Answers to the comments by PRL referees on the draft

"Measurement of the Inclusive Jet Cross Section using the kt algorithm in pp-bar collisions at sqrt(s)=1.96 TeV"

Report of Referee A -- LM10675/Abulencia

This paper presents a careful and detailed analysis of the CDF jet cross section using the Kt algorithm. My main criticism is that it introduces the Kt algorithm as something new, but fails to explain the advantages over other techniques.

The introduction mentions that the jet cross section is sensitive to non standard model physics, but this is never mentioned again in the text. Since there are no limits quoted, what are we to conclude about this sensitivity? Does use of the Kt algorithm make this less sensitive than previous jet cross section measurements?

Thanks for the comment. The sentence was a very general statement and meant to address the fact that the tail of the cross section probes distances down to 10^-19 m. We have understood referee´s point and removed the sentence. The sensitivity of the tail of the pt distribution to the presence of new physics does not depend on the algorithm employed.

The Kt algorithm is used in ee and ep collider results but the text does not explain why we should believe it is appropriate for p-barp where there is a larger underlying event. It also mentions that the Kt algorithm does not require the introduction of additional parameters, but doesn't say which additional parameters are avoided. The D parameter is certainly similar to a cone radius. Does the Kt algorithm improve the comparison between data and theory? Is the comparison more precise? The introduction of the Chad correction factor to the theory seems at some level like another parameter and the uncertainty in Chad seems like a drawback.

The D parameter in the kt algorithm is analogous to R in cone-based algorithms, in the sense that approximately controls the transverse size of the jet. However, the D parameter can be naturally introduced in the theory thanks to the fact that the kt algorithm does not include experimental-based merging/splitting prescriptions of overlapping cones that cannot be followed by parton-level calculations. When using a cone-based algorithm, one needs to introduce an additional parameter (Rsep) to emulate this experimental prescription at NLO. In this sense, the kt algorithm provides a better defined and more direct comparison to the theory. We have slightly modified the text of the introduction to explicitly mention this. In addition, inside the quoted references the reader will be able to find detailed explanations on the subject.

The introduction of a C-had correction factor is independent of the algorithm employed (it is not an artifact of using kt instead of cone-based algorithms) and accounts for the fact that pQCD calculations cannot include non-perturbative effects from remnant-remnant interactions and fragmentation into hadrons.

One of the main motivations of this study is to show that the kt algorithm is suitable for hadron-hadron colliders and that the underlying event effect on the measurement can be controlled and is well understood.

On page 12 the explanation of Chad is not clear. Which interactions were turned off: the interactions between the proton and antiprotons remnants, or the interaction between the remnants and the fragmentation, or the fragmentation? How much is due to fragmentation and how much is due to the proton-antiproton remnants?

As mentioned in the paper we turned off both: the interaction between remnants as well as the string fragmentation. At low pt, the effect of turning off the underlying event (but not the fragmentation) would translate into a C_had correction factor of 1.28. If, in addition, one turns off the string fragmentation the C_had value becomes 1.2. In principle, one cannot quote a separate value from turning off each process separately since to some extent fragmentation effects depend on the amount of underlying event considered.

Given the limitations in space, a complete discussion on this matter is not possible in the PRL, but we have introduced in the text additional words to avoid possible confusion.

In figure 2 the inset shows a difference in shape between the data and the theory. The text should address this. The ranges of systematic uncertainties are quoted in the text and it is stated that they are considered to be fully correlated in pt. The implication is that the shapes are rather smoothly varying, which could imply that such a difference in shape might not be compatible.

As stated in the paper, the different sources of systematic uncertainty (Jet Energy Scale, Jet Energy Resolution, parton-to-hadron level correction...) are considered independent: each is allowed to fluctuate independently of the others. The chi2 study then assumed that each single source of systematic uncertainty is fully correlated across the pt bins. The obtained Chi2 probability therefore accounts for the correlations between bins and in fact proves that the data is compatible with the nominal pQCD prediction.

For your information, the chi2 is written following standards as:

$$\chi 2 = \Sigma_i \frac{(data_i - th(i,\mu))^2}{(\sigma\_data^2 + \sigma\_th^2)} + \Sigma_j \sigma\_sys(\mu\_j)$$

where:

* i runs over the p_T^jet bins
* j runs over the different sources of systematic uncertainty
* μ_j indicates by how much the systematic uncertainty j is fluctuated. Each μ_j is normalized so its probability density function is a Gaussian (centered at 0) of variance 1.

