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Missing experimental challenges to the Standard Model of particle physics

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ABSTRACT

The success of particle detection in high energy physics colliders critically depends on the criteria for selecting a small number of interactions from an overwhelming number that occur in the detector. It also depends on the selection of the exact data to be analyzed and the techniques of analysis. The introduction of automation into the detection process has traded the direct involvement of the physicist at each stage of selection and analysis for the efficient handling of vast amounts of data. This tradeoff, in combination with the organizational changes in laboratories of increasing size and complexity, has resulted in automated and semi-automated systems of detection. Various aspects of the semi-automated regime were greatly diminished in more generic automated systems, but turned out to be essential to a number of surprising discoveries of anomalous processes that led to theoretical breakthroughs, notably the establishment of the Standard Model of particle physics. The automated systems are much more efficient in confirming specific hypothesis in narrow energy domains than in performing broad exploratory searches. Thus, in the main, detection processes relying excessively on automation are more likely to miss potential anomalies and impede potential theoretical advances. I suggest that putting substantially more effort into the study of electron–positron colliders and increasing its funding could minimize the likelihood of missing potential anomalies, because detection in such an environment can be handled by the semi-automated regime—unlike detection in hadron colliders. Despite virtually unavoidable excessive reliance on automated detection in hadron colliders, their development has been deemed a priority because they can operate at currently highest energy levels. I suggest, however, that a focus on collisions at the highest achievable energy levels diverts funds from searches for potential anomalies overlooked due to tradeoffs at the previous energy thresholds. I also note that even in the same collision environment, different research strategies will opt for different tradeoffs and thus achieve different experimental outcomes. Finally, I briefly discuss current searches for anomalous process in the context of the previous analysis.

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1. Introduction

The complexity and the number of events taking place in a typical High Energy Physics (HEP) detector necessitate the selection of a small number of interactions that will be recorded out of a vast number taking place, the selection of data to be analyzed, and the choice of appropriate techniques and statistical methods. I discuss this in Section 1 and note that over time, the efficiency and quantity of production of the recordings and analyzed data were enhanced by the use of automation—but at the expense of the much more direct involvement of physicists in the detection process.

I explain in Section 2 that this tradeoff affected selection of data at every stage of detection. The transition to automation was entangled with a number of structural changes in the ever more complex environment of high energy physics laboratories, including specialization and the increasing separation of theorists and experimentalists. It resulted in the emergence of two distinct (and opposing) approaches to detection: automated and semi-automated.

The semi-automated regime of detection turned out to be the key to the discovery of processes anomalous with respect to the existing background theory. It led to theoretical breakthroughs and the establishment of the Standard Model of particle physics. In Section 3, I discuss three discoveries at the Lawrence Berkley Laboratory and the discovery of J/psi particles at SLAC that were enabled by particular aspects of the semi-automated regime;

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these aspects were either absent or diminished in an automated regime that approaches detection in a much more generic manner. Briefly stated, the semi-automated regime is advantageous in broad exploratory searches loosely dependent on background theory, while the automated regime is most efficient in confirmation of very specific hypotheses in narrow energy domains.

In Section 4, I argue that as the semi-automated regime has turned out to be essential in discovering anomalies, the excessive reliance on automated system might be more likely to miss potential anomalies and thus preclude either theoretical advances or challenges to existing theory that such anomalies would imply.

Despite this danger, the excessive use of automation is virtually unavoidable in hadron collisions due to the staggering complexity of background interactions (which result from interactions of partons) that have to be combed for relevant data. Yet this necessity does not arise in collisions at higher energies *per se*: the electron–positron collisions can be handled with semi-automated system to a much higher degree than hadron collisions. Given the potency of semi-automated detection in discovering anomalies, then, it would be beneficial to divert funds from high-energy hadron colliders to the improvement of electron–positron colliders.

Investing in hadron colliders has been a priority because, due to technical advantages, they have been capable of reaching much higher energy levels than electron–positron colliders. I argue, however, that a threat that potentially key anomalies have been overlooked at lower energy thresholds as a result of previous tradeoffs (and their selection criteria), especially given the increased role of automation, should not be discounted. The chance of discovering such potential anomalies is greatly diminished by diverting majority of available funding to the study of collisions at the higher-energy thresholds.

Finally, I point out that the separation between the regimes of detection did not emerge solely as a result of the differing complexity of collision environments. Other factors including the organization and research strategy also contributed to the emergence of the separation in at least some laboratories working within the same energy range and probing the same kind of collisions. I discuss a relevant case of the search for charmed quark at Fermilab and Bevatron. Therefore, these factors might have profoundly contributed to the success, or the lack of it, in the searches under discussion.

In Section 5, I conclude by touching on current searches for anomalies in the light of the previous analysis.

2. Looking through a foggy lens: selection of data and detection of particles in High Energy Physics

Over the course of a few decades after WWII a dramatic change emerged in detectors and detecting techniques in HEP. Generally speaking, the microphysical processes at high energy labs produce complex patterns from which events of significance must be singled out.[1] The level of complexity has increased significantly with the introduction of the cloud chamber if compared to previous experimental apparatus in particle physics. But it rose to staggering proportions with the invention of the bubble chamber which used liquid instead of gas, as ''[o]ne day of bubble chamber operation could keep a group of ''cloud chamber'' analysts busy for a year'' (Alvarez in Pais, 1986, 491).

The complexity was further increased exponentially by the data rates production in proton–electron collisions in HEP colliders in the 1970s. As protons are composed of partons (quarks and gluons) a cross-section[2] will mostly represent interactions between the partons. Thus, the number of interactions that produce new particles of large mass decreases with the square of energy, as these interactions are being ''overtaken'' by the rate of patron interactions. This results in a dramatic decrease in the ratio of ''significant signals'' produced by such new particles to uninteresting background events. The large ''background'' of already studied phenomena deemed theoretically uninteresting has to be combed for the events (tracks) of significance. Designers of detecting systems cope with this by trying to surround the interaction points as completely as possible, while dividing them into small subunits so that each individual particle can be recorded. This results in a dramatic increase in the number of detecting channels.[3,4]

The limitations due to data selection have become increasingly pertinent with the increase in the complexity of detecting systems. The third generation chamber-type detector, namely the multi-wire proportion chamber, has 10^4 or 10^5 readout channels. Its complexity matches that of the human eye (Dissertori, Knowles, and Schmelling, 2003, 224). Even though one billion particles are emitted in each ''bunch'' from the source, the number of interactions is small because the channels are thinly dispersed. A typical detector can record events only once every 40 times the particle bunches cross. And it takes milliseconds for computers to read the details of such an event, thereby leaving out most interactions. It is thus crucial to make a proper choice for triggering of the reading (i.e., to choose a triggering hypothesis and establish the computer algorithms accordingly) as this will affect what type of interactions will be read out.[5]

Moreover, the process of the analysis of recordings is complex and consists of the following six phases: (1) scanning recordings where the particle tracks of interest are selected; (2) measuring the coordinates on the tracks of interest; (3) three-dimensional reconstruction of the tracks; (4) kinematical analysis using momentum and energy conservation laws; (5) statistical analysis of the obtained results; and finally (6) book-keeping.

The sheer quantity of data obtained in detecting systems presents a gargantuan challenge to the analysis. In a typical experiment, E-516 in Fermilab in the 1970s, ''after a year of data analysis, E-516 had reconstructed but 15% of its total data sample of 20 million triggered events; two thirds have been subjected only to preliminary analysis'' (Hoddeson et al., 2008, 274).

