Probably a discovery: Bad mathematics means rough scientific communication*

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Abstract

According to the media, in spring of this year the experiment CDF at Fermilab had made most likely ("this result has a 99.7 percent chance of being correct" [1]) a great discovery ("the most significant in physics in half a century" [2]). However, since the very beginning, practically all particle physics experts did not believe that was the case. This is the last of a quite long series of fake claims based on trivial mistakes in the probabilistic reasoning that can be sketched with the following statements, understandable by everybody: the probability of a senator to be a woman is not the same as the probability of a woman to be a senator; a free neutron has only 3×10^{-4} probability to decay after two hours, but, if we observe a neutron decaying after such a time, this is not an indication of an anomalous behavior of such a particle; the fact that the probability of a Gaussian random generator with $\mu = 0$ and $\sigma = 1$ to produce a number, rounded to three decimal digits, equal to 3.000 is 4.2×10^{-6} does not allow us to say that, once this number has been observed, there is only 4.2×10^{-6} probability it comes from that generator, neither that 4.2×10^{-6} is the probability that 3.000 is a statistical fluctuation; and not even, still considering the latter numerical example, we can say that the probability of 3.000 to be a statistical fluctuation is 1.3×10^{-3} , 'because' this is the probability of such a generator to produce a number *larger or equal* than the observed one. The main purpose of this note is to invite everybody, but especially journalists and general public, most times innocent victims of misinformation of this kind, to mistrust claims not explicitly reported in terms of how much we should believe something, under well stated conditions and assumptions. (A last minute appendix has been added, with comments on the recent news concerning the Higgs at LHC.)

^{*}Note based on lectures at the University of Perugia, 15-16 April 2011 and at MAPSES School in Lecce, 23-25 November 2011

1 Introduction

The title of this paper is a paraphrase of that of an article ("Probably guilty: Bad mathematics means rough justice"), appeared in New Scientist in October 2009 [3], whose *incipit* induced me to write a 'defense of Columbo',¹ that finally turned into a *sui generis* introduction to probabilistic reasoning [5].

Indeed, as I wrote in [5], "I can give firm witness that scientific practice is plenty of mistakes of the kind reported [in the cited New Scientist article], that happen even in fields the general public would hardly suspect, like frontier physics, whose protagonists are supposed to have a skill in mathematics superior to police officers and lawyers." In fact, although it might sound amazing, the 'bad mathematics' that induces a judge to form a wrong opinion about somebody's guilt or innocence is *the same* that, for example, in April of this year made media and general public *believe that particle physicists strongly believed* (although not yet certain) Fermilab had made "the most significant discovery in physics in half a century" [2] – how else should normal people interpret a statement such as "this result has a 99.7 percent chance of being correct" [1]?

Although I am quite used to fake claims of this kind, some of which are discussed in [6], and I have no interest in analyzing each individual case, the reason I want to return here to this subject, focusing on the Fermilab case, is twofold. First, due to the modern fast communication on the internet, I was involved in discussions about the issue 'discovery or not?' and realized that even people with university background in mathematics or physics were induced to think that it was most likely a discovery, or at least that Fermilab physicists were convinced this was the case. Second, a few days after the CDF announcement, I had to lecture PhD students in Perugia [7] and therefore I amused myself to collect related news and comments on the internet, because I expected that claim would have been a hot topic, as it turned out to be the case. Finally very recent interactions with students and young researchers in Lecce [8] convinced me to resume the paper draft started on the train Rome to Perugia.

2 The facts

On the 4th of April this year a paper appeared in the arXiv reporting about the "Invariant Mass Distribution of Jet Pairs Produced in Association with a W boson in $p\bar{p}$ Collisions at $\sqrt{s} = 1.96 \ TeV$ " [9] and the result was officially presented two days later in a 'special joint experimental-theoretical physics seminar' at Fermilab [10]. In the meanwhile, on the 5th of April the article "At Particle Lab, a Tantalizing Glimpse Has Physicists Holding Their Breaths" appeared on The New York Times [2]. The following days the news spread all around the world (you can amuse yourself enquiring Google with the languages you know).

Let us sketch how that happened.

¹Besides the inappropriate reference to the Columbo's episode, I consider that article substantially well done and I recommend its reading. To those interested in the subject "probability and the law" I also recommend, as starting points for further navigation, refs. [4].

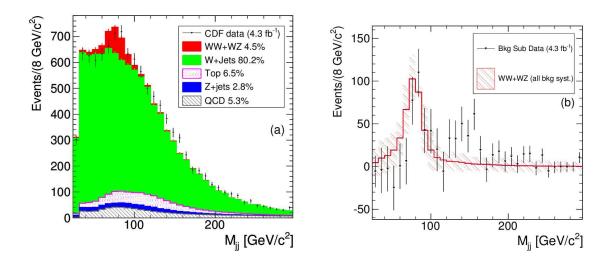


Figure 1: CDF data [9] before and after 'arithmetic' background subtraction

2.1 The (filtered and processed) data

Figure 1 reports the upper plots of figure 1 of the cited CDF paper. The left side one shows the histogram of the 'data',² the jet-jet mass distribution in 8 GeV bins, for a total of about 10800 events³ (points with vertical bars). The colored regions show the predictions split into several contributions, the most important of which is due to the production of two W bosons, or of a W boson together with a Z boson (red). We see that at around 140 GeV there are more events than 'expected' (an expression which we shall return to later).

The right side plot shows the data after the contributions called here 'background' (all but the red one of the left plot) were subtracted 'arithmetically'.⁴ In the five bins between 120 and 160 GeV there are about 230 events (but in the side bins there are even 'negative events' whose meaning is only mathematical). That was 'the excess'.

