Understanding the Standard Model, 
as a bridge to the discovery 
of new phenomena at the LHC

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Abstract

I discuss the basic elements of the process that will lead to the discovery of possible new phenomena at the LHC. We review the status of the tools available to model the Standard Model backgrounds, and the role that such tools can play in the discovery phase, and in the exploration of the features and parameters of such new phenomena.
1 Introduction

The Standard Model (SM) of fundamental interactions has by now been successfully tested over the past 30 years, validating its dynamics both in the gauge sector, and in the flavour structure, including a compelling confirmation of the source of the observed violation of parity (P) and combined charge and parity (CP) symmetries. The inability of the SM to account for established features of our universe, such as the presence of dark matter, the baryon asymmetry, and neutrino masses, are not considered as flaws of the SM, but as limitations of it, to be overcome by adding new elements, such as new interactions and new fundamental particles. With this perspective, the LHC is not expected to further test the SM, but to probe, and hopefully provide evidence for, the existence of such new phenomena. Our ability to predict what will be observed at the LHC is therefore not limited by fundamental issues related to left-over uncertainties about the SM dynamics, but by the difficulty of mastering the complex strong-interaction dynamics that underlies the description of the final states in proton-proton collisions.

Many years of experience at the Tevatron collider, at HERA, and at LEP, have led to an immense improvement of our understanding of this dynamics, and put us today in a solid position to reliably anticipate in quantitative terms the features of LHC final states. LEP, in addition to testing with great accuracy the electroweak interaction sector, has verified at the percent level the predictions of perturbative QCD, from the running of the strong coupling constant, to the description of the perturbative evolution of single quarks and gluons, down to the non-perturbative boundary where strong interactions take over and cause the confinement of partons into hadrons. The description of this transition, relying on the factorization theorem that allows to consistently separate the perturbative and non-perturbative phases, has been validated by the comparison with LEP data, allowing the phenomenological parameters introduced to model hadronization to be determined. The factorization theorem supports the use of these parameters for the description of the hadronization transition in other experimental environments. HERA has made it possible to probe with great accuracy the short-distance properties of the proton, with the measurement of its partonic content over a broad range of momentum fractions $x$. These inputs, from LEP and from HERA, beautifully merge into the tools that have been developed to describe proton-antiproton collisions at the Tevatron, where the agreement between theoretical predictions and data confirms that the key assumptions of the overall approach are robust. Basic quantities such as the production cross section of $W$ and $Z$ bosons, of jets up to the highest energies, and of top quarks, are predicted theoretically with an accuracy consistent with the known experimental and theoretical systematic uncertainties. This agreement was often reached after several iterations, in which both the data and the theory required improvements and reconsideration. See, for example, the long saga of the bottom-quark cross section [1], or the almost embarassing — for theorists — case of the production of high transverse momentum $J/\psi$s [2].

While the present status encourages us to feel confident about our ability to extrapolate to the LHC, the sometimes tortuous path that led to this success demands caution in assuming by default that we know all that is needed to accurately predict the properties of LHC final states. Furthermore, the huge event rates that will be possible at the LHC, offering greater sensitivity to small deviations, put stronger demands on the precision of the theoretical tools.
In this essay I discuss the implications of these considerations, in the light of some lessons from history, and I discuss the role that theoretical calculations should have in the process of discovering new physics. I shall not provide a systematic discussion of the state of the art in calculations and Monte Carlo tools (for these, see Refs. [3] and [4], respectively) but rather a personal perspective on aspects of the relation between theory and data analysis that are sometimes neglected. Furthermore, I shall only deal with what we would call “direct discovery”, namely the observation of the production of some new particle. Of course the LHC can discover new physics in other ways, for example by measuring $B_s \to \mu^+\mu^-$ decays with a rate different than predicted by the SM, or by measuring the top, $W$ and Higgs masses to be inconsistent with the SM expectation. I shall not cover these aspects, since they are subject to sources of theoretical and experimental uncertainties that are rather complementary to those I intend to focus on.

