first bewildering, are necessarily part of the fabric of the universe and have led to a deeper understanding of the nature of matter. The theoretical position has never been stronger than at present but we can never be certain that experimental advances designed to confirm the theory will not bring more unexpected revelations."[72] While unexpected experimental discoveries are always possible, according to the board, theory had "never been stronger" at this point in time.

7.3 Identifying the Field as Being Theory-Driven

We have now seen that the main goals of this era were connected to theoretical progress and research motivated by those expectations was mostly theory-oriented or purely theoretical. Experimentation at, and operation of, large accelerator facilities was motivated largely by theory. Most of the expected progress also stemmed from the further development and testing of theory, specifically in accurately confirming predictions, unifying phenomena, and increasing our theoretical understanding of fundamental particles and interactions. The mode of research of particle physics from the mid-1970s to the mid-1980s is, then, *theory-driven*. Let us now turn to our second case study, which will demonstrate a turn towards an experiment-driven era.

8 Case Study 2: A Turn Towards an Experiment Driven Era Following the Higgs Discovery of 2012?

A Higgs-like boson was discovered in 2012 at CERN and, by checking in the following years the properties predicted by the SM and investigating additional decay channels, it was indeed determined[73] to be the last missing puzzle piece of the SM. The SM is internally consistent, empirically well established, and has no statistically significant deviations. However, as outlined above, it is not generally believed to be a final theory and has some empirical and theoretical flaws that still require further investigation by precisely testing the SM and by devising and probing models of new physics. That experiment really is now driving the field can be seen in terms of the experimental nature of the goals and the avenues for achieving those goals.

8.1 Goals and Expected Progress

Our first question here concerns the present goals of the field. In addition to the passages cited in section 3, we will draw on statements of the European Strategy Group, which defines the optimal measurement facilities and experimental projects, providing recommendations and guidelines for a European strategy in particle physics. Two updates, in 2013 and 2020 respectively, propose visions for the short- and long-term future. In addition, we also consult the Particle Physics Project Prioritization Panel, a US equivalent, which released a 2014 strategic plan titled as "Building for discovery."[74]

In 2013, one of the major goals was the high luminosity update at CERN, but not only instrumental progress was needed.[75] Through the intense study of flavor physics and quark-gluon-plasma, new experimental strategies were thought to be needed as well. Furter, five lines of inquiry are listed by the Panel report that are "now driving the field":[76] 1.) Use the Higgs boson as a new tool for discovery; 2.) Pursue the physics associated with neutrino mass; 3.) Identify the new physics of dark matter; 4.) Understand cosmic acceleration: dark energy and inflation; and 5.) Explore the unknown: new particles, interactions and physical principles. These goals correspond to expected experimental progress. They also involve problem-solving, for example, determining "what is the new physics behind dark matter?" Solving such a problem could result in an eventual theory formulation, but this alone rather indicates the need for experimental groundwork, since the goals do not involve putting any particular models to the test.

Using the Higgs boson as a laboratory for new discoveries has been a recurring theme guiding research since shortly after the 2012 discovery. This seems to be closely connected to a theoretical framework, but the research questions are not necessarily framed in favor of any specific theory: "what principles determine its effects on other particles? How does it interact with neutrinos or with dark matter? Is there one Higgs particle or many? Is the new particle really fundamental, or is it composed of others?"[77] Rather, the goal seems to be to exclude options or find new unexpected features via experiment. A key tool in this respect is the search for signatures that are not necessarily related to a model. A clear indication for the interest in dark matter was the increasing preference for the signatures of "missing energy."[78]

