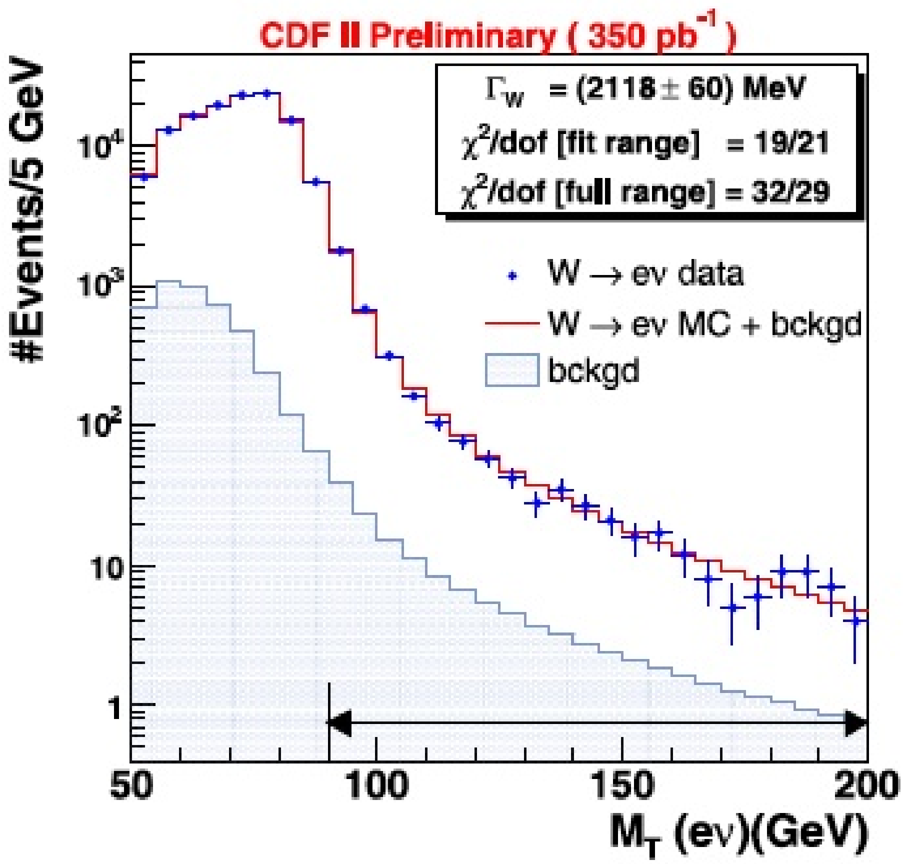
Tquark Updates October 2007

In comments on a 5 September 2007 entry in Tommaso Dorigo's blog:

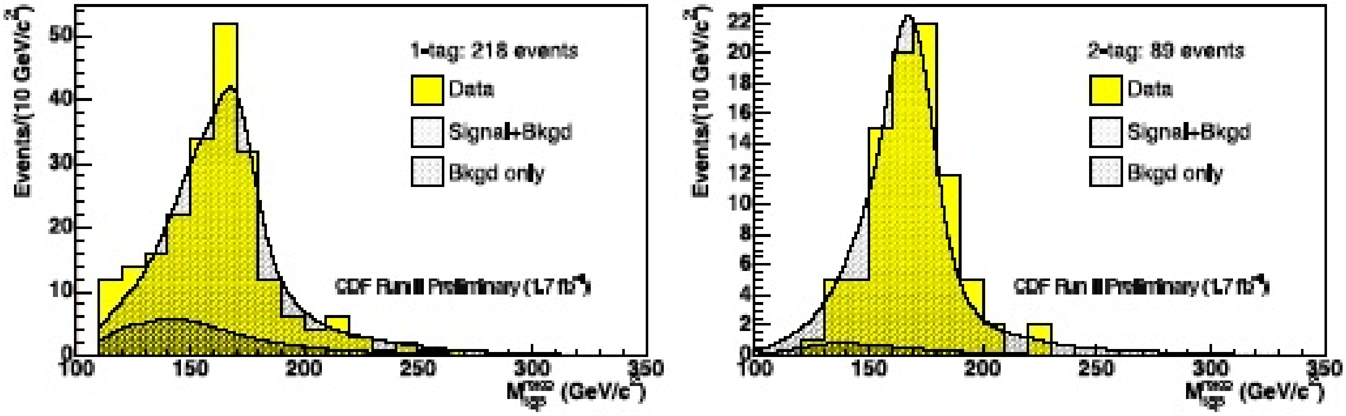
I said: "... in the ... plot ...