²For non experts it is important to clarify, although this is not deeply relevant here, that the histogram's 'data' are non simple 'empirical observations', but a result of selections and analysis (including calibrations), after suitable definitions of physical objects, like what a 'jet' is.

 $^{^{3}}$ This number, as well as 230 that follows, was estimated from the figure – precise numbers are irrelevant for the purpose of this paper.

⁴It seems rather natural to think that, if the purpose of a 'subtraction' would be that of highlight extra physical components in the spectrum, this procedure should not be simply an 'arithmetic subtraction' and, in particular, it should not yield unphysical negative counts.

2.2 The statistically motivated claim

A customary way to quantify the difference between an observed spectrum and the expected one is the famous χ^2 statistic.⁵ The CDF paper reports a " χ^2 per degree of freedom" (χ^2/ν) of 77.1/84 for the entire spectrum and 26.1/20 for the region 120-160 GeV. In both cases statistical practice based on this test states that "there is nothing to be surprised".

I know by experience that, when a test does not say what practitioners would like, other tests are tried – like when one goes around looking for someone that finally says one is right.⁶ Indeed, in the statistics practice there is much freedom and arbitrariness about which test to use and how to use it. This is because *hypothesis tests* of the so called *classical statistics* do not follow strictly from probability theory, but are just a collections of *ad hoc prescriptions*. For this reason I do not want to enter on what CDF finally quotes as *p*-value (with the only comment that it does not even seem a usual p-value). Let us then just stick to the paper, reporting here the claim, followed by a reminder about what a statistician would understand by that name:

- "we obtain a p-value of 7.6×10^{-4} , corresponding to a significance of 3.2 standard deviations";[9]
- "the p-value is the probability of obtaining a test statistic at least as extreme as the one that was actually observed, assuming that the null hypothesis is true." [11] [The null hypothesis (H_0) is in this case "only standard physics, without contributions from new phenomena".]

2.3 How the claim was explained to the general public (and perhaps even what some particle physicist thought)

But now we come to the clue point of this paper, since the CDF report [9] is by itself not so 'dangerous'. People do not know about p-values and, as a matter of fact, even those who calculate them for scientific purposes seem to by highly confused about their meaning [11], as we shall see later. Normal people only understand what is the chance that a team has made a discovery instead of having just observed a statistical fluctuation. Or, at least, how much experts believe that the bump is hint of new physics, instead than a fluke.

Let us go straight to read how the thing was reported in some online resources (boldface is mine).

⁵Let us remind that if a variable is described by a χ^2 distribution with ν degrees of freedom, our (probabilistic) expectation ('expected value') is ν , with expectation uncertainty ('standard deviation') $\sqrt{2\nu}$. Hence if θ_1 and θ_2 are variables of that kind, with $\nu_1 = 84$ and $\nu_2 = 20$, our expectations will be " 84 ± 13 " and " 20 ± 6 ", respectively. (As a side remark, we notice that, that adding a Gaussian component to explain the 'excess', the difference between expected and observed value of the test statistic increases, since the χ^2 goes to 56.7 for the entire region and 10.9 for the 'peak region'.)

⁶After years of practice in particle physics and related subjects, I have developed my rule of the thumb, which until now has never failed: "the funnier is the name of the test used to show that there is a disagreement with the 'Standard Model' (or whatever is considered firmly established), the less I believe that this is the case" (with the corollary that "in the future I will tend to mistrust those people").

• The New York Times, April 5 [2]:

"Physicists at the Fermi National Accelerator Laboratory are planning to announce Wednesday that they have found a suspicious bump in their data that could be evidence of a new elementary particle or even, some say, a new force of nature.

The experimenters estimate that there is a less than a quarter of 1 percent chance their bump is a statistical fluctuation"

• Fermilab Today, April 7 [12]

"Wednesday afternoon, the CDF collaboration announced that it has evidence of a peak in a specific sample of its data. The peak is an excess of particle collision events that produce a W boson accompanied by two hadronic jets. This peak showed up in a mass region where we did not expect one.

. . .

. . .

The significance of this excess was determined to be 3.2 sigma, after accounting for the effect of systematic uncertainties. This means that there is less than a 1 in 1375 chance that the effect is mimicked by a statistical fluctuation."

• Discovery News, April 7 [1]

"If you're a little hazy about the details of Wednesday's buzz surrounding the potential discovery of "new physics" in Fermilab's Tevatron particle accelerator, don't worry, you're not alone. This is a big week for particle physicists, and even they will be having many sleepless nights over the coming months trying to grasp what it all means.

That's what happens when physicists come forward, with observational evidence, of what they believe represents something we've never seen before. Even bigger than that: something we never even expected to see.

. . .

It is what is known as a "three-sigma event," and this refers to the statistical certainty of a given result. In this case, this result has a 99.7 percent chance of being correct (and a 0.3 percent chance of being wrong)."

• Jon Butterworth's blob on the Guardian [13]

"The last and greatest breakthrough from a fantastic machine, or a false alarm on the frontiers of physics?

• • •

If the histograms and data are exactly right, the paper quotes a one-inten-thousand (0.0001) chance that this bump is a fluke."

Let us make the logical complements of the highlighted statements (with the exception of the Discovery News one, that already provided the complementary propositions):

- "there is more than 99 percent chance their bump <u>is not</u> a statistical fluctuation";
- "there is more than 99.93% chance that the effect <u>is not</u> mimicked by a statistical fluctuation";
- "the paper quotes a 99.99% chance that this bump is not a fluke",

that can be summarized saying that 'we' should be highly confident this is a genuine discovery .

3 Where is the problem?

The question is very simple. No matter which test statistic has been used, there is no simple logical relation between a p-value and the probability of the hypothesis to test (' H_0 ' — in this case " H_0 = No New Physics").

