Reporting Results in High Energy Physics Papers: a Manifesto

PIETRO VISCHIA

Institut de recherche en Mathématique et Physique,
Université catholique de Louvain

pietro.vischia@cern.ch

April 29, 2019

Abstract

The complexity of collider data analyses has dramatically increased from early colliders to the LHC. Reconstruction of physics objects has reached a point that requires dedicated papers documenting the techniques, and periodic retuning of the algorithms themselves. Analysis methods evolved to account for the increased complexity of the final states sought and for the need of squeezing the last bit of sensitivity from the data; they often involve a full final state reconstruction—mostly relatively easy at lepton colliders, sometimes exceedingly difficult at hadron colliders—or the use of advanced statistical techniques such as statistical learning.

The need of keeping the papers documenting results to a reasonable size implies nowadays a greater level of compression or even omission of information with respect to papers from twenty years ago. The need for compression should however not prevent sharing a reasonable amount of information that is essential to understanding a given analysis. Infrastructures like RIVET or HepData have been developed to host additional material, but the amount of material which is sent to these databases is still often insufficient.

In this Letter I advocate for an increase in the information shared by the Collaborations, and try to define a minimum standard for acceptable level of information when reporting statistical procedures in High Energy Physics papers.

1. INTRODUCTION

Analyses of collider data have become increasingly more complex all the way from early colliders to the CERN LHC. Early lepton colliders like LEP yielded very clean final states, which favoured simple reconstruction and analysis methods; such results were easily reported in detail even in brief papers. The advent of hadron colliders such as the Fermilab Tevatron introduced a new level of complexity; the proton PDFs force analyses to mostly work in the transverse plane, often replacing a full event reconstruction with the reconstruction of transverse quantities—final states can be fully reconstructed easily only when the missing transverse energy can be assigned to a single neutrino. Final states at hadron colliders are also characterized by additional “pileup” interactions on top of the hard scattering, resulting in dramatically higher hadronic activity in the event. Theoretical advancements also play a big role in both limiting the sensitivity of the results and increasing the complexity of the analyses; in order to tackle these issues it is crucial to have a full understanding of the uncertainties associated to the results, particularly the theory ones.

This Letter is inspired by a detailed comparison of the latest ATLAS and CMS multi-boson (W, Z) measurements in proton-proton collisions at the LHC with centre-of-mass en-
ergy of 13 TeV; an overview of the measurements themselves is beyond the scope of this Letter and can be found in Ref. [1]; here I instead make an attempt at abstracting some general recommendations on executing and reporting (mostly) statistical procedures, that I believe can help in comparing results within and across experiments and with theoretical predictions.

Following the evolution of colliders, analyses of their data had to adapt. The reconstruction of physics objects became more complex because of the increase in the tracker and calorimeter occupancies. The analysis methods also evolved to account for the increased complexity of the final states, resulting for example in more complex assignment problems—e.g. deciding which leptons might come from the decay of a Z boson and which one from the decay of a W boson in WZ multilepton decays—and for the need for squeezing the last bit of sensitivity from the data. This often involves the full reconstruction of the final state, a problem that was mostly relatively easy at lepton colliders and is now sometimes exceedingly difficult at hadron colliders, or the use of advanced statistical techniques such as statistical learning.

The need for keeping publications at a reasonable page count implies nowadays a greater level of compression or even omission of information with respect to papers from twenty years ago. The need for compression should however not prevent sharing a reasonable amount of information that is essential to understanding a given analysis.

The amount of information that is normally published is sometimes lacking. I find a similar situation in literature documenting conventions relative to individual collaborations or joint agreements. A BaBar document on recommended statistical procedures [2] advocates for example quoting only total uncertainties rather than uncertainties split by source, which I think is an outdated position. The note documenting the joint ATLAS and CMS procedures for producing results related to the Higgs boson search [3] (since then used for all Higgs physics results) defines, in the section dedicated to reporting results, only a very minimal set of information and leave to the individual Collaborations any decision on eventually extending this set.