[from Tommaso Dorigo's]... PASCOS 2007 talk ... there seems to be a bump in the W to e nu data for M_T around 170 GeV, which is the CDF accepted Tquark mass and the mass of the middle state in my 3-state Tquark model.

It also seems to me that there is a smaller bump around 130 GeV, which is the Tquark mass for the low state in my 3-state Tquark model.

Note that the ... plot ... does not go above 200 GeV, but in ...[another plot in that paper]... (both for 1-tagged and 2-tagged)



> it seems to me that there is a small peak around 220 GeV, which is the Tquark mass for the high state in my 3-state Tquark model.

Tommaso Dorigo replied:

> "... I honestly do not see the bumps you mention - I think they are quite reasonable fluctuations up and down in the spectra, especially in the top mass plot where stats are poor. In any case, the W sample is really devoid of contaminations from top decays, because of the selection cuts applied to clean up the sample - among them, a jet veto. ...".

I then asked:

> "... What if I wanted to see whether such Tquark bumps might exist, and so wanted to look at the sample without the Tquark suppressing selection cuts ?
>
> How much trouble would it be for CDF to do such an analysis ?
>
> Would CDF provide access to the sample data without such cuts to an outside analyst ? ..".

Tommaso Dorigo replied:

> "... a search for a top quark of 140 GeV or elsewhere was done - when the top had not been found yet. However, I admit that, once it was found conclusively at 174 GeV, people stopped looking elsewhere. In any case, the background modeling has now reached a precision which leaves little margin for other resonances, but admittedly one never knows, at least until a limit is extracted directly. One could say that the xs limits obtained until 1993 still apply though. If your t quark at 140 and its other instantiations at higher masses are predicted to have lower cross sections, they might as well be still allowed, considering some of the backgrounds are tuned using the data (and if some unknown signal is buried therein, it may therefore get washed away).
>
> Doing such a search is tough because no grad student would be willing to waste his time on

setting a limit (few would truly believe the top at 140 or 190 is there) and then being unable to publish it -

the theoretical motivation for the search is wanting, given the lack of any publication in support.

Yes, this is the trouble with mainstream physics.

There is no way you can get your hands on CDF or D0 run II data other than associating to somebody willing to do the analysis. When I did a search for associated VY production in 2001-2003, there was a chinese physicist from Berkeley who participated, and eventually he was accepted as co-author of the paper I wrote, although he always claimed he had seen a signal (I set a limit on the process).