2 Signals of discovery

Three new elementary particles have been discovered by hadron colliders: the $W$ and $Z$ bosons [5,6], and the top quark [7,8]. For the first two, the features of the final states were known in advance with great confidence, and so were the masses and the production rates. The signals stood out of the backgrounds very sharply and cleanly, and their interpretation in terms of $W$ and $Z$ was straightforward. The discovery of the top was harder, but still benefited from the a-priori knowledge that the top had to be there somewhere, and of its production and decay properties.

It is likely that the search and discovery of the Higgs boson will follow a similar path. We have reasonable confidence that the Higgs has to be there, and we know how it would be produced and decayed, as a function of its mass and even as a function of the possible models alternative to the plain SM implementation of the Higgs mechanism. Search strategies have been set up to cover all expected alternatives, and in many cases a signal will be unmistakable: mass peaks such as those obtained from $H \to \gamma\gamma$, or $H \to ZZ \to 4$ leptons, are easily established as soon as the statistics is large enough to have them stand out of the continuum background, without any need to rely on theoretical modeling.

As we move away from the default Higgs scenarios, into the territory of new physics beyond the SM, life becomes more difficult. One should think of two phases for a discovery: establishing the deviation from the SM, and understanding what this deviation corresponds to. It is crucial to maintain these two phases separate. The fact that a given anomaly is consistent with one possible interpretation does not increase its significance as an indication of new physics. If we see something odd in a given final state, it is not by appealing to, or freshly concocting, a new physics model that gives rise to precisely this anomaly that makes the signal more likely or more credible. The process of discovery, namely the detection of a deviation from the SM by more than, say, 5 standard deviations of combined statistical and systematic uncertainties, should be based solely on the careful examination of whether indeed this signal violates the SM expectation. Assigning this discrepancy to a slot in the space of possible BSM scenarios is a subsequent step.

We can broadly group the possible deviations from the SM expectation into three, possibly overlapping, classes: mass peaks, shape discrepancies, and excesses in so-called counting
experiments.

2.1 Mass peaks

Whether in a dilepton, diphoton, or dijet final state, a two-body mass peak in the region of hundred GeV and above is the most robust signature one can hope for. Unless one sculpts the signal with a dangerous choice of selection cuts (like looking for a mass peak in the mass region just above the kinematical threshold set by twice the minimum energy of the reconstructed objects), this signal cannot be faked by a detector flaw. For example, things like malfunctioning calorimeter units occasionally giving a fixed signal corresponding to a high-energy deposition, will only fake a mass peak if all events have precisely the same two detector elements giving the signals for the two particles in the mass bin. Random failures by more calorimeter towers would give different two-body invariant masses, because of the different reconstructed kinematics, and would not build up a mass peak! On the other hand if it is always the same two calorimeter towers giving the signal, this is unlikely enough to be immediately spotted as a localized hardware problem rather than as a $Z'$.

On the theory side, no SM background can give rise to a sharp peak, since, unless you are sitting on top of a $W$ or $Z$, all sources give rise to a continuum spectrum: either from the obvious DY, or from the decay of separate objects (like $WW$ or $t\bar{t}$ pairs).

An experimental analysis would extract the background directly from the data, by studying the sidebands of the invariant mass distribution below and above the peak, and interpolating under it. The role of the simulation of the SM background is therefore marginal, and will only contribute, possibly, in helping the interpolation and establishing more accurately the background level for the experimental extraction of the signal excess. The simulation becomes then crucial in the second phase, that of the determination of the origin of the new signal, and of the study of its properties, as discussed later.

Mass peaks are just an example of a general set of self-calibrating signals, for which data themselves offer the most reliable source of background estimate. Other examples include jacobian peaks, or sharp edges in two-body mass distributions, like in the case of dileptons in supersymmetric chain decays of gauginos [9].