In order to better gauge where the consensus lies on expectations of progress, we come back to our 2018 questionnaire (see section 2 and figures 5 and 6 in the appendix). In figures 2, 3, and 4 we see a change over the years with respect to confidence of what will and has been discovered at the LHC. The most striking change is of course the confidence that the discovered particle is the SM Higgs (those that strongly agreed that the particle is SM Higgs is up from 21 to 71%). There are also changes across the board in estimates of discovering a more complicated Higgs sector and in finding new physics in general. In general, prospects of confirming a BSM model are down and an openness to model-independent methods is increasing. This is seen in figure 5, where we asked about the most promising ways to find new physics in the next decade. The highest ranked answer was *(astro)-particle physics and cosmology* (26.13%), which includes non-accelerator-based observations. We also see that people are hoping to discover new physics by investigating *SM processes at high* Q^2 (22.03%). This can be seen as theoretical as well as experimental progress: validating already formulated hypotheses at higher energy scales is theoretical progress; developing the operational capacities and performing experiments in a new realm, however, can be instrumental or experimental progress. 12.35% of those responding favor using existing BSM models, while 5.39% favor developing and testing new ones. On the other hand, a total of 15.18% tend to either develop new analysis techniques or rely on other accelerator-based processes (6.92%, 8.20% respectively). In total, direct searches for BSM models constitute only 18% of the answers for the best methods of finding new physics. Options *A*, *B*, and *C* involve precision measurements of the SM and options *F*, *G*, *H*, and *I* involve making advancements elsewhere. Only options *D* and *E* are for direct model-guided or theory-guided BSM searches. Here we see a clear preference for model-independent methods. This is also apparent in figure 6, where we show responses to the question: *Let us now ask you for conditions under which you would recommend giving up theoretical research concerning a specific BSM model.* The most popular answers are *New experimental results have been found* (36.26%) and *I have low commitments in any* (34.64%). The latter answer already states that more than one third of the physicists that completed the questionnaire are not committed to any model and explicitly prefer model-independent searches. The answer regarding "new experimental results" indicates that experiments are needed to get an evidence-based direction for theorizing and model-building. From this we can see that more BSM model testing is not a favored way towards new physics and that physicists are looking to experiment to lead the way.

Finally, in the 2020 European Strategy Update regarding future endeavors, we again find an emphasis on advancing detector technologies, but also a highlighted urgency for an increase in understanding and solving problems with experiments, such as the understanding of the nature of the Higgs boson and the search for new massive particles: "The quest for dark matter and the exploration of flavour and fundamental symmetries are crucial components of the search for new physics. This search can be done in many ways, for example through precision measurements of flavour physics and electric or magnetic dipole moments, and searches for axions, dark sector candidates(…)."[79] This quote shows how wide open the searches have become and how some evidence from experiment is needed to guide the way forward.

8.2 Central Research Programmes

Recall that Central Research Programmes can be experimental, theoretical, or focus on instrumentation. If we look at suggested research endeavors,[80] we find the following proposals to achieve the aforementioned goals: "second and third generation dark matter detection experiments, cosmic microwave experiments, construct new facilities, reformulate the long-baseline neutrino program as an internationally designed, coordinated, and funded program with Fermilab as host."[81] The European Strategy Group emphasizes the need for a diversity of approaches and broad participation, further indicating a lack of clear theoretical direction: "Experiments in such diverse areas that offer potential high-impact particle physics programmes at laboratories in Europe should be supported, as well as participation in such experiments in other regions of the world."[82] Nevertheless, the Strategy Update committee also recognizes theory as taking part in achieving the aforementioned goals (as in providing and developing computational tools), but there is a clear emphasis on exploration, observation and experimentation in the report statements.

In addition, a great deal of new data is expected from future large scale accelerator facilities such as the Future Circular Collider (FCC) at CERN, Geneva or the International Linear Collider (ILC) in Japan, though they are not yet being built. A number of strategies are being developed and implemented to deal with the potential flood of data. This gives the impression that experimental research programmes can operate without a high extent of trading with theoretical research programmes, which leads us to consider experiments as the most central research programmes in today's field of particle physics.

8.3 Identifying the Field as Experiment Driven

In the previous section, we described the whole discipline, roughly from the mid-1970s to the mid-1980s, as theory-driven, since the expectations of progress were motivated mostly around theoretical or strongly theory-guided research. Now, we find the goals, based on the shared consensus of the field, to be primarily experimental. Progress is mostly expected from experimentation, with the majority of experiments not guided by specific models or testing theoretical predictions. This gives rise to experimental research programmes being the most central ones, which is radically different from the situation in the 1970s. Today, the confidence in models seems weakened and theories/models seem not sufficient to achieve the goals connected to expected progress. Of course, there are still theoretical endeavors: SUSY and other BSM models are still being pursued as avenues to possible new physics. However, confidence that pursuing BSM model predictions will be the way forward has diminished. As we showed in section 3, many interviewed physicists, spokespersons and strategy committees agree: experiment is now leading the way.

9 Conclusion

We began this paper by noting that a major change was occurring in the field of particle physics: after decades of being driven by successful SM research programme, we are seeing physicists increasingly turn to experiment for guidance. We analyzed this development in terms of a notion of "drivenness," a global characterization of what is driving a field of research. We cashed this out in terms of where major progress is expected to come from, according to the consensus of the field. We argued that during some periods there is a clear consensus that either theory or experiment will be the source of progress and that this can be established by looking at the summary presentations at major conferences and through our widely-distributed questionnaire.

In conclusion, while the field of particle physics once thrived under the guidance of theory, our analysis suggests that this era has given way to one where pioneering experimental research has become more crucial. This shift could mark a new chapter in the evolving story of particle physics.