It is not surprising then, that many physicists felt it necessary to computerize both the recording and the analysis process. This resulted in a complex overhaul of detecting process from which two distinct approaches to the role of automation emerged. The

[1] A track of a desired particle is produced by exciting the particle to an energy level that knocks it out of its bonds within its current energy shell. For example, the energy of a particle with which an electron is bombarded has to exceed the binding energy of an electron in the atom, so that the electron leaves the energy level it occupies in the atom. Its motion results in an independent track that can be detected. Analogously, as the subatomic particles consist of similar energy shells, the incident energy has to knock them off the shell (i.e., the apparatus has to ''produce'' them) in order for some to leave their own tracks. In reality, many such events have to occur for at least some to be detected.

[2] A differential cross-section is the probability of observing a scattered particle in a given quantum (energy) state per unit of a solid angle, given the irradiation flux of one particle. In effect, if there is a sudden jump in the value of the cross-section at a given energy, this might indicate that a particle is being knocked off the shell because there are significantly more particles produced than at lower or higher levels of energy. That particular energy level potentially determines the mass of the detected particle. Physicists are thus looking for ''bumps'' in the cross-section (or in other words, the resonances between the incident and resultant beams).

[3] Also, the required data rate of production must be balanced by the increase of event rate per unit of cross-section (luminosity) with the square of the energy.

[4] The electron–positron colliders are easier to handle than hadron colliders in terms of data production because of the simultaneous decrease in reactions of broad and specific interest. We will return to this very important point later on.

[5] For details on the kinds and structure of detectors see Grupen (1996) and Kleinknecht (1998).

approaches were the outcome of the tradeoff between the need for the physicists to stay involved in each stage of selection in the detection process, on the one hand, and the emerging need for automation in handling ever increasing quantity and complexity of data, on the other.

3. Introduction of automation in High Energy Physics and its impact on particle detection

The first bubble chamber was constructed at the Lawrence Berkeley Laboratory (LBL).[6] It was also the first detector to be equipped with a semi-automated measuring machine. Based on a study of available machines used for similar purposes in other areas, LBL's Franckenstein was designed and applied successfully in 1957. This type of machine quickly became commonplace in HEP laboratories, as it saved grueling hours of plotting histograms and measuring coordinates. It also introduced the "fitting routine" which suggested possible particle candidates for each track of interest.[7] And the Monte Carlo simulations employed statistical methods to distributions in cross-sections to suggest significant "bumps" (resonances) in histograms.

By the 1980s, the trend was towards wholesale computerization. In a move typical for the period, Fermilab's Research and Development program focused on developing multiple parallel-processing computing systems (Hoddeson et al., 2008, 274). The immediate reason was the failure of the E-516 experiment due to the shortcomings of its on-line data analysis: "[O]ut of the near failure of E-516 emerged a concept for computing on a far greater scale … a concept that would affect most of the laboratory's subsequent experiments" (Hoddeson et al., 2008, 275). The expectation was that "such systems … would offer Fermilab experiments a thousand fold increase in cost-effectiveness. The combined reduction in cost and increase in computing power would allow experiments to use less biased trigger assumptions, record more data on tape, and simultaneously accelerate the data analysis leading to publication" (Hoddeson et al., 2008, 274–5). And the introduction of the silicon microstrip detector allowing more channels to be used simultaneously in detection created the incentive for more online and offline computing, as it increased the data rates (Hoddeson et al., 2008, 276). Finally, in one of the first experiments under the new regime, recording was mediated by the program (Hoddeson et al., 2008) as it, in effect, reconstructed (provided estimates) rather than straightforwardly detected hadronically produced charmed-particle events.

Anticipating the downside, which we will discuss shortly, the first Fermilab director, R. R. Wilson discouraged major computing innovations (Hoddeson et al., 2008, 239). However, under L. Lederman, who succeeded Wilson, a formal plan to institutionalize computing began in the early 1980s, while the next director, J. Peoples made sure that "the revolution in computing spread to all aspects of the work of Fermilab" (Hoddeson et al., 2008, 341).

At CERN,[8] very early on, computerizing the detecting process was perceived as the key to improving the success rate of the laboratory in comparison to its American rivals. Yet from the beginning, the *entanglement of the implementation of computerization and the division of labor and rigid hierarchical structure of researchers and staff* was more pronounced than in the American labs.

In the US, in LBL, Alvarez's vision was that of a laboratory run by independent experimentalists who both conceived and performed experiments and who were constantly in tune with the apparatus and every step of the detection process (Alvarez, 1987).

In Fermilab, the division of labor was different from that at CERN throughout the 1970s as well. Wilson continued developing the initial vision of Fermilab even during the collider phase; every in-house staff member provided lab services for outside users and thus became familiar with the beam and the entire detector. The close collaboration between theoreticians and experimentalists was encouraged as it became apparent that the environment of a big lab did not necessarily favor it (Hoddeson et al., 2008, 140). However, a dramatic change in the division of labor took place after Wilson's and especially after Lederman's departure.[9] The experimental physicists increasingly specialized in particular parts of the detector and "typically did not understand the entire detector." (Hoddeson et al., 2008, 276) And the number of physicists who can keep an eye on the details of both the theoretical aspect of the project and the apparatus, or even have a general knowledge of them, has diminished in experiments whose rapidly increasing complexity and size require ever more narrow specialization: "The increased scale of equipment, collaboration, size, and duration of experiments altered what it meant to be a high-energy physicist" (Hoddeson et al., 2008, 311). The only parts of experiments that many physicists seeking tenured appointments at their home institutions worked on were "building equipment or data analysis" (Hoddeson et al., 2008). This, in turn, unavoidably diminished the impact of the experimentalists, who were sitting on the boards making the decisions, on the theoretical aspect of research. It also minimized theoreticians' familiarity with, and potential impact on the details of detection.

Unlike the US labs that initially allowed a fair share of independence and control for experimentalists, the experimentalists' independence at CERN was diminished at the initial stages by the division of labor. A Theory Division was formed shortly after CERN's founding and was run by Niels Bohr until 1954 when its headquarters moved to Geneva (Hermann et al., 1990, 371). Shortly thereafter, the laboratory Group, under Lew Kowarski, was divided into three groups: a group that prepared experimental equipment, an administrative group, and a development group. The idea was that all divisions would be run by scientists. Yet a combination of seniority issues and goals set by the Board of Directors led effectively in 1957 to the emergence of experimentalists as servicemen. This began when the staff was further divided into the Division of Research Physicists, and Applied Physics and Engineering (Hermann et al., 1990 372–73): "[The] smaller research apparatus would usually be developed by the Experimental Physics group, but … some of the bigger items … would be taken over by the Applied Physics groups." At this point, the apparatus and its functioning in big experiments were left to the experimenters who were supposed to limit their activities to their building, maintenance, and operation. The senior staff complained about dividing staff into physicists, applied physicists, and engineers, in part because they felt they lost control over an important part of the experimental process: "They [said] that it would be dangerous for CERN's future to limit these groups to one kind of activity for all time: research always called for shifts from phases dominated by the construction of equipment to phases in which it was used experimentally" (Hermann et al., 1990). Yet such complaints were rejected by the Board of Directors (Hermann et al., 1990, 374). Moreover, within these

[6] Lawrence Radiation Laboratory (LRL) was renamed Lawrence Berkeley Laboratory (LBL) in 1959. Today its official name is Lawrence Berkley National Laboratory.