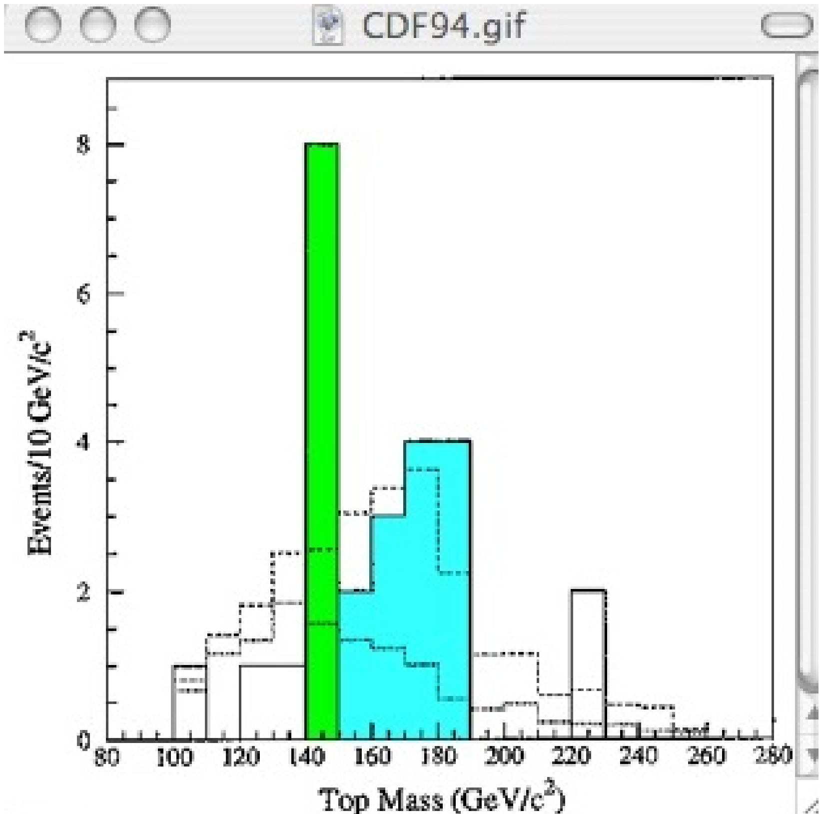
D0 data from Run I is, however, public, or so I am told. However, it is tough to make anything of it without a detailed understanding of the detector, the data acquisition, and suitable monte carlo simulations. ...".

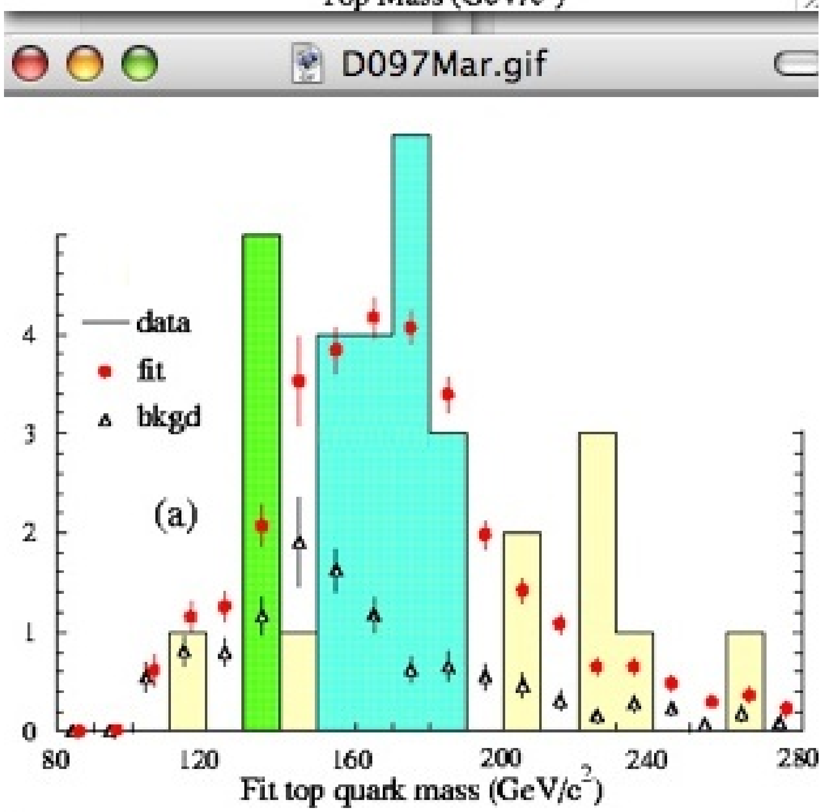
I then said:

"... there will never be a "publication in support" of my model because of blacklisting,

even though it is a model in which the Higgs as a TTbar condensate leads naturally to the Tquark, through variants of NJL models (work of Yamawaki et al), being not a simple single-state system like lighter quarks, but with mass states including around 140 GeV and 174 GeV. ...

about old CDF and D0 semileptonic histograms:





With respect to the CDF figure ... (colored by me with blue for the peak around 174 GeV) ... do you agree with CDF that the green peak around 140 GeV is "a statistical fluctuation" ?