Appendix

Here, we include some graphical presentations of the results of the questionnaires conducted in 2011, 2012, and 2018. The questionnaires consisted of between 8–11 questions about the models in particle physics as well as optional demographic information. It had on average more than 1300 respondents globally each year, among which roughly 54% were theorists and 40% experimentalists. The results were separated into theorists and experimentalists, but here average numbers are presented for simplicity, since we are not drawing anything from those distinctions. In 2011, there were 739 theorists and 696 experimentalists (total 1435); in 2012 there were 464 theorists and 439 experimentalists (total 903); in 2018 there were 896 theorists and 667 experimentalists and 95 identifying as other (total 1658). The questionnaires were distributed via an email list derived from those who have registered and submitted papers to the physics preprint archive, Arxiv.org.

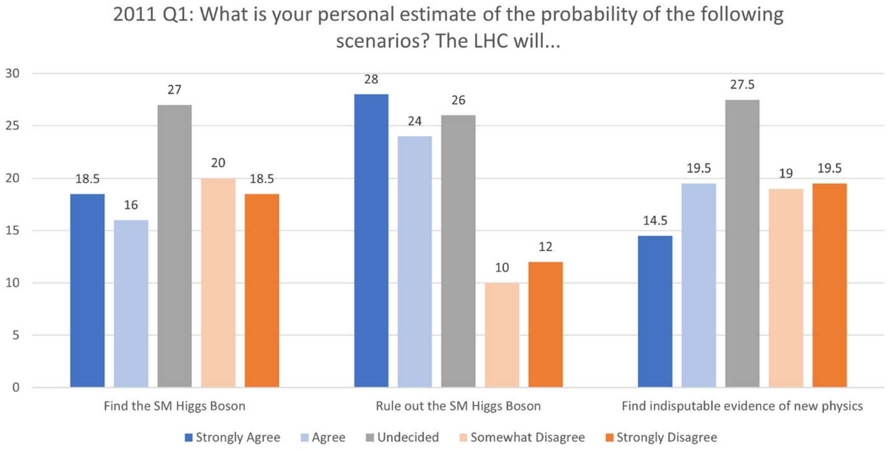

Fig. 2. Author's Questionnaire, 2011, results of Question 1. We inquired about personal agreement with various scenarios as indicated. Source: Figure created by authors

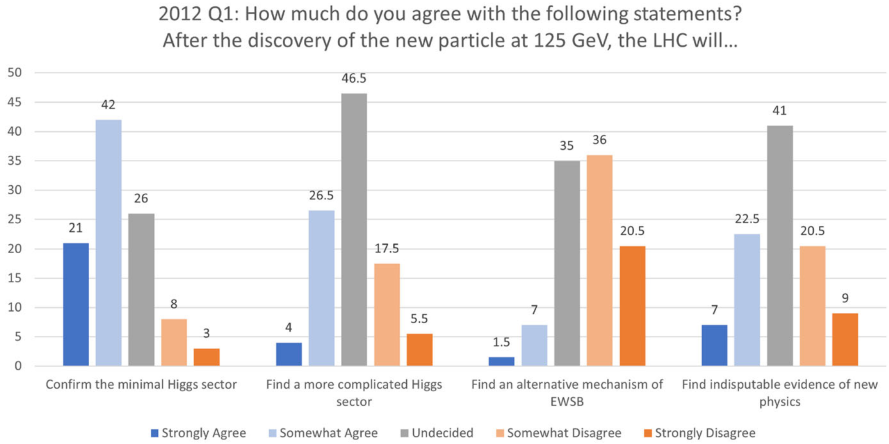

Fig. 3. Author's Questionnaire, 2012, results of Question 1. We inquired about personal agreement with various statements as indicated. Source: Figure created by authors

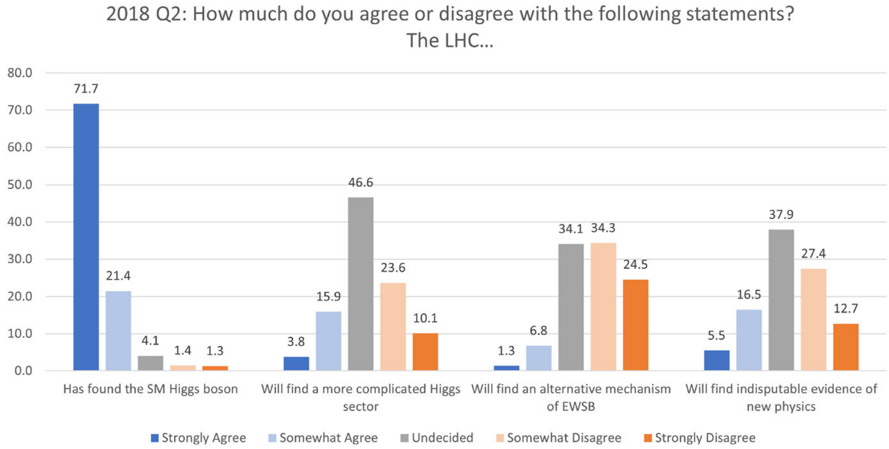

Fig. 4. Author's Questionnaire, 2018 results of Question 2. We inquired about personal agreement with various statements about the LHC. Source: Figure created by authors