[7] The "fitting routine" consists of plotting the possible angles of interactions from two-dimensional recordings given the conservation of momentum and energy laws.

[8] CERN originally stood for *Conseil Européen pour la Recherche Nucléaire* or European Council for Nuclear Research. CERN no longer serves as an acronym for the laboratory's name.

[9] After Wilson resigned, a new strategy favored a much smaller number of large experiments (Hoddeson et al., 2008, 239) that could produce quick results and thus justify further funding (Hoddeson et al., 2008, 310).

boundaries, the senior staff or "the barons of CERN" as they were called, ended up with a kind of veto right over all decisions concerning how the house was managed.[10,11] The CERN experimentalists argued that Alvarez's LBL was an example of how the lab should be organized: "Taking Alvarez at Berkeley as their model, they felt that as builders they were entitled to certain privileges when it came to using equipment they had helped install" (Hermann et al., 1990 375). But they complained in vain.

Even Alvarez eventually felt that the trend of a more strict division between theoreticians and experimentalists as service personnel had won out: "My … criticism of the way particle physics is done these days is that the theorists have veto power over almost any experiment proposed by their experimental colleagues" (Alvarez, 1987, 198). Even though the strong attitude expressed here may be justified only with respect to the attitude prevalent in the organizational structure at CERN, the division of labor in other labs had departed substantially from the kind of independence that Alvarez enjoyed in LBL. The specialization of theoreticians and experimentalists so that they could handle narrow tasks was not helpful to Alvarez or Wilson's romantic vision of a HEP laboratory. The ensuing complex transformation of laboratories and the physics community amplified the role of experimentalists as servicemen of the apparatus; furthermore, their responsibilities were parsed in such a way as to enable engineers and staff with limited skills and knowledge to perform them.[12]

Gradually, the trend towards a sharper division of labor between experimentalists and theoreticians began to determine how the computerization of detecting was introduced in the labs. Thus, the new role played by experimental service personnel went along with an unbridled enthusiasm for the complete overhaul of the detecting process by computerizing at CERN. The attitude of the above-mentioned Lew Kowarski, as an equipment serviceman (Hermann et al., 1990, 545) at the beginning of the automation overhaul of the 1960s, was that *the evolution of data-handling in bubble chambers leads "towards the elimination of humans, function by function"* (Hermann et al., 1990). *This applied to triggering programs as well as to all phases of the analysis*. As a result, two instruments for the automated measuring of bubble chamber results were developed at CERN and offered to other labs. Out of 66 people operating them, half were technicians, and the rest were physicists focusing on a single task (Hermann et al., 1990, 547). External collaborators wrote software for the first and subsequent machines[13,14] and the lab distributed most of its data to other institutions for final theoretical analysis. And perhaps most importantly, results were produced on request by external theoreticians unfamiliar with the details of the apparatus and not involved in the detection process. The experimenters and scanners "apparently did what [these external—author] physicists asked of them" (Hermann et al., 1990, 548).

Once such an attitude was adopted in a lab, the experiments were seen as growing "naturally until they reach a point at which their data is [*sic*] limited by the amount of computing time they anticipate will be possible to squeeze out of the system" (Hoddeson et al., 2008, 274). The more data are processed, the more likely such an experiment is to succeed. *Thus, the feeling that gradually came to dominate the community was that the physicists were primarily "up against the technological barrier of computing power limitations"* (Hoddeson et al., 2008, 239).

In his historical account of particle physics, Pais (1986, 492) writes, "I well recall the initial reservations of quite a few members of the profession in regard to Alvarez's managerial approach[15] which, they felt, would remove physicists yet another step from experimentation done in the old ways," even though his was an approach that limited the use of automation compared to what future efforts. In time, however, the dominant view emerged "that either one accepts the new style as indispensible, or particle physics will languish. How else could the Berkeley group alone have measured 1.5 million events in 1968?" (Pais, 1986). Thus, in the era of the colliders, "the greater scale and complexity of the experiments planned for the colliding-beams program pushed the frontiers of computing" (Pais, 1986) while the extent to which it was pushed was enabled by the profound organizational changes in the labs.

Some leading experimentalists, however, including Alvarez (1968, 134; 1987, 192–3) who actually pioneered the computerization of detection, found the trade-off between efficiency and the removal of the physicist-observer a potential obstacle to the process of discovery. Very early on, while he was taken by the efficiency of the early stages of the analysis, Alvarez remained skeptical of the employment of Monte Carlo simulations and emphasized the limits of their use, for reasons similar to those pointed out by Pickering (1984) and that we will discuss later on[16]: specifically, the technique made it too easy for the experimenters pressured by theoreticians to see desired bumps in a cross-section as significant. Alvarez also emphasized the general limits of the wholesale automated regime of the analysis, in particular, the introduction of professional scanners (Alvarez, 1968, 134). As we will see shortly, he emphasized the lack of the generic nature of the automated regime as the key to the discoveries that won him a Nobel Prize.

Similarly, "Wilson had discouraged major computing innovations" (Hoddeson et al., 341). This was in accord with his bigger vision of a physics lab, notably Fermilab, as a place where an individual physicist has control over the entire process of detection, rather than highly specialized staff working on the narrow domains of it. His ideal was "science … pursued by lone independent explorers," and he organized Fermilab around this ideal as much as he could. To this end, he "opposed the expansion of the laboratory's computing because this violated his sense of what it meant to be a physicist" (Alvarez, 1968, 239), a physicist involved in every step of building and using the apparatus.

4. The comparative advantages of semi-automated to automated detection in discovering potential anomalies. Exploratory and confirmatory experiments

Certain events seemed to support Wilson's point. Fermilab lost the race with CERN to discover charm quark because of its failure

[10] A new director, V. Weisskopf, attempted to introduce change, arguing that "we definitely do not devote enough time for studying and understanding fundamental problems" (Hermann et al., 1990, 389). He attempted to impose clearly defined lines of research on experimentalists in order to shift their focus from maintaining and improving the apparatus to fundamental research. Yet his attempt largely failed, perhaps as a result of his allegedly paternalist attitude (Hermann et al., 1990), arguably a legacy of the managing style he experienced at Los Alamos, one that might not have been effective in the environment of a HEP laboratory.

[11] See e.g. Darriulat (2004, 33) as an example of decision procedures on employing new experimental techniques at Cern in the 1980s.

[12] Another problem was that a small number of physicists were competent working the accelerator; these were the physicists who produced the beam (based on an interview with Peter Koehler, a former director of the Development Division of Fermilab).

[13] The FILTER program that selects the tracks of interest was also introduced. It turned out that it was much easier to use the program in the spark chamber, even though its resolution was much lower than that of the bubble chamber. The CERN spark chamber produced a staggering 2.6 million pictures in three years (Hermann et al., 1990, 551). Alvarez rejected the offer to acquire the program in light of other priorities.

[14] In other labs physicists were also unsuccessful in writing their own software, so specialists were commissioned to produce modularized software and algorithms for triggering programs and the analysis of tracks.

[15] In particular, his pioneering use of Franckenstein programs for the analysis of tracks.