With respect to the D0 figure ... (colored by me with blue for the peak around 174 GeV) ... do you think that its green peak around 140 GeV is also a statistical fluctuation ?

> **If they are both statistical fluctuations, then what are the odds of such large fluctuations showing up at the same energy level in two totally independent sets of data ? ...**

Tommaso Dorigo replied:

> "... As for the two plots you mention, they show single bin fluctuations at 140 GeV. I read 8 events in the CDF one, in the face of a background plus signal totalling probably 2.5, and 5 in the D0 one, with about 2 from bgr+"standard" ttbar. You ask what is the probability of such a fluctuation, and that I can answer.
>
> **It is of the order of 4-sigma.**

[To parapharse Tommaso Dorigo, in his 11 October 2007 blog entry "Single top: new results from CDF!":

WHATSA 4-SIGMA ?

In that blog entry, Tommaso Dorigo explained, saying:

"... To make a simple example, suppose you count events with certain characteristics in a given dataset, expecting to see 100 from known background sources. You see 130: that is a surplus of +30 events, which is unlikely to be due to a fluctuation in the sample size. Usually, event counts follow Poisson statistics, which basically says that the variance of the 100 events is nothing else than , i.e. 10. A Poisson distribution centered at 100 with a width of 10 is basically a gaussian function, which dies out quickly as you move away from 100 on either side. How quickly ? Well, you expect 68% of the distribution to be contained in the [90,110] interval - "1-sigma"; 97% to be within [80-120] - "2-sigma"; and 99.7% to be within [70-130] - a "three-sigma" inteval. ... **a "three-sigma" or larger effect is usually called "evidence" by particle physicists looking for a particle decay signal**. It means the data really fights with the interpretation of containing only background processes you have already accounted for when you estimated your central value (100 in the case above). A separate word is reserved for "five-sigma" effects, which are a really tiny probability of being due to accidental background fluctuations: in that case the effect is called "observation" of the sought particle. ...".]

> However, one would be entitled to claim a 4-sigma effect only if one observed the data after predicting the location of the excess beforehand. In other words, a 13-over-4.5 event excess is less significant if it is allowed to sit anywhere in a plot.
>
> But maybe you had predicted the top at 140 before the CDF plot of 1994 came out, so that is not a concern.
>
> My own concern as an experimentalist is that such a spike is not physical, given the mass resolution of CDF and D0 on a top quark decay. The top has a resolution of about 25 GeV,

> so in any one 10 GeV bin there cannot be more than 20% or so of the total. That spike may be very unlikely as a fluctuation, but it is even more so as a signal.
>
> To be clear, there can be no effect that makes a signal significantly narrower than what it is expected. One would have to hypothesize that all jets in those events fluctuated to neutral pions (measured better), and still the resolution would be far, far larger than the 5-10 GeV necessary to at least hope that a significant portion of the signal falls in a single bin. ...".

I then said:

> "... Claudio Campagnari and Melissa Franklin said in hep-ex/9608003 "The Discovery of the Top Quark": "… The energy of a jet … resolution … is typically only of the order … 1.0 / sqrt(ET) (ET in GeV) … This poor resolution is due to
>
> - (i) the intrinsic large fluctuations in the response of calorimeters to hadronic showers,
> - (ii) differences in the calorimeter response between charged hadrons and electrons or photons,
> - (iii) energy loss in uninstrumented calorimeter regions …
> - (iv) energy loss due to the use of a finite cone-size in jet reconstruction, and
> - (v) overlaps between the jet and hadrons from the underlying event …".
>
> Their figure 32