2.2 Anomalous shapes of kinematical distributions

Typical examples in this category are the inclusive transverse energy ($E_T$) spectrum of jets, or the missing transverse energy ($\not{E}_T$) distribution in some class of final states (e.g. multijet plus $E_T$, as expected in most supersymmetric scenarios). A precise knowledge of the SM background shapes is an obvious advantage in these cases. To which extent one can solely rely on such presumed knowledge, however, is a matter worth discussing. To help the discussion it is useful to consider a concrete example from the recent history of hadronic collisions, namely the high-$E_T$ jet spectrum measured in run 1 by CDF [10].

For years it has been claimed that a high tail in the jet $E_T$ spectrum is a possible signal of an anomalous quark form factor, due for example to the manifestation of quark compositeness. The comparison of CDF’s data with the best available theoretical predictions, based on next-to-leading-order (NLO) QCD and the latest parton density functions (PDFs) extracted from HERA, led to a several-$\sigma$ excess in the region $E_T \gtrsim 250$ GeV, compatible
Figure 1: Example of an expected supersymmetry signal and backgrounds in the multijet+$E_T$ final state [13].

with compositeness at a scale just above the TeV. The difference between theory and data was such that no appeal to yet higher order effects could have possibly fixed it. In that respect, the discrepancy would have already been visible using a plain leading-order (LO) calculation, since, aside from an overall $K$ factor, the shapes at LO and NLO were known to agree very well. On the side of PDFs, the same conclusion could be drawn from the analysis of the HERA data. With the lack of flexibility in modifying the behaviour of the partonic NLO cross section, and given the solid understanding of experimental systematics, the PDFs remained however the only possible scapegoat. As it turned out, the supposedly well-known large-$x$ behaviour of the gluon density was driven mostly by the assumed functional form used in the fits, rather than by data directly sensitive to it. Including the CDF data into the fits, led in fact to new parameterizations [12] that gave equally good descriptions of previous data, as well as explaining away the jet anomaly. The impasse was finally resolved by the subsequent analysis by D0 [11], which considered the $E_T$ spectrum of jets produced at large rapidity. The production of dijet pairs with a large longitudinal boost but of low invariant mass (therefore in a region free of possible new physics contamination) forces the momentum fraction of one of the incoming partons to be close to 1, thus probing the PDFs in the range relevant for the high-$E_T$, central, production. This measurement confirmed the newly proposed fits, and set the matter to rest.

The lesson for the future is that, more than accurate theoretical calculations, in these cases one primarily needs a strategy for an internal validation of the background estimate. If evidence for some new phenomenon entirely depends on the shape of some distribution, however accurate we think our theoretical inputs are, the conclusion that there is new physics
is so important that people will always correctly argue that perhaps there is something weird going on on the theory side, and more compelling evidence has to be given.

A place where we shall (hopefully!) encounter this problem at the LHC is the $E_T$ spectrum in multijet events, the classic signature of escaping neutralinos produced in the chain decay of pair-produced squarks and gluinos. A possible outcome of this measurement is given in Fig. 1 taken from recent ATLAS studies [13]. The variable $M_{\text{eff}}$ is defined as the sum of the transverse energies of all hard objects in the event (jets and $E_T$, in this case). Events are required, among other things, to have at least 4 jets with $E_T > 50 \text{GeV}$, of which one above 100 GeV, and $E_T > \text{max}(100 \text{GeV}, 0.2 \times M_{\text{eff}})$. The solid histogram is the expected signal, the shaded one is the sum of all backgrounds, including SM processes with real $E_T$ (such as jets produced in association with a $Z$ boson decaying to neutrinos), and SM processes where the $E_T$ results from the inaccurate measurement of some jet energies. The signal corresponds to production of squarks and gluinos with a mass of the order of 1 TeV. While the signal has certainly a statistical significance sufficient to claim a deviation from the SM, it is unsettling that its shape is so similar to that of the sum of the backgrounds. The theoretical estimates of these backgrounds have also increased significantly over the last few years, as a result of more accurate tools to describe multijet final states. There is no question, therefore, that unless each of the background components can be separately tested and validated, it will not be possible to draw conclusions from the mere comparison of data against the theory predictions.