In this Letter I advocate for an increase in the information shared by the Collaborations, and try to define a minimum standard for acceptable level of information when reporting statistical procedures in High Energy Physics papers. I will detail in an extended version of this Letter, for each of the points I raise below, some practical examples of desirable and less desirable practices, after hopefully having received feedback from the HEP community.

2. Datasets and simulation: compare apples with apples

The physics program of ATLAS and CMS is large, encompassing both standard model (SM) and beyond standard model (BSM) physics; analyses might be broadly divided into SM precision measurements (which nowadays have reached a precision that enables probing even BSM scenarios, mostly by measuring the couplings between SM particles) and traditional searches for new physics (SUSY or the like). While ATLAS and CMS clearly try to analyze the same datasets roughly at the same time, matching results are often made public with significant discrepancies. It is not uncommon that the latest results on a given topic are derived with very different integrated luminosities. An extreme example is WW production, where ATLAS focused on an inclusive measurement [4] performed with about 3 fb$^{-1}$; CMS has instead focused in making public the evidence for WW production in double-parton-scattering [5] with 77 fb$^{-1}$.

Unpredictable delays in the publication of an analysis might influence this picture, and each Collaboration might have different priorities. While I advocate a better synchronization on the temporal placement of analyses on similar data sets, it’s also true that some degree of temporal displacement might inform
the development of any later batch of analyses, until hopefully the final word with any given dataset is given by both Collaborations at around the same time; I think care should be taken in not having a too large displacement—cases like the one outlined above seem to me rather excessive.

Simulated samples are sometimes different; for the latest ZZ cross section results ATLAS [6] uses SHERPA [7,8] for the nominal signal templates and POWHEG [9,13] for alternative templates used for systematics, whereas CMS [14] uses POWHEG for the nominal signal templates. Signal modelling is nowadays crucial for many precision measurements; different conventions might result in serious issues in comparing results, and might become serious show-stoppers for ATLAS+CMS grand combinations.

One reason that partly explains this situation is that each collaboration’s framework for event simulation implies different challenges in integrating a given generator—at a given point in time for some physics process a Collaboration might be limited to only a subset of the available generators—but when the same generator is available to both Collaborations an effort should be done for uniformity. In case there are specific reasons for choosing one generator rather than the other as nominal prediction, one would expect that an agreement can easily been reached; if there is no particular reason for preferring one over the other, there should be no issue in agreeing either.

An argument against using the same generators might be to have the two Collaborations provide independent cross-checks of results, but I think that this would result in convoluting together any discrepancy caused by physics with discrepancies caused by Monte Carlo modelling; using the same generators decouples such effects, and leave always open the option of doing all the cross-checks in each Collaboration with all the generators available—either by trying multiple generators for nominal predictions or by detailed systematic studies. Non-trivial differences in the Monte Carlo production of the two experiments might also be explored by starting from the same set of hard scattering events rather than from the same generator cards.

3. Display what you use, and choose the same visualizations

Plotting distributions is mostly a visual aid to display the agreement between data and predictions and increase the confidence in the modelling of the relevant physics processes. Oftentimes plot also are input to the statistical procedures applied to extract estimates for the parameter of interest (POI); in this case, it is crucial that the binning used in the published plot reflect the one used when building the statistical model; sometimes one would be tempted to use a finer binning in distributions, to show the extent to which the distributions are sufficiently modelled, and later rebin to feed the distribution to the statistical model (typically to avoid unrealistic sensitivity driven by statistical fluctuations), but this might mislead the reader into thinking that whatever empty bin or problematic tail fluctuation visible in the finely binned plot is also affecting the final result. Without space constraints it would be possible to publish both binnings, but this is practically never feasible; it is therefore best to display in the plots exactly what goes into the statistical model.

Uncertainty bars and bands should likewise be reported clearly. For data points, Poisson uncertainties à-la-Garwood [15] are the best choice, providing correct-coverage intervals even in case of low counts or even empty bin contents—the Poisson uncertainty bar should be plotted also for empty bins. It is equally important to be explicit and consistent on the content of the error bars on the predictions; Ref. [16] reports in all figures a hatched “Uncertainty”, and it is only by reading the caption that the reader figures out that for half of the figures the uncertainty is statistical only and for the other half it is total (statistical plus systematic). While some uniformity is desirable, I don’t think it is a big deal to mix
plots with different definitions of uncertainty bands. What is crucial—particularly for propagation of the result at conferences—is that different definitions of bands have a different pictorial representation in terms of shaded area style and that the caption is explicit (i.e. “Stat. unc.” and “Total unc.” instead of just “Uncertainty”); a virtuous example can be seen in the plots of Ref. [17].