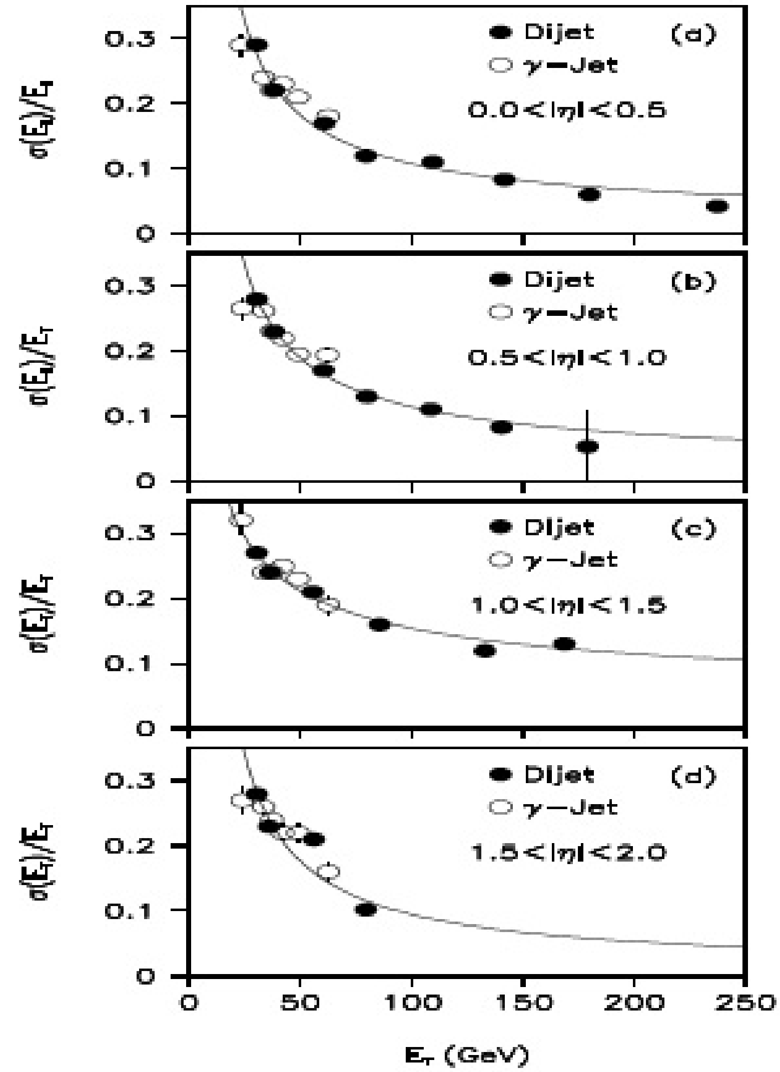


FIG. 32. From the D0 experiment, Abachi 1995d; jet energy resolution as a function of jet transverse energy (E_T) as computed from di-jet and photon-jet events in four pseudo-rapidity regions. The fits are of the form $(\sigma_E/E)^2 = (N/E)^2 + (S/\sqrt{E})^2 + C^2$. The fitted values of N, S, and C are : (a) $N = 7.07$, $S = 0.81$, $C = 0.0$; (b) $N = 6.92$, $S = 0.91$, $C = 0.0$; (c) $N = 0.0$, $S = 1.45$, $C = 0.052$; (d) $N = 8.15$, $S = 0.48$, $C = 0.0$. Jets are reconstructed using a cone-size of 0.5.

shows, for D0, "… jet energy resolution as a function of jet transverse energy (ET) …",and it shows resolution ranging from about 0.25 for ET around 30 GeV to about 0.1 for ET around 70 GeV or above, which is pretty much what you said.

However, it seems to me that the consistency of the two plots I mentioned in that they both show narrow 10-GeV peaks around 140 GeV might indicate that the CDF and D0 detectors might really in fact have had somewhat better resolution.

