Dear Mr. Garisto,

We have resubmitted our manuscript LF10513, “Search for new physics using high mass tau pairs from 1.96 TeV $p\bar{p}$ collisions.” This document contains the list of changes and our response to the individual referee’s comments. Our replies are preceded by a “>” on each line.

We have changed the following in the draft:

- added a phrase describing a bit more the $\pi^0$ identification
- added the PT > 1 GeV to the tau ID description
- added a mention of the unclustered em energy in the tau ID paragraph
- added a footnote to the caption of Table II describing our rounding procedure
- reformatted Table III to shorten the length of the paper

Sincerely,

John Conway
for the CDF Collaboration
This is a review of the manuscript "Search for New Physics using High Mass Tau Pairs from 1.96-TeV ppbar Collisions" by D. Acosta et al (CDF collaboration), PRL document number LF10513.

The paper describes a search for events containing a pair of taus with a large invariant mass in a new high statistics sample of data from Tevatron, the world's highest energy collider. Models of new physics which contain new particles that decay preferentially to heavy or third generation particles, and especially to taus, are very common; and so this result, which demonstrates the sensitivity to this kind of particle in the environment of a hadron collider, will be of great interest to the community. The result should be published in Physical Review Letters.

My two main comments:

1) In Table 1, you compare the expectations, which are mainly from Z->tautau, to the observations. But, you could have turned this around and used the acceptances, etc from the Z->tautau Monte Carlo with the data to measure the Z->tau tau cross section. This could then be compared to the recent measurement by D0 (Phys. Rev. D 71, 072004 (2005). I think this would at least as useful as quotes on limits on new particles are unlikely to exist.

> It is true that our control region could have been used to effectively perform a measurement of Z -> tau tau. However, we prefer to regard it as purely a control region because we do not want to presume that there is no physics signal present, since that is the aim of the analysis. We assume that since LEP has demonstrated lepton universality in Z decay, and that the measurements from both CDF and D0 of the Z production cross section using electron and muon pairs are in good agreement with the theoretical prediction, we can use the theoretical prediction to predict the number of Z events in the control region. The purpose of the control region is solely to check whether our tau ID and fake rate determination are reasonably correct. (This is in addition to the W->tau cross check.) Clearly if new physics is present in the signal region it could also possibly be in the control region, so the control test is not perfect.

> We assume what is meant by the last sentence is that “a measurement of Z -> tau tau is at least as useful as searches for new particles which are unlikely to exist”. Our position is that we do not presume to
know whether it is likely or not that new particles decaying to tau pairs exist, a priori, and we consider it to be very useful to search for them.

CDF has another paper in preparation in which a measurement of the cross section of $Z \rightarrow \tau \tau$ is performed.

2) It would have been nice to see the track multiplicity distribution in these events. While the shapes of the signal and background distributions for the $M_{\text{vis}}$ distribution shown in figure 2 are different, the difference is larger for the track multiplicity. Since the numbers in Table II can be read from Figure 3, I think the figure would be more useful/interesting. It would give the reader more confidence that the 20% uncertainty quoted for the jet faking hadrons is reasonable.

The antecedent of “these events” here is unclear; is it the control region mentioned in point 1) or the four signal events? It is true that for many years the one- and three-prong excess in the track multiplicity distribution has been the clear indicator of tau decays. However it is not clear what is meant by “the difference is larger for the track multiplicity” between (we assume) signal and the high-mass background.

This is not the case. The high mass background is roughly equally from real taus and jets faking taus. (The signal is of course real taus.)

The numbers in Table III (not Table II) can be read from Figure 3, and therefore it is true these are redundant, but we felt it important to provide the numbers for theorists who wish to interpret the results in particular models, and we felt it was important to have a graphic representation of the limits as well.

We have studied many kinematic distributions of the control region, including the track multiplicity, and observe good agreement with the predictions.

Space did not permit a complete justification for the 20% uncertainty on the background; also this has a very small effect on the final limits. The track multiplicity distribution alone does little to support or refute our 20% uncertainty on the jet fake rate; this uncertainty is very conservative, and comes directly from the fake rate determination.