Indeed, p-values are notoriously misunderstood, as well explained in a section of Wikipedia that I report here verbatim for the convenience of the reader [11], highlighting the sentences that mostly concern our discourse.

- 1. "The p-value is not the probability that the null hypothesis is true. In fact, frequentist statistics does not, and cannot, attach probabilities to hypotheses. Comparison of Bayesian and classical approaches shows that a p-value can be very close to zero while the posterior probability of the null is very close to unity (if there is no alternative hypothesis with a large enough a priori probability and which would explain the results more easily). This is the Jeffreys-Lindley paradox.
- 2. The p-value is not the probability that a finding is "merely a fluke." As the calculation of a p-value is based on the assumption that a finding is the product of chance alone, it patently cannot also be used to gauge the probability of that assumption being true. This is different from the real meaning which is that the p-value is the chance of obtaining such results if the null hypothesis is true.
- 3. The p-value is not the probability of falsely rejecting the null hypothesis. This error is a version of the so-called prosecutor's fallacy.
- 4. The p-value is not the probability that a replicating experiment would not yield the same conclusion.

- 5. (1 p-value) is not the probability of the alternative hypothesis being true.
- 6. The significance level of the test is not determined by the p-value. The significance level of a test is a value that should be decided upon by the agent interpreting the data before the data are viewed, and is compared against the p-value or any other statistic calculated after the test has been performed. (However, reporting a p-value is more useful than simply saying that the results were or were not significant at a given level, and allows the reader to decide for himself whether to consider the results significant.)
- 7. The p-value does not indicate the size or importance of the observed effect (compare with effect size). The two do vary together however the larger the effect, the smaller sample size will be required to get a significant p-value."

Are you still sure you had really understood what p-values mean?

4 Why there is such a problem?

Said in short, the reason of confusion is a mismatch between natural way of thinking and what we have learned in statistics courses.⁷

4.1 "The essential problem of the experimental method"

Human minds reason very naturally in terms of how believable (or 'likely', or 'probable') are different hypotheses in the light of everything we know about them (see e.g. [15]) and the mathematical theory of how beliefs are updated by new pieces of information was basically developed in a monumental work of Laplace exactly two hundreds years ago [16, 17], although nowadays this way of reasoning goes under the name *Bayesian*. This approach considers valid sentences such as "probability that the CDF bump is a fluke", "probability that the Higgs boson mass is below 130 GeV,"⁸ and similar, all expressions that refer to "a problem in the probability of causes, [...] the essential problem of the experimental method" [19]: from the observed effects we try to rank in probability the alternative causes that might have produced them.

4.2 A curious ideology

Now the problem arises because of a *curious ideology* of statistic thinking ('*frequentism*') that *forbids to speak of probability of causes*. It is not a matter of a different way of making the calculations, but an ideological refuse to calculate them! Nevertheless – and this is the

⁷Having written quite a lot on the subject, I don't want to go through yet another introduction to the subject and refer to the 'Columbo paper' [5] (someone might find useful also [14]), only reminding here some of the basic ideas.

⁸By the way it was about 36% percent by the best or our knowledge at the beginning of 1999 [18], and it has changed with time, especially during 2011!

worst! – most people, even if they think to adhere to the *frequentistic approach* ("probability <u>is</u> the long run limit of relative frequency", and so on), are not even aware that, according to this unfortunately still dominant school, they should not be allowed to speak about *probability of true values, probability of causes*, and so on. As a matter of fact, when I try to tell it in seminars, people usually stare at me as I had just landed from a far planet.

4.3 The mismatch

As a consequence, the results of frequentistic methods are usually interpreted as if they were probabilities of hypotheses, also because the names attached to them induce to think so, because they do not correspond to what they really are. More or less like the misusing of names, adjectives and expressions common in advertisements. It follows that some results of frequentistic prescriptions are called confidence interval, confidence level or 95% upper/lower C.L., although they are definitely not intended to mean how much we should be confident on something.⁹ If you consider yourself a frequentist, but you find strange what you are reading here, trust at least Neyman's recommendations:

"Carry out your experiment, calculate the confidence interval, and state that c belong to this interval. If you are asked whether you 'believe' that c belongs to the confidence interval you must refuse to answer. In the long run your assertions, if independent of each other, will be right in approximately a proportion α of cases." (J. Neyman, 1941, cited in Ref. [22])

Clearly, this is not what a scientist (as well as everybody else) wants. Otherwise, if one is just happy to make statements that are e.g. 95% of times correct, there is no need to waste time and money making experiments: just state 95% of times something that it is practically certainly true and the remaining 5% something that is practically certainly false.¹⁰

Put in other terms, if what you want is a quantitative assessment of how much you have to be confident on something, on the basis of the information available to you, then use a framework of reasoning that deals with probabilities. The fact that probabilities might be be difficult to be precisely assessed in quantitative terms does not justify the fact that *you calculate something else and then use it as if it were a probability.* For example, on the basis of the evaluated probability you might want to take *decisions*, that is essentially making *bets of several kinds*, that for example might be, sticking to particle physics activity: how much

⁹For example, when I ask about the meaning of 95% CL lower bound on Higgs mass from LEP direct search, practically everybody – and I speak of particle physicists! – 'explains' the result in probabilistic terms [20], although it is well known to frequentistic experts that "The lower bounds on the Higgs mass that are quoted for the direct Higgs searches at LEP say absolutely nothing about the probability of the Higgs mass being higher or lower than some value." [21] (By the way, it seems that the method described in [21] is essentially the one on which the LHC collaborations have agreed to report search limits: at least you know now what these results (do not) mean!)