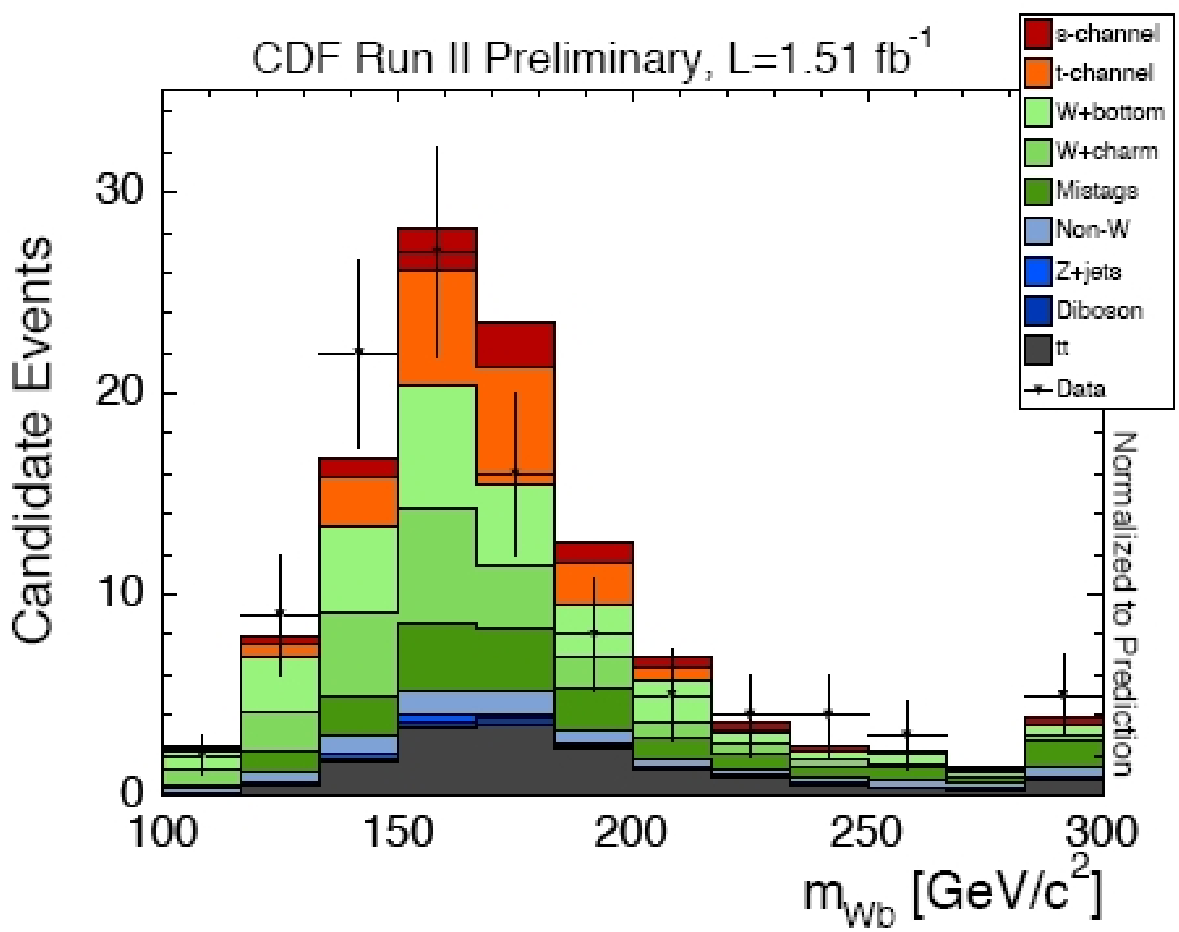
Is it possible that, in an effort to be very conservative and absolutely certain that the Tquark discovery claim be ironclad, the resolution could in fact have been better than they stated ? ...".

On 11 October 2007, Tommaso Dorigo posted a blog entry "Single top: new results from CDF!" in which he said:

"... … New technologies and more confidence in the Monte Carlo simulations of signal and background processes allow much more refined techniques. In the new and very successful CDF analysis, authored by … Florencia Canelli (now FNAL), together with Peter Dong, Rainer Wallny, and Bernd Stelzer (all from UCLA) … use is made of the matrix element of the sought process ...

single top events ... amount... to a cross section sigma(t) = 3.0^{+1.2}_{-1.1} pb ... From this measurement, it is straightforward to derive a measurement of the Cabibbo-Kobayashi-Maskawa matrix element , a number that specifies how likely it is that a W boson couple to a t and a b quark line. The cross section for single top production is in fact proportional to the square of that element. CDF finds V_tb = 1.02 +/- 0.18 +/- 0.07, where the second uncertainty is theoretical and it arises from the uncertainty in top cross section dependence on top quark mass, and other modeling details (fragmentation and renormalization scales, alpha_s value). ...

