Minimum Bias Legacy of Search Results

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The end of LEP and SLC is a good moment to review the way to summarize search results in order to exploit at best, in future analyses and speculations, the pieces of information coming from all experiments. Some known problems with the usual way of reporting results in terms "CL limits" are shortly recalled, and a plea is formulated to publish just parametrized likelihoods, possibly rescaled to the asymptotic insensitivity limit level.

1. INTRODUCTION AND RATIONALE

This contribution starts from an observation which I hope is shared by many other particle phycisists, and scientists in general. Science, and particle physics in the specific case, should be considered a global activity, with individuals co-operating at different levels. Certainly, those who have played relevant role in the research should be acknowledged and, in special exceptional cases, rewarded. But the outcome of the research should be considered, finally, intellectual property of the community which has allowed the research. This point of view implies that a result should be presented in such a way that the pieces of information provided by an experiment should be possibly exploited at best by other scientists. This does not mean that I agree with populistic "do it yourself analysis", of which I have heard, as only those who know in depth detector performance and phenomenology can provide sensible results. Therefore, the problem is only that of making the result public in such a way that the data summaries can be used later in the most efficient way.

As far as searches are concerned, the experiment could report a spectacular effect which fits easily in the rest of the knowledge ("the net of beliefs") and all members of the physics community believe to a discovery. In other, not controversial cases, there could be no hint at all, as it usually happens in most searches. But there could be the case in which there are some indications. The scientists are then in the doubt of making the claim, thus risking their reputation, or keeping quite, thus loosing the chance. "The experiment was inconclusive, and we had to use statistics", said one once. But statistical methods are "inconclusive" by definition, if one interprets them rigorously: even 100 observed events over 1 expected background does not necesseraly implies that they are due to signal, though we all tend to believe so.... Therefore, it is important to distinguish between what we rationally believe from what the empirical facts teach us.

Besides personal preference for prudence or to risk, it is a matter of fact that the three situations sketched above (discovery, no effect and dubious hints) are not sharply separated. It is, therefore, important to understand what is the "optimal presentation", following some desiderata which can be summarised saying that the information contained in the experimental data should be presented in the most powerful and unbiased way.

- The result should not depend on whether one believes that there is no effect (i.e. giving a limit), or that something has been found (a claim with an associated e.g. mass, cross-section, etc).

- The pieces of evidence coming from different experiments should be combined in the

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most efficient way:

- If many independent data sets each provide a little evidence in favor of the searched-for signal, the combination of all data should enhance that hypothesis.
- If the indications are incoherent, their combination should provide a stronger constraint against that hypothesis.

These desiderata are not satisfied by the usual way of reporting results of searches with "CL limits". There are also other kinds of problems with "CL limits", some of which will be illustrated in next section. In particular, there is a problem of interpretation, which is the main cause of false alarms in the past and of the spread of misleading information to the general public (consequently throwing bad shadows to the reputation of particle physicists).

2. PROBLEMS WITH LIMITS. EXAMPLES FROM THE CONTACT INTERACTION SEARCHES

The first time I was confronted with the problems of stating limits was in occasion of an overall analysis of results on electron compositeness.[1] Analysing all results available at that time I found the situation not really satisfactory. In many cases the operative procedure used to evaluate limits was not described, and sometimes the numerical values of published limits seemed to disagree with differential cross-section to which they referred. I considered this problem particulary serious because the procedure which I understood had most consensus at that time had technical intrinsic problems in about 10% of the results, as it will be explained in a while. But out of the many douzens of results (many reactions $\times$ many coupling $\times$ many experiments), no technical anomaly was reported.

Other bad feature was the difficulty (or impossibility) to combine consistently the limits, with the consequent attitude to quote only the larger limit, that often was nothing but the largest statistical fluctuation, and not due to the experiment having the higher resolution power for that channel.

2.1. Costraining Λ to infinity

In a contact interaction analysis, no effect is obtained when the scale Λ is infinity. Therefore, one looks in the data for a compatibility of the measured Λ with infinity, thus providing a lower limit. Needless to say, a MINUIT minimization around infinity is not trivial at all, not to speak about the interpretation of results. As a consequence, I found that, at that times, there was much of kitchen to get a number to quote as lower limit out of the MINUIT printout. The worst case was an experiment which had got an upper limit of 1.3 TeV for a Λ in a certain coupling (this was the number I got reanalysing their data, due to a overfluctuation), but published exactly that number as lower limit.

This technical problem can be overcome working with the conjugate quantity $\epsilon = 1/\Lambda^2$, which should come about zero in case of no effect. The choice of the second power of Λ is due to the observation that the new terms in the cross-section come with that weigth. As a consequence, any additive "noise" make ϵ Gaussian distributed around the "true" value. Reporting the result on ϵ is certainly a good empirical practice, also because the results can be, in most cases, easily combined with the standard weighted average formula. Moreover, the standard deviation of ϵ is an intrinsic property of the experiment, a kind of "resolution power" depending on quality of the detector, luminosity, sensitivity to a particular reaction and level of background. And, in fact, my proposal[1] was to use simply σ_ϵ as measure of the resolution power of an individual experiment or of a combination of experiments.