I am not saying this because I do not believe in the goodness of our predictions. But because claiming that supersymmetry exists is far too important a conclusion to make it follow from the straight comparison against a Monte Carlo. One should not forget relevant examples from the colliders’ history [14, 15], such as the misinterpretation in terms of top or supersymmetry of final states recorded by UA1 with jets, $E_T$, and, in the case of top, leptons. Such complex final states were new experimental manifestations of higher-order QCD processes, a field of phenomenology that was just starting being explored quantitatively. It goes to the theorists’ credit to have at the time played devil’s advocate [16], and to have improved the SM predictions, to the point of proving that those signals were nothing but bread and butter $W$ or $Z$ plus multijet production. But the fact remains that claiming discoveries on the basis of a comparison against a MC is dangerous.

So let me briefly discuss the current status of theory predictions for the SM channels relevant for supersymmetry searches. There are three dominant processes: production of jets and a $Z$ boson, with $Z \rightarrow \nu \bar{\nu}$ giving the missing energy; production of jets and a $W$ boson, where this decays either to a $\tau \nu$ (the $\tau$ faking a jet), or to a $\mu \nu$ or $e \nu$, with the leptons escaping identification; and $t\bar{t}$ pairs, where one of the $W$s from the top decays behaves like in the previous case.

$t\bar{t}$ production has been well tested at the Tevatron [17, 18]. Theoretical NLO calculations, enhanced by the resummation of leading and subleading Sudakov logarithms [19], predict correctly the total cross section. The predictions for the LHC are expected to be equally accurate, if not more, since the main source of uncertainty, the PDFs, fall at the LHC in a range of $x$ values where they are known with precision better than at the Tevatron. The kinematical production properties, such as the transverse momentum distribution or the invariant mass of the $t\bar{t}$ pair, are also well described by theory, and Monte Carlo event generators are available to model the full structure of the final states, including both the full
set of NLO corrections [20] and the emission of multiple extra jets [21], which is relevant for the backgrounds to supersymmetry.

The processes $W+$jets and $Z+$jets are very similar from the point of view of QCD. There are minor differences related to the possibly different initial-state flavour compositions, but the main theoretical systematics, coming from the renormalization-scale sensitivity due to the lack of higher-order perturbative corrections, are strongly correlated. In the case of $W/Z+1$ and 2 jets, parton-level NLO calculations are available [22]. They are in excellent agreement with the measurements at the Tevatron [23,24], as shown for example in the case of $Z+1$ and 2 jets by the CDF results [23] shown in Fig. 2. Going to higher jet multiplicities, and generating a realistic representation of the fully hadronic final state, is then possible with LO calculations. Exact, LO matrix-element calculations of multiparton production can be enhanced by merging with shower Monte Carlo codes [25,26], which add the full perturbative gluon shower and eventual hadronization. An example of the quality of these predictions is given by Fig. 3, which shows the ratio of the measured [27] and predicted $W+$N-jet cross sections, for jets with $E_T > 25$ GeV. The theoretical predictions include the LO results from Ref. [28] (labeled as MLM), and from Ref. [29] (labeled as SMPR), while MCFM refers to the NLO predictions for the 1- and 2-jet rates from Ref. [22]. The systematic uncertainties of the individual calculations, mostly due to the choice of renormalization scale, are shown. The LO results, which have an absolute normalization for all $N$-jet values, are in good agreement with the data, up to an overall $K$ factor, of order 1.4. The prediction for the ratios of the $N$-jet and $(N−1)$-jet rates is also in good agreement with the data. The NLO calculations embody the $K$ factor, and exactly reproduce the 1- and 2-jet rates.