Tony Smith, in a comment below, asks for the distribution of reconstructed top quark mass of candidate events with a high value of EDT, a plot which last year caused some discussion (echoed for the D0 analysis), given that it showed some excess at 140 GeV which could fit Tony's hypothesis of a top quark at that mass value. Here is the updated plot:



For a ghost signal, I must say this 140 GeV top quark issue is hard to die… One bin up, one bin down, and there still is something to talk about! And it all started about 15 years ago… For more information you can read some details of the analysis in the conference note of the analysis. ...".

In my comment to which Tommaso Dorigo referred above, I said:

"... Tommaso said "… New technologies and more confidence in the Monte Carlo simulations of signal and background processes allow much more refined techniques. In the new and very successful CDF analysis, authored by … Florencia Canelli (now FNAL), together with Peter Dong, Rainer Wallny, and Bernd Stelzer (all from UCLA) … use is made of the matrix element of the sought process …".

Isn't this the same group that last year "… performed the first search for single top using a Matrix-Element based analysis …" with a result that Tommaso said in a 20 November 2006 blog post "… does measure a meaningful cross section - but a part of the excess of signal events clusters at low mass, indeed …" ?

- If so, does their new result also have "a part of the excess of signal events [that] clusters at low mass"?
- Are they using basically the same Matrix Element technique, and if not, what are the significant differences?
- Do they have sensitivity charts (plotted as Events v. Mass) for increasing cuts on the Event Probability Discriminant, as they had last year?
- If so, are they available on the web? ...

PS - Wasn't a competing Likelihood Function method also used last year by CDF, with a result that it found no events, thus disagreeing with the Standard Model?

Are there any newer results from the CDF Likelihood Function people?

Tommaso replied:

"... I partly answered you in an update of the post. As for the likelihood, I will report on that too in due time - but it was not "disagreeing with the SM", it was a 2-sigma-ish downward fluke to me. ...".

I then said:

"... Tommaso, thanks for the update and the link to the pdf file.

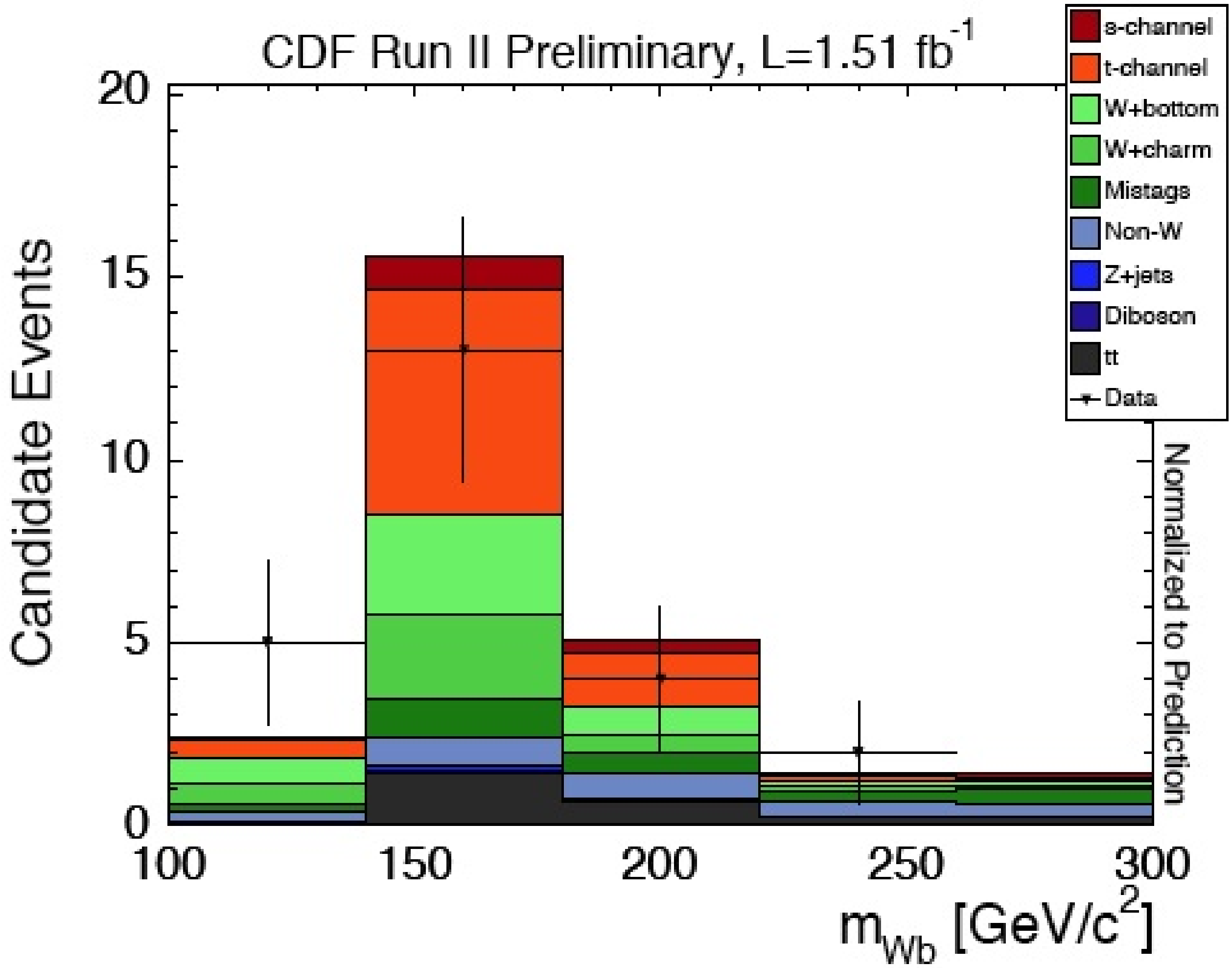
As to the Likelihood Method, sorry for using the language "disagreeing with the SM", when