[16] See Section 3.

to recognize it at the analysis stage. And "[i]n almost a confession of E-516's near failure to detect the presence of charmed particles, Nash stated that isolating low cross-sections effects is 'limited by how many (correct) decisions physicists can make in live time'" (Alvarez, 1968, 274), not primarily by the available computing time.[17]

More importantly, the semi-automated regime of detection turned out to be the key to the discovery of anomalies (i.e. the processes seen as surprising and anomalous from the point of view of the existing theory) and these led to major theoretical breakthroughs. Such discoveries were enabled by aspects of the semi-automated regime that were either absent or greatly diminished in the automated regime. There are three examples of such major discoveries made in LBL run by Alvarez. Perhaps ironically, even though he introduced the computerization of measurement and analysis, he used automated analysis in a very limited manner, in part because his lab was run without a strict hierarchical experimentalists-as-service-personnel/theoreticians division. The examples from his lab, as well as the discovery of J/psi at SLAC (influenced, as we will see, by the LBL managerial style), illustrate that the balanced approach and the limited role of the computerized and generic analysis was essential in the process of discovering the key anomalies.

In 1955, at Bevatron, Alvarez's group performed experiments on interactions with negative pions. They used the 10-inch bubble chamber while building a 72-inch chamber, hoping to gain general insights that they could apply to the larger one. In performing a number of experiments with negative kaons, they unexpectedly discovered muons catalyzing fusion reactions as a result of the explorative analysis of the results (Alvarez et al., 1957; Alvarez, 1968, 192). First, the group noticed that a negative pion decayed to a negative muon, further decaying to an electron. They themselves, rather than trained scanners, analyzed the tracks with very broad criteria. It was shown at the time that a negative pion could not decay at rest in hydrogen, but if the pions decayed in flight, the tracks were supposed to vary more in length than they did in the actual experiments they performed (as there were too many muon tracks of the same length). They kept record of these "anomalous decays." (Alvarez, 1987, 194) Second, the physicists noticed something they deemed unexpected: a gap of a few millimeters between the end of what was supposed to be a primary pion track and the secondary track of a muon.

This resulted in a discovery that would have been very unlikely were professional scanners or physicists not involved in different aspects of the entire project involved[18]: it is hard to see how, without theoretical and experimental knowledge and skills, the gap could have seemed significant. (Alvarez, 1968, 134) *Nor would a group of theoreticians remote from the experimental situation pursue long deliberations and consult external scientists regarding such seemingly insignificant phenomenon.* And it was such a pursuit that led Alvarez's group to reach the following conclusion: an intermediary neutral particle (a small muonic-hydrogen atom) traveled through the hydrogen atom and left no track because it had no charge (hence, the gap). Such short-lived neutral particles catalyzed fusion reactions. Furthermore, they would have lacked the motivation to analyze the tracks of particles that were not strange[19] if they relied solely on the background theory, as there was no theoretical indication that such an effort would lead anywhere. Admittedly, Alvarez's group did not do this either at first. Only subsequently they realized that they had missed many pairs of tracks with such gaps simply because they were not supposed to look for them while assuming that they were unassociated events in a much cluttered bubble chamber. This realization was enabled by their involvement in every stage of the process (and a fact that they worked with the "dirty beam").

The discovery of Y^* (1385) and other short-lived resonance particles tells a similar story.[20] The discovery of the first of the so called strange baryon resonances "paved the way for the suggestion by Gell-Mann, at the international conference on the HEP at CERN in 1962 of the existence of an SU(3) baryon decuplet with spin 3/2" (Ashmore, 1969, 88).

In 1958, the 15-inch bubble-chamber and high quality negative kaon beam were introduced. Franckensteins were used for measurements more intensively than in previous experiments, but other stages of the analysis were still done by physicists involved in the entire process (Alvarez, 1968, 141). The detecting regime was still balanced. The KICK program was used to separate two distinct interactions that resulted in the strange particle production. According to Gell-Mann and Nishijima's strangeness rules, a negative kaon interacting with a proton turns into λ[21] (further turning into a proton and a negative pion) and one positive and one negative pion. But the existing predictions by the theorists turned out to be quite misleading. The bubble chamber was designed for long-lived particles, not even close to the 10^{-10} second range and "no theorist had suggested that we look for such particles" (Alvarez, 1987, 196)

The program for the production of histograms was still being developed, so the experiments were done by two graduate students. Both students, as well as the physicists who did the analysis of the recordings, were involved in the production of the kaon beam and were familiar with the bubble chamber from the initial stage of its construction (Alvarez, 1968, 141). This was essential, as the physicists' thorough knowledge of the apparatus led them to treat seriously the possibility that the peaked departure from phase-space distribution that they noticed could be reflecting a genuine event, rather than another insignificant effect produced by the apparatus background. [22] Not much other knowledge could have made such a conclusion seem justified rather than foolish, as only once before had the peaked departure from the phase-space distribution been observed as significant (Oliphant and Rutherford, 1933).[23]

The graduate students plotted the scattering processes showing the energies of two pions. The density of points did not vary smoothly from one part of the diagram to another but was crossed with two dark bands, one horizontal and one vertical. The result was a sequential pair of two-body interactions – a pion and a heavy object that came apart very quickly into λ and

[17] Ironically, under the new director, the best remedy was deemed to be further computerization of the detecting process.

[18] "We wouldn't have made it [discovery] if we had settled into full bubble chamber professionalism with clean beams and professional scanners carefully trained to recognize and record only certain criteria of events. We were still scanning our own film, so completely absorbed that someone was always waiting in line to take over one of our few film viewers whenever whoever was using it tired" (Alvarez, 1987, 192).

[19] Strange particles are short-lived unstable particles with a strangeness quantum number other than zero.

[20] In 1968, Alvarez was awarded a Nobel Prize for physics, for the use of the bubble chamber in a number of groundbreaking experiments. The results of the Y^* discovery were announced in Alston et al. (1960).

[21] λ stands for permissible combinations of particles based on the conservation of momentum and energy.

[22] The apparatus is massive and interacts constantly with the particles in the detector area, some of which occasionally exhibit patterns identical to the events for which the physicists are looking. These interactions have to be screened off in the analysis.

[23] In contrast, in Fermilab's post-Wilson period, as well as in other big labs, increasingly "[a]lthough most members gained some knowledge of the systems for which they had no direct responsibility, they typically did not understand the entire detector" (Hoddeson et al., 276).

another pion – not an immediate recoiling into λ and two pions as theory predicted ($\bar{k}+p->\lambda+\pi^{+}+\bar{\pi}$). (Alston et al., 1960) The heavy object was recognized as Y^{*} (1385 MeV). Many short-lived counterparts to the Y^{*} (1385) were soon discovered, starting with K^{*} (890) and Y^{*}_{0} (1405) by 1960.

In 1961, the discovery of ω meson – a vector meson with spin 1 that determines the electromagnetic properties of particles – resulted from a novel approach in the analysis of the decay of the potential particle into pions. Here, a discovery that was predicted theoretically in general terms occurred only because the experimenter very tentatively based his scanning and analysis on the theoretical constraints from the initial stage of detection and was familiar with the intricacies of the detecting process.

Bogdan Maglic, a visiting physicist, analyzed the film exposed in the LBL 72-inch bubble-chamber. The theoretical interpretation of the scattering experiments predicted the vector mesons (one unit of spin as opposed to π and K mesons). The conservation of momentum suggested that the particle would never decay into two pions, so the previous, failed, attempts to discover it had concentrated on potential decays into two pions and a gamma particle (Maglic et al., 1961).