The choice of the physics observable to be reported is sometimes incoherent across collaborations and even within the same collaboration. The charge-dependent WZ cross section is reported by ATLAS as the ratio \( \sigma(W^+Z)/\sigma(W^-Z) \) in Ref. [18] and by CMS as the ratio \( A_{WZ,obs}^\pm/A_{WZ,NLO}^\pm \) of the observed ratio \( A_{WZ,obs}^\pm = \sigma(W^+Z)/\sigma(W^-Z) \) to the corresponding NLO expected ratio \( A_{WZ,NLO}^\pm \) in Ref. [17]. Results on anomalous couplings in Effective Field Theory (EFT) can be computed and reported in different basis: while it is mostly possible to convert results to a different parameterization, and therefore in principle any theoretician can convert experimental results to another basis, it is preferable and less error-prone if the experimental result already comes expressed in different parameterization; papers like Ref. [6] go a long way towards the ideal situation.

This kind of issues can in my opinion be easily solved by good bibliographic work when preparing papers for publication, complemented by cross-collaboration agreements in the context of summary plots preparation like the organized efforts of Ref. [19].

4. Phase space: why is it even different?

The extrapolation of fiducial cross section measurements to the full phase space depends crucially on the definition of the fiducial region. When comparing confidence intervals on EFT parameters with theory predictions, as e.g. SFitter does [20, 21], it is likewise necessary to have access to the exact definition of the fiducial region used to compute the appropriate confidence intervals.

ATLAS and CMS most of the times use—for a given analysis—different definitions of the phase space. Some are dictated by the different characteristics of the detectors, but some other could be uniformized across the experiments. In the latest WZ measurement ATLAS selects a Z boson mass interval of 66–116 GeV [18], whereas the corresponding interval for CMS is 60–120 GeV [17]; there is a feedback loop in which theoretical predictions are forced to use two different definitions coming from the experiments [22], and the experiments end up perpetuating the discrepancy by publishing new results using a phase space corresponding to the available predictions.

Any improvement in this direction would simplify the work of theoreticians, both in producing and comparing predictions for the different phase spaces and in reproducing the fiducial cuts when importing experimental results into global fits. Some sub-communities exhibit already a quite high degree of synchronization in terms of phase space, but for others more effort is needed. Ref. [4] sets an exemplary standard for reporting phase space definitions.

An especially good development is represented by the current push to publish the details of phase space definitions as a RIVET [23] routine. CERN Yellow Reports like the ones produced by Ref. [24] are another obvious place to document and cite such agreements—provided that later analyses apply these recommendations.

5. Uncertainties: show the dirty details

In experimental physics, the number quoted as the result of a measurement is meaningless without an uncertainty attached to it.

In HEP it is nowadays important to report not only the total uncertainty affecting a given measurement, but also the individual components that build up the total uncertainty. This gives hints for further studies—which parts of an analysis could be improved with profit—
and highlights the status of particular topics, included but not limited to the understanding of the current theoretical models.

Given a set of results it is unfortunately difficult to find consistent reporting of systematic uncertainties. More often than not, for at least some results the reader is left with the overall uncertainty on the final result, and no information about the individual impact of each source of uncertainty on the final result. This can be problematic when trying to understand how much a measurement is limited by, say, a peculiar theory uncertainty rather than another. Sometimes results are given quoting separately some “relevant” sources of uncertainty (e.g., splitting by experimental/scale/PDF/other).