Thorough comparisons have been performed [30] among a set of independent calculations of $W$ plus multijet final states [28,31–34]. The results of the matrix element evaluation for
these complex processes are all in excellent agreement; differences in the predictions at the level of hadrons may instead arise from the use of different parton-shower approaches, and of different ways of sharing between matrix elements and shower the task of describing the radiation of hard jets. An example of the spread in the predictions is shown in Fig. 1, which shows the $E_T$ spectra of the four highest-$E_T$ jets in $W+$multijet events at the LHC. With the exception of the predictions from one of the codes, all results are within ±50% of each other, an accuracy sufficient by itself to establish possible deviations such as those in Fig. 1. These differences are of size compatible with the intrinsic uncertainties of the calculations, given for example by the size of the bands in Fig. 3. It is expected that they can be removed by tuning the input parameters, like the choice of renormalization scale, by fitting the data. An accurate determination of the normalization and shape of the SM background to a supersymmetric signal could therefore be obtained by analyzing data control samples. The description of the $(Z \to \nu \bar{\nu})+$ jets process can be validated, and the absolute normalization of the rate tuned, by measuring the signal-free $(Z \to e^+ e^-)+$ jets final states. This information can be then directly used to tune the $W+$jets predictions; or one can measure directly $(W \to e \nu)+$ jets in a region where the electron is clearly tagged, and use the resulting tune to extrapolate to the case of $\tau$ decays, or to decays where the $e$ and $\mu$ are not detected. The fact that the calculations appear to well reproduce the ratios of $\sigma[N-\text{jet}]/\sigma[(N-1)-\text{jet}]$, provides a further handle.

A clear path is therefore available to establish the accuracy of the theoretical tools, and to provide robust background estimates for searches of anomalies in the multijet plus $E_T$ final states. As always, however, the devil is in the details. As shown by the Tevatron analyses, even the measurement of the background $W+$multijet cross sections is not an easy task, due to a large contamination from $b\bar{b}$ backgrounds (where both $b$-hadrons decay semileptonically, one giving rise to a hard and isolated charged lepton, the other to a very energetic neutrino), and $t\bar{t}$ backgrounds, which at the LHC are the dominant source of $W+$multijet events. It is
therefore difficult to anticipate the dimension of the challenge, only the direct contact with to data will tell!

### 2.3 Counting experiments

A counting experiment is a measurement defined by assigning some selection criteria, counting the number of events passing the cuts, and comparing this against the expected background. Counting experiments are like searches for shape anomalies, since the analysis cuts act on the distribution of some variables. However the expected statistics are too small to allow a meaningful use of the full distributions, and one simply integrates over the full sample passing the cuts. So counting experiments tend to lack a smoking gun, a truly compelling evidence that something is wrong, and they require the most robust understanding of backgrounds one can possibly need.

A typical example is given by the analyses that led to the top discovery. Different selections were applied to single out complementary data samples, each characterized by at least one of the expected features of top final states:

- a charged lepton, with $E_T$, 3 or more jets, and possibly one of them containing either a lepton, or a secondary displaced vertex (SVX), expected features of $b$-hadron decays;

- a pair of opposite-charge leptons, with invariant mass away from the $Z$ peak is same flavour, and one or two jets, possibly with a $b$-quark tag.
Each of the objects listed above had to pass some kinematical or quality cuts, in terms of minimum \( p_T \) or \( E_T \), or in terms of variables defining the cleanness of the leptonic of SVX tags. The estimate of the backgrounds to a counting experiment is usually very hard. One can always suspect that, even if the background estimates are tested on control samples, the extrapolation in the signal region could fail. For example, backgrounds that are a negligible component of the control samples could sneak into the signal region, and suddenly dominate the rates. Furthermore, backgrounds could have strong correlations among different variables, and the probability of extrapolating their rate using their relative weight for various variables may not factorize into a product of probabilities. A typical example is the one given earlier for the contribution of \( b\bar{b} \) pairs to events with isolated electrons (or muons) and missing transverse energy. The contribution to isolated leptons is proportional to the probability of having a \( b\bar{b} \) final state, \( P_{bb} \), times the probability of a suitable decay to a charged lepton, \( P_{b\rightarrow\ell} \). The contribution to a large \( E_T \) signal is likewise given by \( P_{bb} \times P_{b\rightarrow\nu} \). But the probability of having both a hard charged lepton and the \( E_T \) is not the product of the two, but is given by \( P_{bb} \times P_{b\rightarrow\ell} \times P_{b\rightarrow\nu} \), which is larger than the product by \( \sim 1/P_{bb} \approx 100 \).