The law of conservation of momentum did not prevent the decay into three pions—an option previously unexplored (Maglic et al., 1961, 178). But the triplets of pions could have been detected, as there were four neutral combinations for each event—even though the neutral pion could not be seen (it does not leave a track as it does not interact with the surrounding particles), the conservation of momentum allowed the calculation of its energy and direction. Maglic thus decided to concentrate on the proton–antiproton annihilations into five pions (two negative, two positive, and one neutral). Thus, even though the existing theoretical prediction specifically stated that the particle could not decay into two pions, the research which did not concentrate on the decay into combination of pions with a different spin (including, crucially, neutral ones) and a gamma particle, was successful. *As much as the decision was theoretically clever, it was also a result of a pragmatic experimental decision to try and look for measurable values in the bubble-chamber: the combinations of tracks with a neutral pion and its "invisible track". It was a case of theoretical and experimental versatility coming together, where the existing theoretical foundations are treated as broad tentative constraints rather than a road map to discovery.*

Although the analysis was performed manually as the computer system was still full of bugs, the automated regime probably would not have prevented the discovery, as it was used only in the measurement segment of the process, on the tracks already selected by Maglic in the explorative rather than the straightforward background and theory-driven fashion.

In the case of the discovery of the second element of the meson pair, the ρ meson; however, Franckensteins were used full time on different problems selected by the senior members of the lab. So Anderson and his collaborators (Anderson et al., 1961) made measurements directly on the scanning table. They applied the Chew and Low extrapolation method to predict dipion resonance (i.e., ρ meson). The discovery occurred because there were physicists available, capable and willing to work outside the automated regime used for the problems deemed higher priority by the senior staff. As a result of changing organizational structure of the laboratories this scenario has become much less likely; the problems and proposals deemed low-priority typically do not have a fighting chance.[24]

The LBL is a remarkable example of a laboratory that relied on semi-automated aspects of the detection. As one commentator notes, "Looking at the tabulations of particle properties, it is remarkable how often the Lawrence Radiation Laboratory figures in the references *particularly the earlier ones*" (italics by the author; Ashmore, 1969, 89) which coincides chronologically with the extent to which the discovery process remained semi-automated.

The discovery of the J/psi particle at SLAC[25] was a bold crucial step in establishing electroweak theory.[26] It occurred unexpectedly,[27] to a great extent thanks to the independence of the experimentalists involved, and due to a carefully crafted semi-automated regime of recording interactions and analyzing data. Thus, in 1974 the existence of the charm quark was established in the form of J/psi particles (neutron mesons with 3–4 GeV masses) as a combination of a charm quark and its anti-quark.[28] Even though it might be far-fetched to claim that the experiment that led to the discovery was "[p]erhaps the most revolutionary experiment in the history of particle physics" (Goldhaber, 1997, 57), it was certainly remarkable and stands out in the history of physics.

One set of experiments was performed at SPEAR (Stanford Positron-Electron Asymmetric Ring) at SLAC, using two systems of analysis, one at SLAC and another one at LBL. As the SLAC team that analyzed data emerged from the bubble-chamber experiments, it "had a tendency to produce visual displays of the data" (Goldhaber, 1997, 58). The scanning of recorded microfiche was done manually—again a legacy of LBL management. The experiment started as an energy scan without expectation of "narrow structures." The existing theory did not predict anything at the examined energy range. Thus, initially, the program of the scanning and analysis did not include the possibility of discovery at such an energy range, and the cross-section did not turn up anything unusually non-flat. But an apparent "inconsistency" at 3.1 GeV, at an otherwise flat result, was noted early and explored meticulously (Goldhaber, 1997, 58–9). Moreover, the team decided to change the energy pattern and experimented with the cross-section as a function of colliding energy while looking for predicted resonances, and this resulted in the discovery (Goldhaber, 1997 59–62).

Importantly, the analysts were part of the team who constructed the experimental protocol, prepared beams, scanned the recordings visually and introduced a new exploratory technique (the examination of the cross-section as a function of colliding energy). As a leading physicist on the team stated, "Our work on the ψ and ψ' was not influenced by [existing] theoretical predictions" (Goldhaber, 1997, 66). Had the recording, scanning and the analysis been fragmented and performed by the staff and servicemen in a standardized manner or run on the automated regime, the seeming inconsistency would almost certainly have been disregarded as irrelevant in light of the goals and existing theoretical ramifications. In such circumstances, it is hard to envision that recognizing the apparently irrelevant inconsistency as significant would have been anything short of a miracle. Instead, the inconsistency was treated as a convenient occasion to introduce the exploratory technique. And the scientists involved required only a bit of luck to stumble upon the

[24] For example, in Fermilab, initially open to diverse and numerous experiments, the number of smaller experiments was severely reduced during the tenure of Leo Lederman who succeeded Wilson. He sought efficiency in a few experiments deemed staple experiments by the upper management.

[25] The particle was discovered independently at LBL's Bevatron.

[26] In 1976, a Nobel Prize in physics was shared by the SLAC team and MIT researchers who used Brookhaven's synchrotron for the discovery.

[27] "The last completely unexpected discoveries were made 12 years ago, at Stanford and at Brookhaven, of the J/ψ meson and the tau lepton" (Alvarez, 1987, 198).

[28] For the chronology of the discovery and a list of the series of papers where it was reported see Goldhaber (1997).

significant bump at the explored energy level, as they had raised their chances by thoroughly and innovatively exploring that energy level. Crucially, even though they loosened the triggers (i.e., explored a broad range of hypothesis of interactions of interest) which increased the amount of data to be processes they did not "make up" for the growth of data by rushing the analysis with excessive use of the automated regime and/or trained scanner. Rather, they stuck to the semi-automated regime, including visualization and manual scanning, and this turned out to be crucial in the analysis of one-event displays in on-line analysis (Goldhaber, 1997).[29]

Judging by these and similar cases, the semi-automated regime creates an experimental environment conducive to exploratory experiments as it allows experimentalists more freedom to tinker with the triggering programs, scanning, and analysis.[30] Such experiments [31] typically do not aim at particular phenomena within very particular energy domains. If successful, they often result in discoveries of unexpected phenomena that challenge or greatly enrich the theory by providing surprising connections. They avoid strong background theory dependence, as the focus is on broad searches: the exploration of broad energy domains and a multitude of seeming inconsistencies across energy levels. Accordingly, devising multiple selection criteria and procedures for careful examination of backgrounds often deemed uninteresting by current theory are priorities in such experiments.

In contrast, the automated regime and confirmatory experiments reinforce each other. The automated regime is most efficient for the confirmation of a specific hypothesis that predicts the occurrence of a very specific phenomenon in a very specific energy domain. Such searches require analysis of vast numbers of narrow ranges of interactions (i.e. tracks). Trained or automated scanners, rather than a small group of physicists, can perform this task much more efficiently. In addition, such efficient automated scanning of narrow domains/tracks quickly generates vast amounts of data which, in turn, benefits from further automation of the analysis.

The exploration of inconsistencies in confirmatory experiments is typically confined to narrow energy domains. A multitude of background interactions is not explored as there is a strong reliance on the existing background theory to suggest which ones might be interesting. Thus, unlike in exploratory experiments, the selection criteria do not need to be varied extensively. Further, the experimental work, at least in part, is done on the direct (often external) request of theoreticians (the case at CERN, as noted previously); it is not surprising that theoreticians substantially removed from the experimental practice will favor choices that do not vary experimental conditions in an exploratory way, as the hypothesis they suggest are typically very specific.