We are in the era of profile likelihood ratios, where computing the fiducial cross section by applying the naïve formula

\[
\sigma = \frac{N_{\text{data}} - N_{\text{bkg}}}{\epsilon L}
\]

(and the corresponding formula for the total cross section) is becoming a rare occurrence; this implies that uncertainties—modelled as nuisance parameters—assume now an unprecedented importance. There is some residual discrepancy in the extraction of uncertainties; symmetric uncertainties on fiducial cross sections for WZ production are computed by propagation from Eq. 1 and combined with HERA-era methods by the ATLAS Collaboration [18], whereas the CMS Collaboration opted in Ref. [17] for asymmetric intervals from a profile likelihood fit in each channel, and a simultaneous fit to the four channels to obtain the combined result.

Some results estimate simultaneously the “main” observable and a parameter representing a given source of uncertainty, as in Ref. [25] where the top quark mass and the jet energy scale are measured simultaneously. Even when the analysis is not sensitive enough to measure an uncertainty as POI, the parameter can still be constrained while profiled in the fit, indirectly giving information—when appropriate—about the amount of over- or under-estimation that went into the determination of its prefit value. This in turn can inform theoretical development in the modelling of some sources of uncertainty (PDF, \(\alpha_s\), ISR/FSR, heavy flavours).

It is therefore crucial to report in detail both the prefit and the postfit values of the uncertainties, split by uncertainty source. Many results tend to quote only the total uncertainty on the final result; this is understandable historically, because the uncertainties in the efficiencies \(\epsilon\) of Eq. 1 are often propagated in bulk to the final measurement—unless the combination of the results with methods like BLUE [26–28] is performed.

I think that while projects like HepData [29] already facilitate sharing information, a strong push is still needed across the Collaborations to publish a minimal set of information. The prefit uncertainties should be always quoted in the paper text, together with explicit mention to the way they were computed. Postfit uncertainties should be similarly always quoted, I suggest in a tabular way, and should be split into as many groups of independent components as it makes sense to quote separately, as is done in Ref. [17]; this is crucial for understanding the intimate characteristics of a result, and provides hints for future developments. Although I see quite some resistance to this concept, I think the best way of reporting postfit uncertainties is by quoting also the pulls and constraints of the nuisance parameters in the statistical model, rather than only quoting the impact of the uncertainties on the POI; any worry that reporting the pulls would make the result less solid should be shunned. On the contrary, a result is more solid if it is accompanied by a complete analysis of the uncertainties that affect it.

Sometimes the postfit uncertainties are expressed in terms of their effect on the yields rather than on the POI [6]: while it’s interesting to see a table of the postfit yields and their total uncertainty, in the era of differential measurements and measurements of the couplings I find more useful to quote the impact on the POI rather than on the yields.
Finally, the individual pulls and impacts, while useful, do not represent the full picture because they don’t inform about the correlation among the various sources of uncertainty; I therefore advocate for the publication of the full postfit covariance matrix, split by uncertainty sources.

A simplified-likelihood approach described in Ref. [30] proposes instead to collapse all systematic uncertainties into individual per-search-region variations of the yields and to write a simplified likelihood that accounts for the correlations of the yield variation across the regions; it then proceeds to merge search regions into aggregated regions in order to simplify reinterpretations. While this approach maintains the information on the correlation between regions (although loosing some of it when aggregating regions), it gains in computation efficiency and in simplicity of the published modelling at the price of loosing all information relative to the individual contribution of each source of uncertainty to the final measurement. This is certainly a better approach with respect to not publishing even the correlation of the yields among search regions; it also provides a simple way for third parties to test the sensitivity of new models without having to simulate the full detector, having therefore a high practical value. These simplifications can however be made starting from the full published covariance matrix with all the separated sources of uncertainty, and I therefore regard this method as a practical addendum to the disclosure of the full information.

6. Machine Learning: don’t let it be a black box

The proliferation of the use of statistical learning (usually called machine learning) in HEP and its successes have resulted in a desensitization of the community to the internal details of it.

While this is a testimony to the trust the community puts into methods that 10–15 years ago were looked at with suspicion by many people, on the other side this implies that entire parts of the analyses are described succinctly with an utter lack of information. In case of deep artificial neural network (“deep learning”), the relative novelty makes so that a certain level of detail is still given, but in case of simpler classifiers like boosted decision trees (BDTs) the level of detail nowadays does not go beyond “we have applied a dedicated BDT to solve this problem”. Even analyses in which BDT classifiers are crucial to reach a given sensitivity threshold—e.g. Ref. [16]—sometimes contain no information neither on the gain given by using the classifiers nor on the details of the structure and methods used to train them.