¹⁰If you want to try, you can play with *The ultimate confidence intervals calculator* [23] and no strict follower of Neyman's teaching can blame you of the results, that asymptotically will 'cover' the true value of whatever quantity you have in mind in exactly the proportion of times you pre-define.

emphasis you want to give to a 'bump' (just send a student to show it in a conference, publish a paper, or even make press releases and organize a 'cerimonius' seminar with prominent people sitting in the first rows); or if it is worth continuing an experiment; if it is better to build another one; or perhaps to invest in new technologies; or even to plan a future accelerator; and so on. In all cases, *rational decisions* require to balance the *utilities* resulting from different scenarios, weighted by how probable you consider them. Using p-values, or something similar, as if they were probabilities can lead to very bad mistakes.¹¹

As you may know CERN DG, L.Maiani, has decided to shut off LEP. The decision is to be confirmed at a CERN Committee of Council meeting on Friday 17th.

As you probably know there is evidence for a Standard Model Higgs boson seen in the data in the last few months, with a probability as a background fluctuation of 4 per mille, or 2.9 sigma.

In other words we are seeing exactly what we should expect if Mh=115....

[If you wonder why 2.9σ 's is 4 per mille, instead of 2, don't ask me.] The message ended with a request to write to Maiani in support of extending LEP run. Here follows my instant reply:

Let me understand:

. . .

do you REALLY feel 99.6% sure that the Higgs is around 115GeV (let's say below the effective kinematical threshold at the present LEP energy)? If not, how much are you confident?

Running or not running is a delicate decision problem which involves beliefs and risks (both financial and sociological). Therefore, I cannot disagree much with Maiani, being in his position.

On the other hand, in the position of any LEP collaborator I would push to run, certainly! (Given the same beliefs, the risk analysis is completely different).

Being myself neither the CERN Director-General, or a LEP physicist, but, with this respect, just a physics educated tax payer, I find myself more on the side of Maiani than on that of our LEP colleagues.

To make it clear, the "99.6%" could not be how much we had to rationally believe the Higgs was at 115 GeV, because it was a 0.004 p-value incorrectly turned into probability. Estimating correctly the probability, one would have got *a few percent* (see e.g. [18] for the method, although the numbers had changed in the meanwhile). And with a few percent, it would have been crazy to continue the LEP run, delay LHC and so on. On the other hand, if there was really a 99.6% probability, then LEP had to go on. (As it often happens with misinterpreted frequentistic methods, the errors are not little, like getting 99.6 for what should have better been 99.1, 98.5, or even perhaps 97%! – see chapter 1 of [6] for other examples. Here one considered practically certain something that was instead almost impossible.)

¹¹For example, in 2000 there was some excitement at CERN because some LEP experiments were observing some events above the expectation and there was a big action against the CERN directorate, that had decided to stop LEP in order to use structures and human/financial resources for LHC. This was an email I received the 10th of November 2000, addressed to a short list of physicists:

Subject: Do you want the Higgs found next year?

5 The mathematics of beliefs

Among the web resources mentioned above, I find particularly enlighting Jon Butterworth's blob on the Guardian [13]. Let us go back to the expression he used to explain the statistical meaning of the result, and compare it with the last paragraph of the article, split here into three pieces (1-3):

- (0) "the paper quotes a one-in-ten-thousand (0.0001) chance that this bump is a fluke."
- (1) "My money is on the false alarm at the moment,..."
- (2) "... but I would be very happy to lose it."
- (3) "And I reserve the right to change my mind rapidly as more data come in!"

We have already seen that proposition (0) is just a misleading misinterpretation of p-values, about which there is little to discuss. Instead, the last paragraph is a masterpiece of correct good reasoning (I would almost say *Good's reasoning* [22]), that deserves some comments.

5.1 Stating the strength of "pragmatic beliefs" by odds

From proposition (1) we finally understand very well Butterworth's beliefs, in spite of the contradiction with (0). In fact, since ancient times betting has been recognized to be the best way to check how much one really believes something, as well stated by Kant when he talks about *pragmatic beliefs*: [24]

"The usual touchstone, whether that which someone asserts is merely his persuasion – or at least his subjective conviction, that is, his firm belief – is betting. It often happens that someone propounds his views with such positive and uncompromising assurance that he seems to have entirely set aside all thought of possible error. A bet disconcerts him. Sometimes it turns out that he has a conviction which can be estimated at a value of one ducat, but not of ten. For he is very willing to venture one ducat, but when it is a question of ten he becomes aware, as he had not previously been, that it may very well be that he is in error."

And, in fact, in the mathematical theory of probability of Laplace all probabilistic statements can be mapped into betting statements, like his famous one concerning his evaluation of the uncertainty on the value of the mass of Saturn: [17]

"To give some applications of this method I have just availed myself of the opus magnus that Mr. Bouvard has just finished on the motions of Jupiter and Saturn, of which he has given very precise tables. ... His calculations give him the mass of Saturn as 3,512th part of that of the sun. Applying my probabilistic formulae to these observations, I find that the odds are 11,000 to 1 that the error in this result is not a hundredth of its value."

That is

$$P(3477 \le M_{Sun}/M_{Sat} \le 3547 \,|\, I(\text{Laplace})) = 99.99\%$$

where I(Laplace) stands for all information available to Laplace (probabilistic statements are always conditioned by a state of information). The Laplace's result is a very clear statement and there is a perfect match between beliefs, odds and probabilistic statement. Instead, I ensure you, a "95% C.L. lower limit" result <u>cannot</u> be turned into a 19:1 bet that the quantity in object is above that limit (see footnote 9), neither a p-value of e.g 10^{-4} can be turned into a 10000:1 bet in favor of a discovery (see also the last minute reference [30].)

5.2 Coherent virtual bets

A few comments on the way Laplace reported his result in terms of betting odds are in order.