An experiment performed at CERN in the 1980s resulted in a major discovery, namely, W and Z bosons. It was a typical case of a confirmatory experiment. The twofold goal was the production of particles whose existence was widely believed and the precise determination of their masses. With respect to the latter goal, the masses of particles had already been determined indirectly and fairly accurately, as it turned out, by multiple experiments (see Amaldi et al., 1987, 3) for the knowledge about W and Z bosons before LEP; Blondel, 1994, 418). What was needed was a "precise measurement" experiment.

Thus, proton and antiproton collisions were pushed by C. Rubia as a suitable test of standard electro-weak model. From the outset, the experiment was background-theory driven. For instance, one of the two detectors used in the experiment, UA2, was so specialized and dealt with such a narrow domain that "it could not measure particle charges except for limited regions where W decay asymmetry was [expected to be] maximal" (Darriulat, 2004, 38).[32] And detecting the particles was not such a major achievement given the state of knowledge about them at the time; rather, "what had been a real achievement … [was] the production of the weak bosons, not their detection" (Darriulat, 2004, 39). Actually, it was not even clear to the lab management that the experiment should be given priority as, based on the existing theory, most physicists were convinced of the existence of the particles (Dariulat, 2004, 39). They also doubted its feasibility (Dariulat, 2004). But the so-called intersecting storage rings in CERN were deemed to fit Rubia's idea very well (Darriulat, 2004, 26). And the fact that the experiment was conceived as a result of theoreticians' desires (the pressure to directly record W and Z was strong (Darriulat, 2004, 34)) fit CERN's practice and helped the case.

Many particle interactions in colliders are simply due to the massive size of the apparatus that surrounds the detecting area. A goal is to avoid recording such events ("artifact products") as much as possible.[33] Due to such complexity the confirmation strategy and the criteria that guide it is usually realized by the computer simulations. Monte-Carlo protocols are typically used in simulations of collisions to estimate the ratio of products of "real" interactions and artifact products. They are based on the existing theory through the choice of the parameters on which to focus and which exact data to fit. As such they can play a decisive role in the formation of the kind of the expected results (Grupen, 1996, 202).[34,35]

The experiment that led to the discovery of weak neutral currents was one of the first major experiments in which the use of Monte Carlo simulations was central to the production of the results. It was suggested that some interactions between neutrons and nuclei are mediated by neutral (Z) bosons (responsible for weak neutral currents). In this case, the lack of muons in relevant interactions was deemed evidence for the existence of such

[29] They worked in the electron–positron environment which is much easier to handle with the semi-automated regime than the environment of hadron collisions. We will turn to this issue shortly; arguably, their attitude was a result of the habits the team acquired in the bubble chamber in LBL.

[30] E.g., a similar story could be told of the tau lepton discovery at SLAC.

[31] See Steinle (1997, 1998) on explorative experiments and their systematic nature.

[32] A striking example of a confirmatory experiment that illustrates how strongly background-theory driven experiments might turn into idle quests was the search for the proton decay. It was initiated by a few leading theoreticians, even though it was regarded as completely uninspiring before their initiative. The financially and technically exhaustive search turned out to be futile—the half-life of proton decay was found to be very close to the value suggested before the theoreticians' initiative, close to infinity. (For the review of the literature concerning the issue see (Nishino et al., 2009).)

[33] This painstaking process can be complicated by the failure of a single circuit in the detector which might be reflected in the recordings as an event of significance. The analysis of trigger biases is run on different conditions to discard as many of these as possible.

[34] The "tight" triggering assumptions used in confirmation of specific hypothesis have presented a problem from the outset (Hoddeson et al., 2008, 273), and one reason for introducing the automated regime of detection was the hope that it could loosen them. Therefore, typically the goal in shaping the triggering program is to be neither too tight nor too broad. However, a gain in looser triggering assumptions has led to increased reliance on computerized analysis as the amount of data has grown exponentially. Reliance on the automated regime increased as "much more off-line computing was available;" soon, it was deemed necessary "to handle the greatly increased amount of recorded data" (Hoddeson et al., 2008, 278). As mentioned earlier, the SLAC team that discovered J/psi, however, avoided making this choice for various reasons.

[35] Monte Carlo protocols play another function of a more general significance. They are based on the backgrounds produced in known processes and include the estimates of resolution of the detector, angular acceptance and efficiency. This determines the range of discovery of the collider for a given mass.

currents forces (and ultimately for the unification of electroweak forces). Some particle interactions looked like such evidence, but they were byproducts of interactions with the apparatus surrounding the detecting area (artifact products). Thus, the Monte Carlo simulations gave estimates of the ratio between interactions caused by weak neutral currents and artifact products.

Pickering's (1984) detailed analysis of this case suggested that essentially it was the same interpretation of the experimental results which was received with skepticism in the 1960s but and accepted as discovery in the 1970s. According to Pickering (1984, 97), Fermilab's initial failure to discover weak currents was simply a result of their use of strict criteria in treating results of Monte Carlo simulations.[36] The theoreticians' expectations of detecting neutral currents might have influenced the change in the attitude. As J.C. Ward, a leading neutrino experimenter commented, "Theorists all over the world really started screaming for the neutral currents" (Ward in Pickering, 1984, 194).[37] Be it as it may, the experiment was an elaborate study of theoretically anticipated phenomenon that had been produced a decade earlier and an attempt to confirm a specific hypothesis. The success of the search hinged on the interpretation of statistical estimates provided by Monte Carlo simulations, not on any of the aspects of detection indispensible in exploratory searches.[38] Perhaps not surprisingly, CERN triumphed in the race to discover neutral currents, given that its organization was conducive to the performance of confirmatory experiments.

5. The automated detecting regime, colliding energy, and potentially missing anomalies

It is certainly possible that certain aspects of an experiment might be exploratory while others are closer to confirmatory goals (characterized here). In general, however, the experimental circumstances will enhance one of these aspects: the excessive use of automation at various levels of an experiment indicates its primarily confirmatory nature; less emphasis typically enhances its exploratory side. As the above examples indicate, the semi-automated regime is crucial in discovering potential anomalies. Thus, an excessive reliance on automated systems seems more likely to miss anomalies, precluding either unanticipated theoretical advances or challenges to the existing theory.

Weinberg says the following of the nature of the Standard Model: "The standard model *cannot* be complete, *or even precisely accurate* [author's italics]. There are reasons for this conclusion, both aesthetical and physical" (Weinberg, 1997, 123).[39] The key segments of the Standard Model have been built from the ground up, from fairly autonomous segments (20 free parameters) often almost directly from the experimental results. Which segments of those concerning higher energies are "precisely accurate" has been up to the experiments to decide. Weinberg believes, perhaps overly optimistically as our analysis will suggest, that discoveries at higher energies will resolve such uncertainties.