I propose that at the very minimum the following information is provided in the paper text for each classifier used in the analysis: details on the training samples (different generators for training and validation? Same generator, splitting available events?), details on the training methodology (how are the training, validation, and application samples defined? Is there any possible effect due to an extrapolation from the training region to the application region, or are they the same?). The way the relevant hyperparameters of the classifier have been chosen should be definitely quoted. All of this is interesting information per-se and can (and should!) also inform further needs for future improvements of such methods. Validation of the classifiers—although possibly only for classifiers used for later statistical inferences—should also be highlighted; the agreement between data and simulation should be studied in control regions and shown in the signal region.

Publishing the trained model or set of training weights can probably be counterproductive, as a legitimate worry is that classifiers trained at detector level might be improperly used at particle level or with some different simplified detector simulations. This is a point that should be somehow discussed, however, particularly in an era in which any statistical learning development is published together with any possible detail; would a dis-
Unfolding is an inverse problem that sometimes should not be solved. In HEP there are however compelling reasons for unfolding spectra, mostly gravitating around concerns about the future consumption of the results. The most commonly used unfolding methods include some way of regularizing the result. Regularization is essentially an artificial constraint introduced to fix high-frequency fluctuations induced by the structure of the response matrix; while keeping variance under control, it introduces a bias towards the simulated distribution. This bias can be studied, but regularization should nevertheless be used with care and only when necessary. For many cases in which the response matrix is reasonably diagonal, regularization does not improve the measurement.

Likelihood-fit-based unfolding embeds regularization as an explicit Tikhonov term, and the value of the regularization strength is chosen by scanning some quantity sometimes related to a $\chi^2$. Other algorithms are based on iterating to convergence and regularization is introduced in the form of early-stopping, as for example the D’Agostini method [31]—although its improved version [32] supposedly does not necessarily involve any iteration thanks to the possibility of tweaking the prior for the method. In any case, iterative methods converge in a number of steps that depend on the problem [33]: it is therefore important to mention both the value used for the parameter describing the number of iterations, and most crucially the stopping criterion used to determine that parameter. It is not infrequent in HEP literature that the software default (4, for the standard D’Agostini implementations) is used, but that value does not necessarily represent the best choice for the problem at hand; sometimes more iterations are needed to achieve convergence, sometimes even less.

For some algorithms, increasing too much the number of iterations can even worsen the result [33]. Not choosing correctly the number of iterations can induce unwanted and non-studied regularization biases, and failing to report the number of iterations and the method used might lead to doubting the result itself.

Agreeing on the details of the procedures is an open topic, discussed often in the ATLAS and CMS statistics communities; mostly we have prescriptions on what to not do rather than unique recommended ways of doing something. Leaving aside the open questions on the procedure itself, when it comes to reporting on the procedure we should nevertheless come together as a community on a minimal set of details to be reported for any unfolding measurement.

We should first of all always publish the response matrix and the covariance matrices. It’s true that in many cases the response matrix is available as supplementary material in HepData, but while this is a step in the good direction I think that both response and covariance matrices should have their space in the paper body itself.

Results should be published not only in plotted form, but also in tabulated form; the recent push of the Collaborations to fill the HepData entries for each new result is definitely the symptom of a helpful attitude towards the theory community.

I finally think that the minimal amount of information to be published about any unfolding procedure should include also a description of the settings used for any given method; the choice of introducing a regularization parameter should be first of all justified—regularization should be used as originally intended as a way of dealing with difficult cases in which the usual method does not yield meaningful results. In case regularization is used, then both the value of the regularization parameter and the method used to define such value should definitely be quoted, regardless of the unfolding method or software used.