- First, he does not say that he would be ready to make a 11,000 to 1 in favor of the result, but rather that "the odds <u>are</u> 11,000 to 1". This implies that "11,000 to 1 in favor" and "1 to 11,000 against" are both fair bets. This is essentially the idea behind the so called de Finetti's *coherent bet* [25]: in order to express your degree of belief in favor of something, you fix the odds and leave somebody else to choose in which direction to bet. This is the best way to force people to assess what they really believe, no matter what the event is and how the probability has been evaluated (at limit, just by intuitive reasonings, if no other means are available why not? what is important is that once you fix the odds you have no sensitive preference towards either direction.).
- Second, what is the sense of a bet whose result would have not probably been solved in Laplace's lifetime? This is another important ingredient: the fact that bets have to be considered *hypothetical* ('*virtual*'). It is just a way to assess probability.¹²

5.3 Belief Vs imagination, beliefs Vs wish, subjective Vs arbitrary: the role of the coherent (virtual) bet

The role of the bet, although *virtual*, in the sense of 'as I would be called to bet', is crucial to make clear distinctions between different concepts that could otherwise be confused.

• We can imagine something, just combining ideas (even "the New Jerusalem, whose pavement is gold and walls are rubies" – on this issue a reference to Hume is a must [15], T.1.1.1.4), but, nevertheless, we could not believe it.

 $^{^{12}}$ But if you really have the chance of making real bets, don't use the fair odds: you want to maximize the expected gain! This is what insurance companies and professional bookmakers do: evaluate the fair odds and then propose *the most unfair ones in a given direction*, unbalanced as much as someone can still accept them.

- We should also be careful not to confuse what we wish with what we do belief. I would like to win the highest prize playing at a lottery, but I don't believe I will. Similarly and this is well stated in proposition (2) I think everyone working in frontier science would be very happy if something really new 'appears', such that it forces us to change our vision of the world. But before we can accept something like that we really need much experimental evidence, obtained in different ways with different techniques.
- Finally, it is a matter of fact that

"Since the knowledge may be different with different persons or with the same person at different times, they may anticipate the same event with more or less confidence, and thus different numerical probabilities may be attached to the same event." [26]

It follows that probability is always conditional probability, as again well stated by Schrödinger [26],

"Thus whenever we speak loosely of 'the probability of an event,' it is always to be understood: probability with regard to a certain given state of knowledge,"

i.e. P(E) has always be understood as P(E | I), where I stands for a given status of information, that changes with persons (*subjects*) and *time*. Hence a probability assessment has always to be meant as

 $P[E \mid I_s(t)].$

This is the meaning of the adjective *subjective* attached to probability, that *has nothing to do with arbitrary*. Once again, thinking in terms of bets, instead of noble but empty ideals of 'objectivity' that can easily drift to 'metaphysics', helps to distinguish what is really arbitrary from sound rational beliefs.

To conclude this subsection, when somebody claims something on the basis of arguments that you do not clearly understand, follow Kant's suggestion and ask him/her to bet for money. And, if it is a claim in favor of new/extraordinary physics only based on a p-value, don't hesitate to cash, as nicely shown in the comic of figure 2 [27], appeared immediately after the recent (in?-)famous result on superluminar neutrinos [28]. (But, besides the humorous side, I invite my colleagues to reflect on the fact the general public is not by definition stupid and there is an increasing number of well educated tax payers who are starting to get tired of fake claims.)

5.4 Updating beliefs

Let us come finally to proposition (3): rational people are ready to change their opinion in front of 'enough' experimental evidence. What is *enough*? It is quite well understood that it all depends on

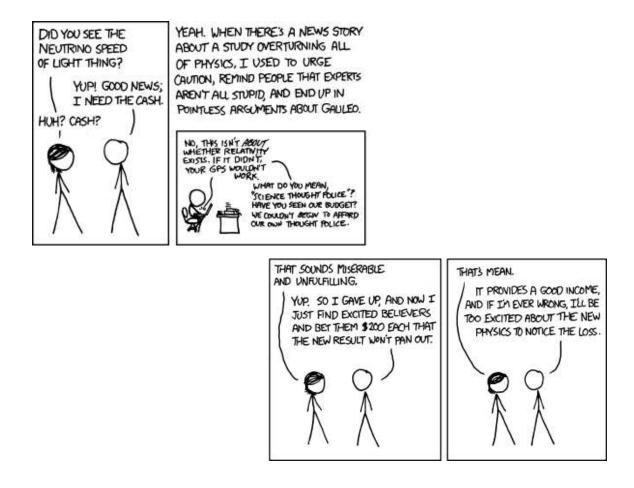


Figure 2: A comic from *xkcd*,[27] on superluminar neutrino, valid for any fancy claim.

- how the new thing differs from from our initial beliefs;
- how strong our initial beliefs are.

This is the reason why practically nobody took very seriously the CDF claim (not even most members of the collaboration, and I know several of them), while *practically everybody is now convinced that the Higgs boson has been finally caught at CERN*[31] – no matter if the so called 'statistical significance' is more ore less the same in both cases (which was, by the way, more or less the same for the excitement at CERN described in footnote 11 - nevertheless, the degree of belief of a Higgs boson found at CERN is substantially different!).