> a direct quote from your November 2006 blog entry, with more context, is:
>
> > "… CDF can not measure the production of single top yet, and actually is in the awkward position of excluding its production according to the predictions of the Standard Model. … Nobody really believes that single top production is not there: it must be. It probably is just a unlucky downward fluctuation of our data. But still, it starts to be embarassing! …".
>
> I am looking forward to your "in due time" report about any new Likelihood Method results at CDF. ...".

Tommaso replied:

> "... Hi Tony, it's tough to argue with people who quote you : |)
>
> Anyway, I will let you know soon about the likelihood, I think there is a blessed result out on that too. ...".

The "conference note of the analysis" mentioned by Tommaso Dorigo contains not only the figure shown above that "... includes the last three bins of the EPD discriminant (EPD > 0.9) ..." but also this figure



that "... incliudes the last bin of the EPD discriminant (EPD > 0.966) ...".

So, it seems that the new 2007 CDF Matrix Element analysis still shows "a part of the excess of signal events [that] clusters at low mass", and, as Tommaso Dorigo said,

"... this 140 GeV top quark issue is hard to die… And it all started about 15 years ago…".

Even though over 10 years ago it appeared to have 4-sigma significance in CDF and D0 Tquark event histograms, and has appeared repeatedly in Fermilab data since then (up to and including the updated analyses described on this web page), I have been blacklisted from posting such ideas on the Cornell arXiv,

and it seems unlikely that detailed search for the truth about the Truth Quark

will be undertaken with respect to CDF or D0 Run II data, or LHC data, because, as Tommaso Dorigo said above,

"... the theoretical motivation for the search is wanting, given the lack of any publication in support ..."

and

the Cornell arXiv blacklisting prevents any publication of my theoretical model that would support the search.

Such a Catch-22 situation is frustrating to me, as is the Kafka-esque blacklisting of me by the Cornell arXiv, with respect to which they said that I am in "... a large pool here ... typically flagged by reader complaints ...", but refuse to

tell me who has complained about me, or

tell me exactly what complaint they have about me, or

give me a reasonable opportunity to reply to any such complaints.

Tony Smith's Home Page

......