While most of this information is (or should be) already available to the analyzers and
could therefore be dumped into the main paper quite easily, some can result in a significant amount of additional work, particularly for papers with tens of unfolded distributions; in cases where the theory community is adamant about the need of using such information, Collaborations already tend to provide this material—e.g. for Ref. [34] whose HepData entry includes the response and covariance matrices to be used by theoreticians for PDF computations. When the use case is not already explicit or clear, though, the Collaborations tend to not see the value gained by the additional effort; the theory community can therefore probably help by proposing concrete ways of making use of any additional information the experiments could provide.

8. Discussion

Tools for easy dissemination of useful information are already in place; for information that would be too large or requiring a format unsuitable for the paper text, tools like RIVET and HepData provide handy ways of sharing most if not all the information discussed above. There are recent talks of defining an Analysis Description Language for LHC analyses [35], which could improve the situation even more, defining an universal way of sharing information that would be compatible out of the box for the purpose of later studies.

The level of shared information—even in presence of these tools—is still often insufficient; I tried in this Letter to outline the most striking issues that I observed in my daily activities, without pretending to provide a complete list. I hope this can spark a deeper discussion about such issues and a higher level of sharing information for the HEP results to come.

9. Summary

I have outlined a number of common issues in the reporting of high energy physics results, mostly focussing on statistical issues.

Taking inspiration from examples from multiboson measurements at ATLAS and CMS, I have abstracted a certainly non-exhaustive list of the minimal details that should be quoted for each procedure, outlining that some tools are already readily available and some others are in course of development. The choice about how much information to share resides ultimately within the Collaboration themselves; I identified a few areas in which improvement might be substantial, with the hope that this abstraction might be a guide for sparking a discussion about reporting results of HEP analyses at the LHC and beyond.

I foresee preparing an extended version of this Letter, documenting the details of desirable and less desirable practices, after hopefully having received substantial feedback from the community.

Acknowledgements

I wish to thank the ATLAS and CMS Collaborations for being an endless source of inspiration, the CMS Collaboration for having me among its members since exactly 10 years, and the SM@LHC 2019 folks: the organizers for hosting the talk that inspired this Letter, and all the participants for the stimulating discussions that helped me refine the ideas behind this Letter.

A special thanks goes to Pedro Silva and Tommaso Dorigo for their useful comments on an earlier draft of this document, to Fabio Cossutti for a nice discussion on reporting unfolding results, and to Javier Cuevas for the endless discussions and encouragement that helped me shape my own critical views on a broad selection of topics.

References

[1] P. Vischia, Multiboson production with W and Z bosons at the ATLAS and CMS detectors. Talk at SM@LHC 2019, Zürich.
[2] R. Barlow et al., Recommended Statistical Procedures for BABAR. BABAR Analysis Document n. 318.

[3] The ATLAS Collaboration, The CMS Collaboration, The LHCG Higgs Combination Group collaboration, Procedure for the LHCG Higgs boson search combination in Summer 2011, Tech. Rep. CMS-NOTE-2011-005. ATL-PHYS-PUB-2011-11, CERN, Geneva, Aug. 2011.

[4] ATLAS collaboration, M. Aaboud et al., Measurement of the $W^+W^-$ production cross section in pp collisions at a centre-of-mass energy of $\sqrt{s} = 13$ TeV with the ATLAS experiment, Phys. Lett. B773 (2017) 354 [1702.04519].

[5] CMS Collaboration collaboration, Evidence for WW production from double-parton interactions in proton-proton collisions at $\sqrt{s} = 13$ TeV, Tech. Rep. CMS-PAS-SMP-18-015, CERN, Geneva, 2019.

[6] ATLAS collaboration, M. Aaboud et al., $ZZ \rightarrow \ell^+\ell^-\ell'^+\ell'^-$ cross-section measurements and search for anomalous triple gauge couplings in 13 TeV pp collisions with the ATLAS detector, Phys. Rev. D97 (2018) 032005 [1709.07703].

[7] T. Gleisberg, S. Hoeche, F. Krauss, M. Schonherr, S. Schumann, F. Siegert et al., Event generation with SHERPA 1.1, JHEP 02 (2009) 007 [0811.4622].

[8] M. Schonherr and F. Krauss, Soft Photon Radiation in Particle Decays in SHERPA, JHEP 12 (2008) 018 [0810.5071].