The problem remains if we insist to report a CL limit. Calling $\epsilon_\circ$ the best fit value, and σ_ϵ the standard deviation (the latter is related to the curvature - "width" - of the χ^2 parabola) the standard 95% lower limit for $\Lambda^\pm$ is given by

$$\Lambda^\pm = \frac{1}{\sqrt{1.64\sigma_\epsilon \pm \epsilon_\circ}}\,. \qquad (1)$$

This implies that, if $|\epsilon_\circ|$ is approximately equal to $1.64\sigma_\epsilon$, then either limit becomes very large.

If $|\epsilon_\circ| > 1.64\,\sigma_\epsilon$ (10% of cases) there are problems with the standard procedure, including the fact that for either sign of coupling there should be an evidence, yielding an upper limit for Λ. As a matter of fact, unwanted results are tamed using "prescriptions", including "Bayesian prescriptions" – a contradiction in terms, in my view.

Besides the details of procedures, it is clear that this kind of approach can produce large limits just as statistical fluctuations, limits which have nothing to do with the effective resolution power of the experiment for a particular channel. It is also a matter of fact that the difficulty to combine limits obtained in such a way resulted in a general tendency to believe that a larger limit would make the lower ones obsolete, though the latter might result from higher resolution experiments.

3. MISINTERPRETATION OF "C.L. RESULTS"

A second problem with standard "C.L." is their interpretation, as it resulted from a survey I made in 1998 [2]. It came out, in fact, that most particle physicists believed that, e.g., a 95% C.L. lower bound on the Higgs mass meant that "the mass of the Higgs, provided it existed, has 95% chance to be above the limit, and 5% chance to be below". So, the problem is: do we understand each other? Do we communicate the correct information to the general public of tax payer which financed our research?

Nowadays it is quite understood by many people that such probabilistic statements are erroneous and misleading, but there are still people which use such wrong statements based on frequentistic C.L.'s to report the results to the general public.[3] So, for example, the 2000 hint of a 115 GeV Higgs was reported by a spokesperson of the LEP experiments saying that *"It is a 2.6 sigma effect. So there's still a 6 in 1000 chance that what we are seeing are background events, rather than the Higgs"*[4]. So, basically the problem persists, since also those who agree on what CL's *should not* mean, but still stick on the frequentistic approach, have difficulty in explaining what those results *do* mean. In my opinion, the very reason of this matter of fact is that, from a genuine inferential point of view – which is what matters in Science [5] – frequentistic CL's have *no meaning*,[2] and only in some case, under some hidden hypotheses, they can be related to sensible probabilistic statements. This is the reason why I insist in my position that "the solution to the problem of confidence limits begins with removing the expression itself".[7]

4. BAYESIAN WAY OUT

In my opinion, the solution is not to propose a new prescription with the hope that it will be adopted by some influential friends, but rather to change radically the attitude. This means we should review critically how our beliefs about physics quantities or laws of nature are modified by empirical observation and, once we have understood this scheme, we should stick to it, using logic instead of tradition and/or authority. The most powerful tool to learn from data is – and this is not merely my opinion – a <u>theorem</u>, which states how our beliefs must be updated by new pieces of information. This is the undiscussed role of Bayes' theorem, on which there is agreement also by those who disagree on the use of beliefs in physics (but this is a different story, on which there is little to discuss, since Science is nothing but a collection of beliefs based on empirical facts and intellectual constructions...).

Referring to a physics quantity of unknown value μ, the Bayes' theorem can be shortly expressed as

$$f(\mu\,|\,\mathrm{data},\mathrm{I}) \propto f(\mathrm{data}\,|\,\mu,\mathrm{I}) \times f_\circ(\mu\,|\,\mathrm{I})\,, \qquad (2)$$

where the three ingredients of the Bayesian inference are, from left to right, final pdf, likelihood and prior. Note that $f()$ stands, in this approach, for the the pdf expressing the relative beliefs.

It is self-evident that the likelihood has the role of re-shaping (re-weighting) the beliefs. This is a fundamental ingredient of inference, but not the

[2] Here is the most recent example I know of the series of misleading press releases in which frequentistic *numbers* (i.e. without a precise inferential meaning) are translated into probabilistic statements: *"The experimenters reported a three-sigma discrepancy in sin2qW, which translates to a 99.75 percent probability that the neutrinos are not behaving like other particles."*[6]

only one. Usually it is considered "more objective" than priors, because it is easier to agree on the response of a detector than on purely speculative values of μ. Nevertheless, $f(\text{data}\,|\,\mu, \text{I})$ is a probability too, and, as such, tells how much we believe that some data could be observed, for every hypothesis on μ.