Of course is impossible to have a detailed discussion on the paper. We have rephrased the sentence with the aim to make it clearer. Given the level of systematic uncertainty in the measurement and the quoted chi2 probability, the shape in the data should not be a concern.

Although this data is at a higher center of mass energy, a statement addressing the compatibility with previous CDF results and with D0 results would be appropriate.

As the referee mentioned a direct comparison with Run I is not possible due to the difference in center-of-mass energy which translates into a significantly different pt spectrum. The only possible comparison would be made indirectly via the agreement observed with NLO predictions, using the same PDFs sets. In addition, one should address *in detail* the differences in algorithms and treatment of non-perturbative corrections between Run I and Run II results. We feel this cannot be addressed by a single statement and therefore is out of the scope of the paper.

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Report of Referee B -- LM10675/Abulencia

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This manuscript, LM10675, reports measurements by the CDF Collaboration of the high transverse momentum (pT) jet spectrum at the highest energy center-of-mass currently accessible in an accelerator experiment. This extends the pT range by more than 150 GeV compared with previous results. In doing so, it extends the cross-section sensitivity by more than an order of magnitude. In addition to presenting the data with good statistical precision, the authors present a careful study of the systematic uncertainties associated with measuring the jet cross-section over 8 decades. While there are a few issues of presentation which should be addressed, I recommend that PRL accept this paper for publication.

The authors might improve the value of this paper by addressing all, or some, of the following issues.

On line 10 of page 8, the text says that, "Similar measurements have been carried out using cone-based jet algorithms in Run II [7]." Reference [7] does not exist. It is a pointer to "in preparation". This provides zero useful information to even the best-informed reader. If there is no publicly accessible document, the reference should be dropped. If there is a publicly accessible document, at least for the definition of "cone-based jet algorithm", it should be used. PRL readers should not be expected to know the difference between a kT algorithm and a cone-based algorithm. It should be explained, or a useful reference provided.

Thanks for the comments. At this moment a valuable reference on this study exists and it is included in the new draft (now reference [6]). A detailed discussion about difference between kt and cone-based algorithms can be found in reference [5].

Reference 26 to the thesis by Olga Norniella is marginally more useful as one may hope that it becomes publicly accessible in the near future. The "CDF Graduate Student Theses in Progress" web page at http://www-cdf.fnal.gov/grads/thesis_progress.html says that this thesis was expected in December 2005. Unfortunately, the "Completed CDF Theses Page" at http://www-cdf.fnal.gov/grads/thesis_complete.html lacks an entry for this thesis. A quick search of the web using SPIRES and Google also fails to find it. References which cannot be accessed do not provide useful information, only frustration for the diligent reader. Also, the name of the thesis author's affiliation (the University of Barcelona) does not accord with "Universitat Autonoma de Barcelona" used in the list of institutions on this paper or with the English translation "Autonomous University of Barcelona" which has been used in previous papers. If I remember correctly, the "Universitat de Barcelona" and "Universitat Autonomous de Barcelona" are two distinct institutions.

The thesis we refer to is still in preparation but we are confident it should be ready rather soon at this point. The authors have made the necessary changes and corrections in reference (now [25]).

The presentation of the data using bin-wide horizontal "error bars" in Figure 1 and pT ranges in Table 1 is not wrong. However, there are subtleties when fitting distributions where the function is dropping rapidly across a single bin. Table V in the authors' reference [10] provides a table similar to Table 1 in this manuscript, but presents nominal values of ET at which the cross-sections are presented. Having similar values of pT associated with each bin of Table 1 might be useful. If this is to be done, some of subtleties related to determining the nominal values are discussed in,

"Where to stick your data points: The treatment of measurements within wide bins" by Lafferty and Wyatt in NIM, A355 (1995) 541.

We agree with the referee this is a very delicate point. After careful considerations the collaboration decided to quote the cross section “as it is measured” since providing an average pt value for a given bin introduces a model dependency on the given pt spectrum inside the bin. Given the dramatic dependence of the measured cross section on pt, this could then lead to future misleading comparisons with theoretical curves.

The first full paragraph on page 13 has a single sentence discussion of "a chi2 test" which give a "chi2 probability" of 56%. Interpreting this comment is very difficult because so little information is given. I had hoped to find a further discussion in reference [15]. As noted above, this is not yet publicly available. Absent a pointer to a fuller discussion of this chi2 test elsewhere, perhaps the authors could expand a bit here.