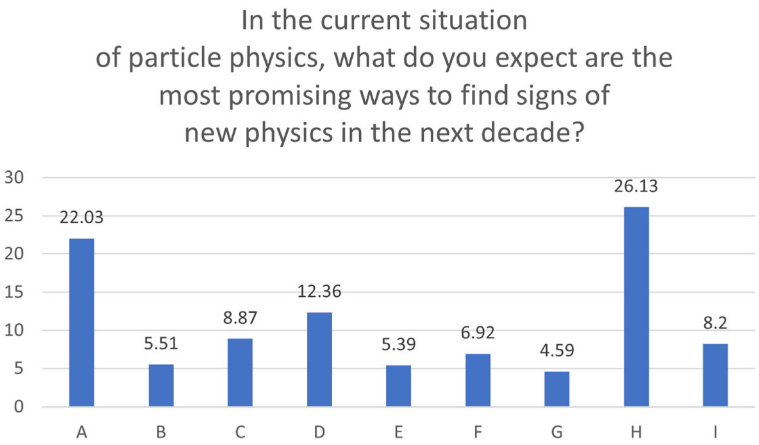

Fig. 5. Author's Questionnaire, 2018, results of Question 9. We enquired about a personal assessment of the most promising ways of finding signs of new physics in the next decade. The options were: A) SM processes at high Q^2; B) SM processes at low energies; C) SM processes of bottom, charm or tau; D) Continue direct searches for existing BSM models; E) Devise and probe new BSM models; F) Develop new analysis techniques and detectors; G) Develop new calculation techniques in theory; H) Astro-particle physics and cosmology; and I) other accelerator-based physics. We identify options D, E, and G, as primarily theoretical; options A, B, C, E, and I as primarily experimental; and H as mixed. Source: Figure created by authors

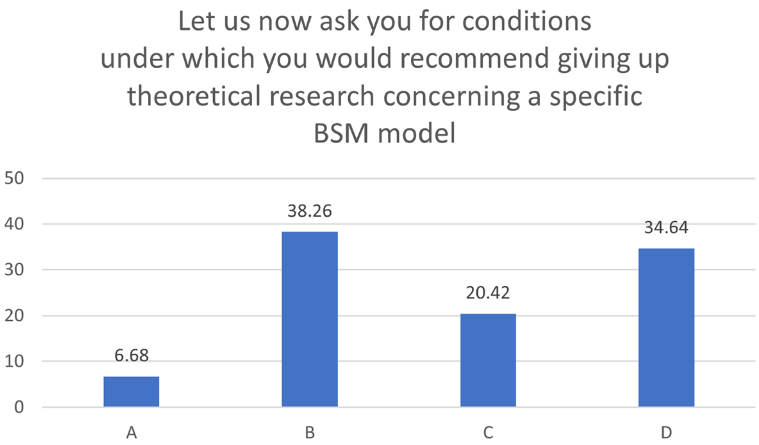

Fig. 6. Author's Questionnaire, 2011, results of Question 10. We inquired about conditions under which the responder would recommend giving up theoretical research concerning a specific BSM model. The options were: A) The remaining parameter space for the model has become very small; B) New experimental results have been obtained that can only be accommodated by this model in a very artificial way, that is, by violating some key theoretical virtue(s) of the model; C) I do not consider the reasons mentioned in a) and b) sufficient for abandoning theoretical studies of a model and would only recommend giving it up in favor of a new competitor model that is significantly more promising than the one I presently favor; and D) I have low commitments in any currently discussed model and favor model-independent searches/operator product expansions. Source: Figure created by authors

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Physics Institute, University of Bonn
Heinrich-von-Kleiststr. 22-28
53113 Bonn, Germany

Munich Center for Mathematical Philosophy, Ludwigs-Maximilian-University Munich
Ludwigstrasse 31
80539 Munich, Germany
e-mail: martin.king@lrz.uni-muenchen.de

Department of Philosophy, University of South Carolina
Columbia, SC 29208, USA