I have a few questions for the authors:

Page 9, last paragraph. You discuss pi_zero candidates, but give no real description of how they are identified, apart from the phrase "identified using the CES" in the previous paragraph. Can you give some kind of identification efficiency or some more information about your pi zero reconstruction?
We now include a phrase indicating how we find the candidates. It is not straightforward to define a pi0 efficiency because it depends heavily on the presence of nearby tracks and other details of the environment. For isolated high energy pi0’s in the fiducial region, the efficiency is very good.

The important thing is that the pi0 efficiency is well-simulated; we have made several distributions that show good agreement between the pi0 efficiency in the observed data and in the simulation.

Page 9, last paragraph. To form a kind of isolation, you say you search for tracks and pi zero candidates outside the alpha cone but inside 30 degrees. What is the PT cut on the tracks (or the reconstruction threshold)? What about EM energy that is not identified as a pizero. Is this ignored in the isolation variable?

We have a 1 GeV cut on the PT of tracks, and we state that now in the paper. Also, we did have a cut on unclustered e.m. energy in the isolation and we now mention that in the paper. (Thank you for pointing this out!)

Figure 1, tau identification efficiency versus E_vis.
1) is E_vis the reconstructed or generated visible energy?
2) It would nice to have some hand-waving argument as to why this efficiency plateaus at 50%

E_vis is the generated visible energy. This is now indicated in the figure caption.

The plateau occurs due to the various energy thresholds in the tau identification cuts. If the question is instead why the plateau is at 50% and not, say, 60%, those details are beyond the scope of the Letter; suffice it to say that the efficiency is the result of all the cuts, of course.

Page 10, the paragraph starting "The main challenge...." When you use the jet data to get the jet fake probability, you must somehow review the Z->tautau events? How is this done?

We are not sure what is meant by “review the Z-> tau tau events”. The jet samples we use for the fake rate determination do have a very small contamination of real taus, for which we correct.

Figure 2. Are there any overflows, or is the highest Mvis event at 145 GeV?

The highest mass observed event is the one in the figure; there are
> a total of four events, as stated in the text and the table.

What is the approximately acceptance and efficiency times acceptance for Z->tautau in each channel?

> We have an overall efficiency times acceptance for Z/gamma* -> tau tau (where the tau pair invariant mass exceeds 30 GeV) > of 0.15%
> (0.07% e+tau, 0.05 mu+tau, and 0.02% tau+tau). (We do not have the numbers for the geometric acceptance readily available.)  Note also that this analysis is not optimized for Z -> tau tau but for much higher-mass objects decaying to tau pairs.

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Report of Referee B -- LF10513/Acosta
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This paper presents data on the search for anomalous resonant production of tau lepton pairs with large invariant mass in ppbar collisions. The paper is well written and the analysis is very thoroughly made and well supported by models. No evidence for tau pair production is observed. However, the data give upper bound on the cross section times branching ratio to tau pairs for scalar and vector particle production. I find the paper clearly written and it satisfies the validity, importance and general interest criteria of Physical Review Letters.

I have, however, one minor points that I believe would be interesting to make more clear. It is:

Table II shows the contribution from different processes to the etauh, mutauh and tauhtauh channels. Adding up the contributions to the etauh channel I get 1.1+-0.2 but the table shows 1.0+-0.2. Adding up the contributions to the tuahtauh channel I get 0.7+-0.1 but the table shows 0.6+-0.1. Is this a mistake or are the numbers wrong. Other numbers like the total Z/gammastar -> tau+tau- and the total expected are also affected. I suggest that the authors correct the table or explain the discrepancies.

> Of course our numbers are known with greater numbers of digits than shown in the table, and in fact when we have a case like 0.54 + 0.63 = 1.17, we end up in the table with 0.5, 0.6, and 1.2 after rounding. In short, we round the numbers after adding, not before, to maintain accuracy. We add a footnote to this effect in the table caption.

I strongly recommend the paper for publication in Physical Review Letters. The paper does not have to be sent back to me before publication.
On behalf of the CDF collaboration we would like to thank both referees for their time and effort in reviewing this Letter.