Probability theory teaches us how to update the degrees of belief on the different causes that might be responsible of an 'event' (read 'experimental data'), as simply explained by Laplace in his *Philosophical essay* [17] ('VI principle'¹³ at pag. 17 of the original book,

¹³In the *Essai* 'principles' do no stand for what we mean now as 'first principles', or 'axioms', but are rather the fundamental rules of probability that Laplace had derived elsewhere.

available at book.google.com – boldface is mine):

"The greater the probability of an observed event given any one of a number of causes to which that event may be attributed, the greater the likelihood¹⁴ of that cause {given that event}. The probability of the existence of any one of these causes {given the event} is thus a fraction whose numerator is the probability of the event given the cause, and whose denominator is the sum of similar probabilities, summed over all causes. If the various causes are not equally probable a priory, it is necessary, instead of the probability of the event given each cause, to use the product of this probability and the possibility of the cause itself. This is the fundamental principle of that branch of the analysis of chance that consists of reasoning a posteriori from events to causes."

This is the famous *Bayes' theorem* (although Bayes did not really derive this formula, but only developed a similar inferential reasoning for the parameter of Bernoulli trials¹⁵) that we rewrite in mathematical terms [omitting the subjective 'background condition' $I_s(t)$ that should appear – and be the same! – in all probabilities of the same equation] as

$$P(C_i | E) = \frac{P(E | C_i) \cdot P(C_i)}{\sum_j P(E | C_j) \cdot P(C_j)}$$

This formula teaches us that what matters is not (only) how much E is probable in the light of C_i (unless it is impossible, in which case C_i it is ruled out – it is falsified to use a Popperian expression), but rather

- how much $P(E | C_i)$ compares with $P(E | C_j)$, where C_i and C_j are two distinguished causes that could be responsible of the same effect;
- how much $P(C_i)$ compares to $P(C_i)$.

$$f(p \mid n, x) = \frac{f(x \mid n, p)}{\int_0^1 f(x \mid n, p) \, dp}$$

Laplace solved independently this problem and, indeed, the formula that gives the expected value of p, i.e.

$$\mathbf{E}[p] = \frac{x+1}{n+2} \,,$$

is known as Laplace's rule of succession.

 $^{^{14}}$ Note that here likelihood is the same as probability, and has nothing to do with what statisticians call 'likelihood' – reading directly the original French version might help, also taking into account that two hundred years ago the nouns were not as specialized as they now are.

¹⁵In modern terms, the problem solved by Bayes in a quite convoluted notation [29] was the inference of the binomial parameter p, conditioned on x successes in n trials, under the assumption that all values of p were *a priori* equally likely

The essence of the Laplace(-Bayes) rule can be emphasized writing the above formula for any couple of causes E_i and E_j as

$$\frac{P(C_i \mid E)}{P(C_i \mid E)} = \frac{P(E \mid C_i)}{P(E \mid C_j)} \times \frac{P(C_i)}{P(C_j)} \times \frac{P(C_i)$$

the odds are updated by the observed effect E by a factor ('*Bayes factor*') given by the ratio of the probabilities of the two causes to produce that effect.

In particular, we learn that:

- It makes no sense to speak about how the probability of C_i changes if:
 - there is no alternative cause C_i ;
 - the way how C_j might produce E has not been modelled, i.e. if $P(E | C_j)$ has not been somehow assessed.
- The updating depends only on the Bayes factor, a function of the probability of *E* given either hypotheses, and not on the probability of other events that have not been observed and that are even less probable than *E* (upon which p-values are instead calculated).
- One should be careful not to confuse $P(C_i | E)$ with $P(E | C_i)$, and in general, P(A | B) with P(B | A). Or, moving to continuous variables, $f(\mu | x)$ with $f(x | \mu)$, where 'f()' stands, depending on the contest, for a *probability function* or for a *probability density function*, while x and μ stand for an observed quantity and a true value, respectively.

In particular the latter points looks rather trivial, as it can be seen from the 'senator Vs woman' example of the abstract. But already the Gaussian generator example there might confuse somebody, while the ' μ Vs x' example is a typical source of misunderstandings, also because in the statistical jargon $f(x | \mu)$ is called 'likelihood' function of μ , and many practitioners think it describes the probabilistic assessment concerning the possible values of μ (again misuse of words! – for further comments see Appendix H of [5]).

6 Conclusions

Fake claims of discoveries are mainly caused by statistical prescriptions that do not follow probabilistic reasoning, meant as *mathematics of beliefs*, as it was conceived as a whole by Laplace and that nowadays is known under the appellative 'Bayesian'. As a consequence

- the concept of probability of causes is refused;
- the role of Bayes' theorem to update beliefs is rejected, and hence
 - the role of prior knowledge is not explicitly recognized;

- the myth has been created that a single hypothesis can be 'tested' without taking explicitly into account alternative(s);
- the intuitive concept of 'probabilities of causes' has been surrogated by *ad hoc* hypothesis test prescriptions,
 - whose choice and use are rather arbitrary;
 - whose results are routinely misinterpreted.

Unfortunately, this wobbly construction faces against the human predisposition to think naturally in terms of degrees of belief about anything we are in condition of uncertainty, including the several causes that might have produced the observed effects. The result of this mismatch is that

- probabilities of the effects given the causes are confused with the probabilities of the causes given the effects;
- even worse, p-values are used as if they were the probability that the hypothesis under test is true.

In addition, the pretension that 'priors are not scientific and should not enter the game' ("the data should speak by themselves") avoids that sound scientific priors mitigate the deleterious effects of misunderstood p-values.

But, fortunately, being the natural intuition of physicists rather 'Bayesian' [20], after all it is more a question of *rough scientific communication* than of *rough science*. In fact, even the initial excitement of someone who takes a bit too seriously claims that the rest of the physics community classifies immediately as 'fake' – priors! – is harmless, if the discussions remain in the community. And the debates are often even profitable, because they offer an opportunity to check how new possible phenomena and new explanations could fit into the present *network of beliefs* based on all previous experimental observations. This is for example what has recently happened with the exchange of ideas that has followed the Opera result on neutrino speed, from which most of us have learned something.