[9] P. Nason, A New method for combining NLO QCD with shower Monte Carlo algorithms, JHEP 11 (2004) 040 [hep-ph/0409146].

[10] S. Frixione, P. Nason and C. Oleari, Matching NLO QCD computations with Parton Shower simulations: the POWHEG method, JHEP 11 (2007) 070 [0709.2092].

[11] S. Alioli, P. Nason, C. Oleari and E. Re, A general framework for implementing NLO calculations in shower Monte Carlo programs: the POWHEG BOX, JHEP 06 (2010) 043 [1002.2581].

[12] T. Melia, P. Nason, R. Rontsch and G. Zanderighi, $W^+W^-$, $WZ$ and ZZ production in the POWHEG BOX, JHEP 11 (2011) 078 [1107.5051].

[13] P. Nason and G. Zanderighi, $W^+W^-$, $WZ$ and ZZ production in the POWHEG-BOX-V2, Eur. Phys. J. C74 (2014) 2702 [1311.1365].

[14] CMS Collaboration collaboration, Measurement of the pp $\rightarrow$ ZZ production cross section at $\sqrt{s} = 13$ TeV with the Run 2 data set, Tech. Rep. CMS-PAS-SMP-19-001, CERN, Geneva, 2019.

[15] F. Garwood, (i) Fiducial Limits for the Poisson Distribution, Biometrika 28 (1936) 437.

[16] ATLAS collaboration, G. Aad et al., Evidence for the production of three massive vector bosons with the ATLAS detector, 1903.10415.

[17] The CMS collaboration, Measurements of the pp $\rightarrow$ wZ inclusive and differential production cross sections and constraints on charged anomalous triple gauge couplings at $\sqrt{s} = 13$ tev, Journal of High Energy Physics 2019 (2019) 122.

[18] ATLAS collaboration, M. Aaboud et al., Measurement of $W^\pm Z$ production cross sections and gauge boson polarisation in pp collisions at $\sqrt{s} = 13$ TeV with the ATLAS detector, 1902.05759.

[19] Representatives of the LHC experimental and theory communities, LHC Top Physics Working Group.
[20] A. Biekötter, T. Corbett and T. Plehn, *The Gauge-Higgs Legacy of the LHC Run II*, 1812.07587.

[21] A. Biekötter, D. Gonçalves, T. Plehn, M. Takeuchi and D. Zerwas, *The global Higgs picture at 27 TeV*, SciPost Phys. 6 (2019) 024 [1811.08401].

[22] M. Grazzini, S. Kallweit, D. Rathlev and M. Wiesemann, *W±Z production at the LHC: fiducial cross sections and distributions in NNLO QCD*, JHEP 05 (2017) 139 [1703.09065].

[23] A. Buckley, J. Butterworth, L. Lonnblad, D. Grellscheid, H. Hoeth, J. Monk et al., *Rivet user manual*, Comput. Phys. Commun. 184 (2013) 2803 [1003.0694].

[24] Representatives of the LHC experimental and theory communities, *LHC Electroweak Working Group*.

[25] CMS collaboration, S. Chatrchyan et al., *Measurement of the top-quark mass in t¯t events with lepton+jets final states in pp collisions at √s = 7 TeV*, JHEP 12 (2012) 105 [1209.2319].

[26] A. C. Aitken, *Iv.—on least squares and linear combination of observations*, Proc. of the Royal Soc. Ed. 55 (1936) 42–48.

[27] L. Lyons, D. Gibaut and P. Clifford, *How to combine correlated estimates of a single physical quantity*, NIM A 270 (1988) 110.

[28] L. Lista, *Combination of measurements and the BLUE method*, EPJ Web Conf. 137 (2017) 11006 [1611.01927].

[29] E. Maguire, L. Heinrich and G. Watt, *HEPData: a repository for high energy physics data*, J. Phys. Conf. Ser. 898 (2017) 102006 [1704.05473].

[30] CMS Collaboration collaboration, T. C. Collaboration, *Simplified likelihood for the re-interpretation of public CMS results*, Tech. Rep. CMS-NOTE-2017-001. CERN-CMS-NOTE-2017-001, CERN, Geneva, Jan, 2017.