Analyses of this type require full Monte Carlo codes, where the best possible perturbative input (e.g. multijet matrix elements) is used together with a complete description of the shower, hadronization, particle decays, and underlying event. A good description of a \( b \)-hadron decay, based for example on an empirical fit to existing data, may have more value than the inclusion of first-principle NLO corrections to the matrix elements. The challenge for the theorist is to provide a prediction where the accuracy is uniformly distributed over all the delicate areas, rather than concentrated on some specific spots. The calculation must have enough tunable parameters that the predictions can be adjusted to fit the data in the control samples, but not too many that the extrapolation from the control samples to the signal regions may not be trustable. The crucial question that a theorist is called to answer for applications to counting experiments is, in fact, to which extent the predictions of the code tuned on some sample can be trusted when exported to another sample. When codes can be properly tuned, the portability issue becomes the dominant source of systematics. Sorting out all of these issues requires a very careful and skilled work, both on the experimental and theoretical side. This sort of explains why it took about 100 pages [7] to document the steps that led to a credible first evidence for the top quark!

The discovery of the Higgs in complex final states, such as the weak-boson fusion channels, with the Higgs decaying to final states without a sharp mass peak, such as \( H \rightarrow \tau^+\tau^- \) or \( H \rightarrow b\bar{b} \), and with vetoes on the presence of jets in the central rapidity region, will fall in this category of extremely difficult and hard-to-validate searches.

An interesting historical example in this class is the famous \( e^+e^-\gamma\gamma E_T \) event seen by CDF in run 1 [35]. The expected SM background for this event is less than \( 10^{-6} \) events. The estimate drops to \( 10^{-5} \) if one assumes that the most forward electron (for which the tracking information, and therefore a charge-tag, is missing) is actually a photon. According to the rules, this is a 5\( \sigma \) excess, and qualifies for a discovery. After all, even the \( \Omega^- \) discovery was based on just one, compelling, event. In terms of pure statistics, the \( e^+e^-\gamma\gamma E_T \) is (still today, after 30 times more luminosity has been collected by CDF and D0) even more significant as a deviation from the background (whether caused by physics or instrumental) than the first \( W \) observations at UA1 and UA2. Why do we not consider it as evidence of new physics? Because consensus built up in the community that, in spite of the “5\( \sigma \)”,
evidence is not so compelling. On one side plausible BSM interpretations have been ruled out [18] by the LEP experiments, which inconclusively explored, for example, scenarios based on gauge-mediated supersymmetry breaking, with $\tilde{e} \rightarrow e\gamma \rightarrow e\gamma \tilde{G}^{3/2}$. On the other, doubts will always remain that some freaky and irreproducible detector effect may be at the origin of this event. However difficult, the estimate of the physics SM background to this event at the leading-order is relatively straightforward and has been checked and validated. Higher-order effects, not known, cannot be reasonably expected to change the rates by more than a factor of two. I do not think that anyone can seriously argue that the knowledge of the background rates with a NLO accuracy would have changed our conclusions about this event, so I do not think that here theory could have played a more important role. As in other examples of the past (most frequently in the discovery of hadronic resonances, see e.g. the recent case of pentaquarks), theoretical bias (e.g. the availability and appeal of a theoretical framework — or lack thereof — within which to fit the claimed discovery), the possible prejudice towards the robustness of the analysis or of the group that performed it, and other considerations generically labeled as *guts’ feeling*, heavily interfere with the purely statistical and systematics assessment of a finding, making its interpretation more difficult. We find a similar situation in other areas of particle physics. The examples of neutrino oscillations and of the muon anomalous magnetic moment come to mind. Davies’ solar-neutrino anomaly had been sitting there for years, and no improvement in the solar model was ever going to be good enough for the reality of neutrino oscillations to be uniformly accepted by the community. New data, less sensitive to the details of theory, and providing the opportunity to test more convincingly the model assumptions beyond the shade of any doubt, had to come for Davis’ signal to be incorporated in a broader evidence for neutrino oscillations. It is likely that the $3.5\sigma$ of BNL’s $g_\mu - 2$ experiment [36] will have a similar fate, regardless of how much progress will be made in the theoretical understanding of the hadronic contribution to light-by-light scattering. QCD is simply too vicious for everyone to accept that this anomaly is a conclusive evidence of physics beyond the SM, let alone to commit to an interpretation such as supersymmetry.