As far as the communication of claims to non experts, that include also physicists of other branches, or even of a close sub-branch, my recommendation is of making use, at least qualitatively, of the Bayesian odd update, i.e.

- state how much the experimental *data push* towards either possibility (that is the Bayes factor, which *has nothing to do with p-values*);
- state also how believable are the two hypotheses independently of the data in object.

I am pretty sure most people can make a good use of these pieces of information. Moreover, my recommendation to journalist and opinion makers (including bloggers and similar) is that, in the case of doubt:

- don't accept answers in terms of p-values, unless you are sure you understand them well and you feel capable to explain their correct meaning to the general public without they become somehow probabilities of the hypotheses to be compared (good luck!);
- refuse as well 'confidence levels', '95% confidence exclusion curves' and similar;
- ask straight the direct questions:
 - How probable it is? (Possibly informing threatening! him/her in advance that his/her answer will be reported as "Dr X.Y. considers it such and such percent *probable*".)
 - How much do you believe? (Same as the previous one.)
 - How much would you believe in either hypothesis if you did not have this data? (The answer allows you to estimate the priors odds.)
 - How much would you believe in either hypothesis given these data, if you considered the two hypotheses initially equally probable? (The answer allows you to evaluate the Bayes factor.)
 - How much would you bet in favor of your claim? (And if you realize there are the conditions described in section 5.3 and figure 2, *don't miss the opportunity to gain some money!*)

To end, I would like to congratulate all people working at LHC on the amazing high quality work done in these years and on having been able to report these convincing hints on the Higgs boson in a record time (I had never betted in favor of this possibility in 2011 even six months ago! But now the real exciting bet is *what next?*).

References

- Discovery News, What Has the Tevatron Really Discovered?, April 7 2011, http://news.discovery.com/space/what-has-the-tevatron-really--discovered-110407.html
- The New York Times, At Particle Lab, a Tantalizing Glimpse Has Physicists Holding Their Breaths, April 5 2011, http://www.nytimes.com/2011/04/06/science/06particle.html
- [3] A. Saini, Probably guilty: Bad mathematics means rough justice, nr. 2731, 24 October 2009, pp. 42-45, http://www.newscientist.com/issue/2731.
- [4] A. Saini, A formula for justice, http://www.guardian.co.uk/law/2011/oct/02/ formula-justice-bayes-theorem-miscarriage;
 N. Fenton, Science and law: Improve statistics in court, Nature 479 (2011) 36-37,

http://www.nature.com/nature/journal/v479/n7371/full/479036a.html; Bayes and the Law, https://sites.google.com/site/bayeslegal/home.

- [5] G. D'Agostini, A defense of Columbo (and of the use of Bayesian inference in forensics): A multilevel introduction to probabilistic reasoning, http://arxiv.org/abs/1003.2086.
- [6] G. D'Agostini, Bayesian reasoning in data analysis a critical introduction, World Scientific 2003.
- [7] http://www.roma1.infn.it/~dagos/dott-pg/
- [8] Methods of Analysis for Physics in Space, Earth and Sea, MAPSES 2011, Lecce, Italy, 23-25 November 2011, http://mapses.le.infn.it/ http://www.roma1.infn.it/~dagos/dott-pg/MAPSES-2011/
- [9] CDF Collaboration, T. Aaltonen, et al, Invariant Mass Distribution of Jet Pairs Produced in Association with a W boson in $p\overline{p}$ Collisions at $\sqrt{s} = 1.96 \text{ TeV}$, Phys.Rev.Lett.106:171801,2011 (preprint http://arxiv.org/abs/1104.0699).
- [10] http://vmsstreamer1.fnal.gov/Lectures/WC/110406Cavaliere/index.htm
- [11] http://en.wikipedia.org/wiki/P-value
- [12] Fermilab Today, The data peak that causes excitement, April 7, 2011, http://www.fnal.gov/pub/today/archive_2011/today11-04-07_CDFpeakresult.html
- [13] J. Butterworth, The**Tevatron** bump, Blob 'Life goesPhysics', hosted the Guardian, and by 9 April 2011,http://www.guardian.co.uk/science/life-and-physics/2011/apr/09/1
- [14] G. D'Agostini, From Observations to Hypotheses: Probabilistic Reasoning Versus Falsificationism and its Statistical Variations, 2004 Vulcano Workshop on Frontier Objects in Astrophysics and Particle Physics, Vulcano (Italy), http://arxiv.org/abs/physics/0412148
- [15] D. Hume, A Treatise of Human Nature, 1739; An Enquiry Concerning Human Understanding, 1748.
- [16] P.S. Laplace, Théorie analytique des probabilités, 1812;
- [17] P.S. Laplace, Essai philosophique sur les probabilités, 1814, http://books.google.it/books?id=JrEWAAAAQAAJ (the English quotes in this paper are taken from A.I. Dale's translation, Springer-Verlag, 1995).