At this point, the usual objection is that "there are the priors". In my opinion, there is no real problem if we understand on the logically crucial, often practically irrelevant role of priors. Without them, it would be impossible to make the "probabilistic inversion" from $f(\text{data}\,|\,\mu, \text{I})$ to $f(\mu\,|\,\text{data,I})$, which is the essence of the probabilistic inference. As far as its practical role, it depends on the different problems, and, more specifically, on the shape of the likelihood. This is the reason why I like to classify the inferential problems into *closed* and *open* likelihood.

4.1. Closed likelihood

The easy case is when, for a given set of data, the likelihood function, $\mathcal{L}(\mu) = f(\text{data}\,|\,\mu, I)$ is *closed* in both sides, i.e. $\mathcal{L}(\mu) \to 0$ when μ tends to the extremes of its physical region of definition (usually $-\infty < \mu < +\infty$ or $0 < \mu < +\infty$). The best understood example of this case is a Gaussian response of the apparatus. Under this condition and, in particular, when the width of $\mathcal{L}(\mu)$ is narrow with respect to any rational prior knowledge (the so Savage's "precise experiment" situation), the conclusions do not depend *practically* (in the sense how the result is perceived) from any reasonable model of the prior. Obviously, there could be someone who has a pure mathematical approach to the problem and propose a fancy mathematical expression for the prior. But I do not think this is a problem for physicists (and, anyhow, I am very curious to meet a defender of such fancy prior to check how ready he/she is to defend it with a suitable combination of bets...).

4.2. Open likelihood

The question becomes really problematic when the likelihood is *open* in either side, i.e. $\mathcal{L}(\mu)$ goes to a constant at the edges of the physical region (if it is open in both sides the experiment has little relevance, unless it helps in enhancing a certain region selected by other experiment having closed likelihood). For example, in the Higgs search of the kind performed at LEP, an infinite mass produces a non zero pdf of observing what we do observe. This "bad" feature is due to background: whatever we observe, we are never *certain* that it is not due to background alone. If this is the case, we are never able to normalize the final pdf, unless we force the normalization with a prior (or other empirical evidence, like radiative correction in the specific problem of the Higgs [8], which forbids (or strongly inhibits) high masses. It follows that the sensitivity to the prior is usually so critical (for an extensive, introductory discussion of problem and proposed solution, see Ref. [9]) that one should refrain from publishing, and spread to the general public, probabilistic limits, or limit which are usually mis-interpreted as such (this is what happens practically always with CL's limits).

In the case of open likelihood, only the likelihood itself should be reported. This information can be easily combined with that coming from other experiments, and satisfies the desiderata of Section 1. Alternatively, one could report the log-likelihood or the rescaled R function proposed in Refs. [9,8]. This functions can be parametrized in a suitable way, and perhaps stored in web sites in the case of searches having the likelihood depending of several quantities. An example of published parametrized log-likelihood can be found in Ref. [10].

An alternative to probabilistic, or CL bounds, to quantify with a single number (or with contour lines in the case of multidimensional analysis) is to report the *sensitivity bound*. This quantity should give, though roughly, the edge after which the experiment looses sensitivity to the search [9,7].

5. CONCLUSION: A PLEA FROM A EU TAX PAYER

In conclusion, my plea to LEP and SLC colleagues, as well as to all other physicists involved in searches, is to report likelihoods for all searched channel, so that the effort of all community can be used at best for all future analyses.

I would like to remember that publishing likelihoods was, actually, a point about which there was unanimous agreement in the January 2000 workshop on confidence limits held at CERN, as explicitly asked by Massimo Corradi and recorded by Louis Lyons [11]. It was, indeed, the only generally agreed conclusion of the workshop. [3]

On the light of the well known properties of the likelihood and on the agreement during that meeting, attended by representative by all major experiments, it is surprising to realize that reporting likelihoods has not become the standard yet. Some say that I am too naïve, and that this will never happen, essentially for two reasons. First, publishing a likelihood is much more committing than just giving a single "95% CL limit" in the region where other experiments report similar limits. Second, there are people specialized in combination of results which do use likelihoods, but are afraid to loose this privileged position if likelihoods are available to any student. I hope it is not so, and that it is only due to some inertia. Therefore I am still optimistic (or naïve).

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[3] In my point of view there were also another objective, though implicit, conclusion of that workshop. The fact itself that many "specialists", all educated on the same books of statistics, meet to try to solve self-evident problems, absurdities and contradictions originated by the approach followed by those books, means that the basic concepts of those books should be critically reviewed, and that the gurus of that school of thought, responsible of such confusion, should simply keep silent for the rest of their life.