### 3 Measuring parameters

A key element of the discovery programme at the LHC will be improving the accuracy of the SM parameters, and measuring, as precisely as possible, the parameters of the new physics that will hopefully be discovered. The relation between $m_{\text{top}}$, $m_W$, $\sin \theta_W$ and $M_H$ is an important prediction of the SM, and deviations from it should be accounted for by the effects of new physics. And in presence of new physics, the values of the new particles’ masses and couplings will be the starting point to reconstruct the new theory.

This is an area where the ability of theory to describe the final states is crucial. Couplings will be extracted from the determination of production cross sections, branching ratios, or angular distributions. Masses will mostly be obtained via direct kinematical reconstructions. In all cases, an accurate modeling of both the SM backgrounds, which contaminate and deform the signal distributions, and of the signals, will be required.

Cross sections are obtained by counting events. Since the analyses defining a given signal have always selection cuts, to go from event counts to a cross section one has to model
the acceptance and efficiency of those cuts. These depend on the details of the production process, something that only a theoretical calculation can provide. This implies that the calculations should not only provide a precise value of the total cross section of a given process, but also of the kinematical distributions that are used in the experimental analysis. For example, in the case of the $W$ or $Z$ cross section one needs the precise form of the $p_T$ and rapidity spectra of the decay leptons [37]. The problem with this is that typical higher-order calculations are more easily done at the total cross section level, to benefit from the full inclusivity of the final state and more easily enforce the cancellation of the divergencies that appear separately in the real and in the virtual corrections. A great amount of work has therefore been invested recently in developing techniques capable of delivering the same perturbative accuracy both at the level of total cross sections and at the level of distributions (for a review of recent developments in higher-order perturbative QCD calculations, see e.g. Ref. [3]). For example, the full next-to-next-to-leading-order (NNLO) calculation of the lepton distributions in $pp \rightarrow (W \rightarrow \ell\nu) + X$ was recently completed, in Ref. [38]. Their conclusion is that the inclusion of NNLO corrections is necessary to control the rates at the level of few percent. This is required, for example, for the extraction of the LHC absolute luminosity at a similar level of precision. Such an accurate knowledge of the luminosity is the prerequisite for the precise measurement of all other cross sections, including those of interesting new processes. More recently, even the calculation of rates for some Higgs final states has reached a full NNLO precision for realistic leptonic observables [39].

Purely leptonic observables, where the leptons arise from the decay of non-strongly interacting particles, make it possible to fully integrate over the strongly interacting components of the events and, experimentally, enjoy a reduced dependence on the full hadronic structure. Under these circumstances, the use of parton-level calculations for realistic studies is legitimate (see e.g. Ref. [40] for a discussion of $pp \rightarrow H \rightarrow WW \rightarrow \ell\ell\nu\nu$).