The overlooking of anomalies crucial for theoretical breakthroughs was always present in particle physics; its likelihood increased with the increase in the complexity of experimental apparatus and techniques, and with the higher energy thresholds. A classic example of a discovery missed in the early days of particle physics is a 1928 experiment by Cox (1928) and his collaborators. They suggested that electrons in the beta decay are polarized because they "double-scattered" with different probabilities to the right and to the left. At the time, the theory did not allow the violation of the principle of the conservation of parity that the experiment seemed to violate. It was thus not even clear theoretically what exactly double-scattering of electrons would mean and whether it made sense. The violation of parity was suggested two decades later by the introduction of weak interactions (Lee and Yang, 1956) and was confirmed by various experimental searches. This experiment was ahead of the dominant theory, and its result could only be accommodated within a significantly changed theoretical framework. But the theoreticians did not recognize the discovered phenomenon as significant.[40]

Similarly, Einstein (1905) was the only physicist who noticed the fundamental significance of the measured results of entropy. [41] As a result he devised a quantum (particle) theory of light in defiance of the wave theory that was deemed a clear winner by the physics community at the time. The experimental result that turned out to be crucial would have been overlooked had it not been for Einstein's theoretical effort. This is an example of how the theoretical significance of an experimentally recorded phenomenon can be overlooked.

A HEP laboratory is significantly more likely to *produce a potentially key phenomenon and fail to take notice of it at any stage of the elaborate detecting process*, let alone recognize it as significant, than a laboratory in pre-HEP experiments. Moreover, theoretical predictions of experimental phenomena can more easily fail to be confirmed at the level of detection, let alone those details which are not predicted by the existing theory. An illustrative example is the case of a SLAC team missing the discovery of pions even though their apparatus was producing it in a required way at the outset of HEP (Pais, 1986, 479). The physicists relied on the fundamentals of the theory to suggest the energy-range in which pions are to be discovered. It turned out that the experimenters were based on an incorrect estimate of the energy of the alpha-particles they were producing, as they thought the apparatus could not reach the 95 MeV needed for the production of pions. However, the so-called Fermi shift (McMillan and Teller, 1947) adds kinetic binding energy of the impacted nucleus to the initial energy of the alpha particle which then suffices for pion production. The apparatus was actually producing pions in sufficient numbers to be detected, but the experimenters guided by the existing, albeit broad, fundamentals of the theory in conceiving the experiment did not even attempt to establish the detecting machinery for pions. Instead, they concentrated on increasing the energy of the beam. Meanwhile, pions were detected in experiments with cosmic radiation.

In contrast to the missing pions, the previously discussed discovery of neutral omega bosons was the result of an experimentalist taking the existing theoretical constraints very provisionally. But at CERN, physicists *missed the discovery of neutral bosons* even though they recorded the needed events for their detection. They were even confused about the reason for this miss. Kowalski, in accord with his desire for full computerization of the detecting process, argued that the miss was a result of the detecting process at CERN being inferior to that of LBL (where the bosons were detected) due to the inferior computing power of

[36] Pickering's account was vigorously disputed by Galison (1987) and as vigorously defended by Schindler (2010) and Pickering (1990) himself.

[37] See also Schindler (2010, Section 7).

[38] The statistical analysis of data is also affected by the fact that the data to be produced serve a strict confirmatory role in cases like this. The exploratory statistical methods (Tukey, 1977) are far less useful for such tasks as the constraints on the discovery process are strict.

[39] For instance, there are more than 20 intrinsically unrelated numbers, namely, the masses of particles and coupling constants that reflect the strength of interactions, postulated by the Standard Model.

[40] See Franklin (2005, 23–30) for an in-depth analysis of this case; see also Alvarez (198, 199).

[41] For a detailed analysis of this case see Norton (2007).

the automated detecting regime (Hermann et al., 1990, 548). Ironically, as we saw, the discovery occurred because the physicist who made the discovery was involved in every step of the detecting process, while the computer was not used in the analysis as it was full of bugs.

It is not surprising, then, that experimentalists take seriously the possibility of missing potential anomalies in HEP collider environment.[42] In this vein, W. Panofsky says the following of the (never realized) plans of SSC where all but one in 10^8 events must be rejected in triggering:

> The experimental consequences of this circumstance have been studied extensively for processes that have been theoretically conjectured in the new energy range accessible to the SSC, and detector designs, and the triggering algorithms that go with them to isolate such processes, have been conceived. Although, when looking at specific process, the analysis problem appears to be tractable, the tantalizing question remains whether and how one can intelligently develop selection criteria that will not also prohibit the discovery of phenomena that have not been conjectured in advance to occur. In other words, what is the extent to which we are negating the discovery potential of very-high-energy proton machines by the necessity of rejecting, a priori, the events we cannot afford to record? (Panofsky W.K.H., *Particles and Policy*, The American Institute of Physics, 1994, p. 133)[43]

Yet despite one's awareness of this tantalizing possibility, and even though the automated system of detection in colliders is less conducive to the discovery of potentially crucial anomalies, it is virtually impossible to maintain a semi-automated approach to detection in colliders probing hadron collisions. Hadron collision, unlike electron–positron collisions, produce vastly more background interactions as a result of hadrons splitting into partons. In such collisions, the number of background events (i.e. already familiar interactions between partons) increases with the increase of energy, while the number of events that might be of interest decreases. In electron–positron interactions, however, the number of both background events and events that might be of interest decreases with the increase in energy. Thus, one has the luxury of treating the electron–positron interactions with a semi-automated regime, thereby reducing the likelihood of missing potential anomalies; for example, the SLAC team had the choice of avoiding excessive automation. In hadron collisions, however, distinguishing a few events of possible significance from a vast background of uninteresting background events is a gargantuan task that requires excessive automation.

Now, given that semi-automated detection is instrumental for explorative searches that have a chance of detecting potential anomalies, and given that it is essential not to rely heavily on automated data-processing in such cases, the fact that the electron–positron colliding environment is much better suited for semi-automated detecting regimes becomes crucial.[44] There are, unfortunately, serious technical limitations to electron–positron searches at higher energies. For example, the so-called loss of synchrotron radiation makes electron–positron colliders very hard to cool down at comparatively low energies (unlike proton–proton colliders). And the number of particles that can be "crowded" in a given area of the cross-section is severely limited in such colliders (Panofsky, 1994, 135–142). Overcoming these obstacles by innovation becomes a priority given the extent to which potential hidden anomalies discovered in the J/psi and other similar cases of successful semi-automated detection, provided theoretical supplements to or challenged the aspects of the Standard Model. Perhaps even a portion of the enormous resources that have been invested in hadron colliders could address substantially these technical obstacles.[45,46]

The lion's share of resources in high energy physics is allocated to hadron colliders because they can currently reach much higher energy levels than electron positron colliders. A standard pattern seems to be that once a theory is believed to have been confirmed at a particular level of energy, physicists focus on research at higher energy levels. However, key anomalies may have been missed at previous threshold energy levels, given the selection criteria established by the tradeoffs between automation and exploration at such energy thresholds. And the chance of discovering potential anomalies is greatly diminished by diverting a great portion of funds to the study of collisions at higher-energies, especially given the increased use of automation that makes searches for anomalies less effective.

Certainly, the shift of focus to the new energy level deserves more deliberation. Missing phenomena might be discovered at the new energy level, as well as the level at which the Standard Model was thoroughly tested. As the Standard Model is made from inherently independent segments, the fact that a segment has been well confirmed does not indicate that the search at that energy level has been sufficiently exhaustive. As in the case of the J/psi discovery, the search was ongoing at a higher energy level; however, the unexpected discovery not only occurred at a lower level but also required a reformulation of the segment of the Standard Model that concerned the higher energy level. In short, a novel insight at a particular energy level has resulted in foundational restructuring in the past (in the case of Y^* in terms of life times of particles) and will likely require at least major "fine tuning" in the future (for a similar argument see e.g., Cobal, 2006, 268).