- [18] G. D'Agostini and G. Degrassi, Constraints on the Higgs Boson Mass from Direct Searches and Precision Measurements, Eur. Phys. J. C10 (1999) 663-675, http://www.springerlink.com/content/101160/ (http://arxiv.org/abs/hep-ph/9902226).
- [19] H. Poincaré, "Science et Hypothèse", 1905.
- [20] G. D'Agostini, "Bayesian reasoning versus conventional statistics in high energy physics", Proc. XVIII International Workshop on Maximum Entropy and Bayesian Methods, Garching (Germany), July 1998, V. Dose et al. eds., Kluwer Academic Publishers, Dordrecht, 1999 (http://arxiv.org/abs/physics/9811046).
- [21] A.L. Read, Modified frequentist analysis of search results (the CL_ss method), 1st Workshop on Confidence Limits, CERN, Geneva, 17-18 Jan 2000, http://cdsweb.cern.ch/record/411537?ln=en.
- [22] I.J. Good, Probability and the weighing of Evidence, Charles Griffin and Co., 1950.
- [23] http://www.roma1.infn.it/~dagos/ci_calc.html
- [24] E. Kant, Critique of Pure Reason, 1871, (Part VIII of the 1929 Norman Kemp Smith translation, e-text available at http://www.hkbu.edu.hk/~ppp/cpr/dmeth.html).
- [25] B. de Finetti, "Theory of probability", J. Wiley & Sons, 1974.
- [26] E. Schrödinger, The foundation of the theory of probability I, Proc. R. Irish Acad. 51A (1947) 51.
- [27] http://xkcd.com/955/
- [28] The Opera Collaboration, T. Adam et al., Measurement of the neutrino velocity with the OPERA detector in the CNGS beam, http://arxiv.org/abs/1109.4897.
- [29] T. Bayes, "An assay towards solving a problem in the doctrine of chances", Phil. Trans. Roy. Soc., 53 (1763) 370 http://rstl.royalsocietypublishing.org/content/53/370.full.pdf
- [30] The New York Times, 'Tantalizing Hints' but No Direct Proof in Particle Search, December 13 2011, http://www.nytimes.com/2011/12/14/science/tantalizing-hints-but-nodirect-proof-in-search-for-higgs-boson.html
- [31] http://indico.cern.ch/conferenceDisplay.py?confId=164890

Appendix: '???' at Fermilab Vs Higgs boson at CERN

Since, just before I was going to post this paper there has been the joint ATLAS and CMS seminar on the Higgs boson search at CERN, followed by days of rumors, I cannot avoid to add here some last minute comments on these results, comparing them with the CDF case.

The big difference between the Fermilab result discussed here and that of CERN [31] is essentially a question of *priors*, whose role was discussed in section 5. If we observe something unexpected, we need an overwhelming experimental evidence before we are convinced this is really a genuine discovery, which is not case of the highly expected Higgs at LHC. These are the arguments in favor of the fact that *the elusive beast has been finally surrounded* (every particle hunter sniffs it, although it will be considered to finally in 'our hands' only when we shall be able, with the increasing number of events, to study its behavior, such as decay modes etc.):

- the so called Standard Model of particle physics provides an excellent description of a *network of experimental facts*, and such a particle is required to give a sense to the theory;
- the indirect information on the Higgs boson ('radiative corrections') constrains its mass at the *order of magnitude* of 100 GeV (although with a large uncertainty see [18] for a probability distribution, even though this has been slightly changing with time);
- direct searches at LEP have pushed its mass with *almost certainty* above 114 GeV;¹⁶
- similarly, direct searches at the LHC and at the Tevatron have squeezed its mass value into a relative narrow window (I save the reader yet another disquisition on the meaning of those limits);
- the CERN indication shows up
 - in the middle of the remaining window of possibility (and then not in contradiction with other experimental pieces of information);
 - with production rate in agreement with the theory and with many other experiments (from which the theoretical parameters have been inferred);
 - with decay modes also in substantial agreement with expectations;
 - in two detectors, although with some differences that can be considered physiological, taking into account of the difficulty of the search.

 $^{^{16}}$ As mentioned in footnote 9, the 95% CL bound has nothing to do with 95% probability that its value was above the bounds. Translating the experimental information from the direct search into probabilistic assessments is not that easy, because the number also depends on the upper limits. In particular, if there would be 'no' upper bound on the mass (that obviously cannot weigh grams!) there is no way to calculate the required probability. For further details see [18] and chapter 13 of [6].

In addition, I would also like to remark that the presentations of the two team leaders have been rather prudential, as if, instead of the Higgs, it were just an unexpected bunch of extra events in the middle of nowhere.

Some further remarks are in order.

- The reason why practically every particle physicist is highly confident that the Higgs is in the region indicated by LHC has little to do with the *number of sigma's* (I hope the reader understands now that the mythical value of 5 for a 'true discovery' is *by itself* pure nonsense, as it is clear from the comparison between '???' at the Tevatron and the Higgs boson at CERN *in the only place it could be* after it has been hunted unfruitfully elsewhere.¹⁷)
- This number of sigma's cannot be turned in probabilistic statements (or odds!) about Higgs or not-Higgs, as we read again on The New York Times:¹⁸

The Atlas result has a chance of less than one part in 5,000 of being due to a lucky background noise, which is impressive but far short of the standard for a "discovery," which requires one in 3.5 million odds of being a random fluctuation. [30] (Again misinterpreted p-values – basta!)

- Instead, if we want to make quantitative probabilistic assessments, we *need the like-lihoods* (this time this noun has the technical meaning statisticians use), per each experiment and per each channel, instead of the frequentistic 95% CL exclusion curves, of dubious meaning and useless to be combined. A plea to the LHC collaborations is therefore in order: **please publish likelihoods**.
- In the past days I have visited some internet resources to check the rumors. As a result
 - I have seen quite a lot 'creative thinking' concerning related statistics/probability matter (starting from the New York Time article cited above) and you can amuse yourself browsing the web. I just would like to suggest to Italian readers http://www.keplero.org/2011/12/higgs.html where there are some attempts (in particular by nicola farina and Moping Owl) to clarify some probabilistic issues;
 - in the name of many contributors to forums and blogs, I make special plea to my colleagues physicists and to journalists:

please stop relating the Higgs boson to God!

¹⁷And if it wouldn't exist at all? OK, formulate the alternative model and try to assess your beliefs in the alternatives.

¹⁸I definitely hope that when this influential newspaper reports on probability of important, uncertain scenarios that really matter for our lives, such as economy, health, international crises, future of the Planet and so on, its experts really know what they are talking about!