Precision measurements of observables directly sensitive to the hadronic component of the events are typically more demanding. A good example of the difficulties that are encountered in these cases is given by the measurement of the top quark mass. In hadronic collisions the top quark mass can only be measured by reconstructing, directly or indirectly, the total invariant mass of its decay products. Due to the large phase-space available, top quark pairs are always produced well above their kinematical threshold. One cannot therefore use techniques such as those available in $e^+e^-$ collisions, where the mass of a new particle can be deduced from an energy scan at the production threshold. Furthermore, contrary again to $e^+e^-$ collisions where a top-quark pair at threshold is produced without any other object, in the $pp$ case the top pair is always accompanied by both the fragments of the colliding protons, and by the multitude of hadrons that are radiated off as the incoming quarks or gluons that will fuse into $t\bar{t}$ approach each other (initial-state radiation). It is therefore impossible to exactly decide which ones among the many particles floating around originate from the top decays, and have to be included in the determination of the top invariant mass, and which ones do not. As an additional obstacle, the top quark is coloured, it decays before hadronizing, but the detected decay products must be colour-singlet hadrons. This implies that at some stage during the evolution of the quarks and gluons from the top decay they will have to pair up with at least one antiquark drawn from the rest of the event, to ensure the overall colour neutrality of the final state. There is therefore no way, as a matter of principle, that we can exactly measure on an event-by-event basis the top quark mass.
The best that we can do is to model the overall production and decay processes, and to parameterize a set of determined observables as a function of the input top quark mass. For example, such an observable could be the invariant mass of three jets, assuming one of them comes from the evolution of the $b$ quark, and the other two from the decay of the $W$. This modeling cannot be achieved with parton-level tools, regardless of their perturbative accuracy. A full description of the final state is required, including the non-perturbative modeling of both the fate of the proton fragments and of the transition turning partons into colour-singlet hadrons. With the current level of experimental uncertainties [41] on $m_{\text{top}}$, at the 2 GeV level, we are approaching the level where the theoretical modeling [42] is not validated by a direct comparison with data. At the LHC, where the experimental uncertainties could be reduced below the 1 GeV level, theory will be the dominant source of systematics. Observables will have to be identified that will allow a validation and tuning of this systematics, in the same way that analogous problems had to be addressed for the determination of the $W$ mass at LEP. This is an area where the Tevatron statistics are too small to allow any progress, and all the work will be left to the LHC. Needless to say, all of this work will benefit the precise measurement of the masses of possible new particles decaying to quarks and gluons.

4 Conclusions

Advanced MC tools for the description of the SM, and for the isolation of possible new physics at the LHC, are becoming mature. Validation and tuning efforts are underway at the Tevatron, and show that a solid level of understanding of even the most complex manifestations of the SM are well under control. The extrapolation of these tools to the energy regime of the LHC is expected to be reliable, at least in the domain of expected discoveries, where the energies of individual objects (leptons, jets, missing energy) are of order 100 GeV and more. However, the consequences of interpreting possible discrepancies as new physics are too important for us to blindly rely on our faith in the goodness of the available tools. An extensive and coherent campaign of MC testing, validation and tuning at the LHC will therefore be required. Its precise definition will probably happen only once the data are available, and the first comparisons will give us an idea of how far off we are and which areas require closer scrutiny.

Ultimately the burden, and the merit, of a discovery should and will only rest on the experiments themselves! The data will provide the theorists guidance for the improvement of the tools, and the analysis strategies will define the sets of control samples that can be used to prepare the appropriate and reliable use of the theoretical predictions.

Aside from the discovery of anticipated objects like the $W$, $Z$ and the top, we have never faced with high-energy colliders the concrete situation of a discovery of something beyond the expected. In this respect, we are approaching what the LHC has in store for us without a true experience of discovering the yet unknown, and we should therefore proceed with great caution. All apparent instances of deviations from the SM emerged so far in hadronic or leptonic high-energy collisions have eventually been sorted out, thanks to intense tests, checks, and reevaluations of the experimental and theoretical systematics. This shows that the control mechanisms set in place by the commonly established practice are very robust.
Occasionally, this conservative approach has delayed in some areas of particle physics the acceptance of true discoveries, as in the case of Davies’s neutrino mixing, and as might turn out to be the case for the muon anomaly. But it has never stopped the progress of the field, on the contrary, it has encouraged new experimental approaches, and has pushed theoretical physics to further improve its tools.

The interplay between excellent experimental tools, endowed with the necessary redundancy required to cross-check odd findings between different experiments and different observables, and a hard-working theoretical community, closely interacting with the experiments to improve the modeling of complex phenomena, have provided one of the best examples in science of responsible and professional modus operandi. In spite of all the difficult challenges that the LHC will pose, there is no doubt in my mind that this articulated framework of enquiry into the yet unknown mysteries of nature will continue providing compelling and robust results.

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