The separation between the regimes of detection, however, did not emerge solely as a result of different conditions in different interaction environments. As shown in Section 3, a number of factors contributed to the emergence of this separation in laboratories working in the same collision environments. It is thus important to understand the extent of automation at the same energy level and in the same interaction environment and to compare the consequences of each approach.

Consider, for example, the significant differences between the Bevatron search that collided protons and beryllium and resulted in the J/psi discovery independently of SLAC and the analogous failed search at Fermilab. Here we see an enormous rift in priorities.[47] On the one hand, the Fermilab team operated on

[42] As a result of such worries, some prominent theoreticians have set out to develop alternative theoretical approaches to the Standard Model (e.g., Georgi, 2007).

[43] See also Jenni (1994, 318, Section 11) for a similar worry regarding the Atlas detector at CERN's current LHC.

[44] For a detailed analysis of the advantages of scanning interactions in electron-positron environment and the ideas for improvements of luminosity at high energies see Bethke (1994) as well as http://www.linearcollider.org/about/Publications/Reports-and-statements.

[45] The official estimate of the initial cost of the Large Hadron Collider at CERN has been estimated at around four billion dollars. (See http://user.web.cern.ch/user/LHCCost/2001-10-16/LHCCostReview.html) Also, the project took about thirty years to be realized.

[46] The current theory suggests that the background complexity might increase significantly in the electron–positron environment but only at the collision energy domains close to those currently achieved by hadron colliders. (Panofsky, 1994, 141) Even if this turns out to be the case, it leaves a vast energy domain to be explored efficiently for potential anomalies given the current energy capacities of electron–positron colliders.

[47] It should be pointed out that even though the SLAC team was working in the electron–positron environment, it could have chosen to prioritize automation in the spirit of understanding HEP problems as primarily problems of computing power. The LBL habits of the SLAC team probably contributed to a different choice.

the production of electron–positron pairs (by a proton beam collision with a fixed beryllium target), but at least in part due to an unfavorable combination of priorities, it did not produce a combination of a refined technique of analysis and nuanced triggering procedures; nor did it produce a (potential) explorative search that could have led to the J/psi discovery. The Bevatron team, on the other hand, was successful.

The Fermilab researchers continuously had to "struggle against the high backgrounds in their data" (Hoddeson et al., 2008, 177), in particular, the high background of leptons. They assumed this was due to "limitations in the experimental apparatus" (Hoddeson et al., 2008, 177) and did not pursue this occurrence inconsistent with the existing theoretical expectations in explorative ways. Such a pursuit, it turns out, could have easily resulted in the J/psi discovery. The group, led by L. Lederman, "realized that its highest priority was improving its "trigger" (Hoddeson et al., 2008, 176) to eliminate high backgrounds. The feeling, not at odds with Lederman's positive attitude to computerization, was that a priority in dealing with the problem was the development of "more sophisticated analysis programs" (Hoddeson et al., 2008, 177) and the replacement of computer equipment "that was far from the state of the art" (Hoddeson et al., 2008, 176). It turns out, however, that the high background was caused "by two unexpected physical effects that were not understood in early 1974" (Hoddeson et al., 2008, 177) and that were due to J/psi particles. While waiting for the improvement of the apparatus, including more sophisticated computer equipment and programs to eliminate the high production of leptons, they focused on attempting to identify the source of the direct production of leptons in the form of an already known particle (a vector φ meson): "The group speculated that the direct leptons could come "primarily from the phi" (Hoddeson et al., 2008, 180). Lederman noted that "lulled into a lack of urgency", they "did not push the issue" (Hoddeson et al., 2008, 180). As a result, "the [subsequent] realization that their direct lepton signal was itself largely due to the newly discovered family of particles", the high background being the unrecognized indication of this, "was particularly galling" (Hoddeson et al., 2008, 181).

The Bevatron team did not characterize the problem of detection as a struggle with computing power. Rather, from the outset, they looked to the development of triggers and analysis procedures suited for a broad search, as stated in the proposal for the experiment (Ting, 1977, 238). It also displayed very little reliance on existing theory. In fact, the idea was to set up the apparatus for the search of narrow resonances not predicted by the existing theory (Ting, 1977, 241). The backgrounds were sorted out by a systematic procedure necessitated by the broadness of the search.[48]

The physicists trained in Alvarez's LBL and the Bevatron team developed a distinctively different approach from members of the Fermilab (at that particular stage of its development) team; they focused on developing (for several years in the Bevatron case) detailed multiple-step detecting procedures suited for broad explorative searches instead of placing their bets on benefiting from increased efficiency by automating the process. Lederman, however, said he gave preference to wholesale improvement of the equipment and to moving on to the study of higher energies whenever possible (Lederman and Teresi, 2006, 316).[49]

[48] This was the key to the discovery by SLAC team as well. The team developed the technique that enabled them early on to distinguish relevant electron-positron annihilations into hadrons from the background (cosmic rays, beam-gas interactions and muon-pair production) (Goldhaber, 1997, 58).

[49] Another experiment led by Lederman in the proton–proton collisions environment performed on the ISR collider at CERN missed the J/psi discovery because they chose the wrong trigger and had a problem with the background (L. Lederman in Hoddeson et al., 2008, 181). Lederman cites his urge to move on quickly on to higher energies as the reason for this failure (Lederman and Teresi, 2006, 316).

6. Conclusion

Lederman's attitude regarding automation is not unusual and can be very productive in confirmatory experiments. I am not claiming that the more generic automated detecting regime does not result in discoveries. But it is likely, given the history of the discoveries and misses, and the way it has affected the structure of the discovery process that it vastly increases the likelihood of missing anomalous details and thus precludes theoretical advances. The examples we have discussed demonstrate how an appropriate balanced detecting strategy can avoid potential experimental and theoretical impediments.

Thus crafting the detecting regime to the context of research rather than assimilating a cookie cutter regime might be advantageous in terms of the search for anomalies and theoretical alternatives. For instance, the symbiosis of the organizational structure and detecting regime that has worked well in the case of confirmatory experiments at CERN might impede the search for new particles at the Large Hadron Collider as they must be exploratory in nature due to the unknown mass range of the expected particles (Peach and Vick, 1994). Global exploratory searches (within a wide energy range) for anomalies in the Standard Model are under way (Nachtman, 2008; Cobal, 2006; H1 collaboration, 2004; D0 colaboration, 2001) but the effort seems meager in comparison to background-theory-driven searches and has a number of constraints that could be perhaps addressed by a different detecting regime. An indication that LHC and the detecting regime might not be well geared for searches for anomalies (nor are Tevatron and HERA in Germany on which these projects are currently performed) is the necessity of tying the background analysis in such searches more closely to the Standard Model as a trade-off for the relaxed dependence of the main signal (Nachtman, 2008, 431). Also, the LHC is apparently bound to miss a part of the spectrum that potentially contains Higgs boson as predicted by SUSY, a major alternative to the Standard Model (Cobal, 2006, 271) which is a good example of the machine being strongly background-theory dependent. If the difference in productivity between different detecting regimes is as profound as the history suggests, the search for potential anomalies becomes a pressing issue that requires a thorough overhaul of detection and organization of big HEP experiments.

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