Thanks for the comment, please note our answer above concerning reference [25] in the new draft. In addition, a second publication is being prepared with an extended discussion on the subject.

As stated in the paper, the different sources of systematic uncertainty (Jet Energy Scale, Jet Energy Resolution, parton-to-hadron level correction...) are considered independent: each is allowed to fluctuate independently of the others. The chi2 study then assumed that each single source of systematic uncertainty is fully correlated across the pt bins. The obtained Chi2 probability therefore accounts for the correlations between bins and in fact proves that the data is compatible with the nominal pQCD prediction.

For your information, the chi2 is written following standards as:

$$\chi 2 = \Sigma_i \frac{(\mathrm{data}_i - \mathrm{th}(i,\mu))^2}{(\sigma_{\mathrm{data}}^2 + \sigma_{\mathrm{th}}^2)} + \Sigma_j \sigma_{\mathrm{sys}}(\mu_j)$$

where:

* i runs over the p_T^{jet} bins
* j runs over the different sources of systematic uncertainty
* μ_j indicates by how much the systematic uncertainty j is fluctuated. Each μ_j is normalized so its probability density function is a Gaussian (centered at 0) of variance 1.

Of course is impossible to have a detailed discussion on the paper. We have rephrased the sentence with the aim to make it clearer.

The use of language in the abstract is not as precise as it might be, and not may not be as accessible to the general reader as possible. The expression "measurement of inclusive jet cross section" may be mis-read in a number of ways. It could be interpreted to mean the integrated cross section in the (rapidity,pT) range given. It could be interpreted to mean the doubly differential cross section in these two variables. It could mean the differential cross section in pT integrated over the rapidity range given. Also, for a general audience (the PRL reader who is not an expert on jet analyses at hadron colliders), saying "Jets are reconstructed using the kT algorithm" in the abstract is jargon which provides no useful information. In fact, even in the introduction it would be useful to provide a forward pointer indicating that the algorithm will be discussed in detail later in the paper so the reader does not think it necessary to find the referenced papers.

Thanks for the comments. We have removed the mention to the kt algorithm in the abstract following referee´s comment. We have also avoided possible confusion in the definition of the cross section we measured.

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Report of Referee C -- LM10675/Abulencia

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General comment:
This paper is novel and should be published in PRL. However, the authors do not help the reader in determining what makes the result interesting or unique. The first section should be expanded to explain for the non-specialist why the kt algorithm is important, why it is challenging in a hadron collider environment and the technical improvements made by the group to make it a desirable alternative to cone jets.

The introduction makes the important points:

**** kt is the standard algorithm in e+e- and ep, it has substantial advantages over cone jets as the definition of a jet requires fewer parameters and those parameters are more theoretically tractable.***

**** This work also corrects for initial state radiation and fragmentation and after those corrections perturbative QCD calculations can reproduce the observed distribution.***

**** These data have higher statistics and higher energy than any previous measurement***

Additional points that could be made in the introduction to emphasize the importance of the result would be:

**** in hadron collider experiments there are substantial additional experimental challenges due to initial state radiation and multiple interactions in the same beam crossing.***

**** This work finds that the effects of multiple interactions can be compensated by a simple correction based on the number of vertices.***

The first section should be reworked to make these points more clearly, at the expense perhaps of details of the CDF detector and the general kt algorithm, which have been published before. As it is written, one has to be quite current on jets at hadron colliders to appreciate the full impact of this paper.

Otherwise, the technical presentation of the result in the rest of the paper is well done.

The authors appreciate referee´s comment on the introduction part. We have slightly modified the introduction along the lines of referee´s comments. However, we have limited the discussion according to the available space since we believe the description of the detector is already reduced to the minimum. It is important for the paper to maintain a precise description of the jet search algorithm employed, since it is an essential part for the definition of the measurement.

One additional note about nomenclature: the non-perturbative correction refers to the underlying event (interaction between proton and antiproton remnants) and string fragmentation into hadrons. No initial-state radiation subtraction has been made since this is included (to a given order) in the NLO pQCD calculation.

Specific comments:

**** Abstract: It would be better to state that NLO QCD is agreement with the measured cross section rather than the other way around. The measurement is the measurement and it is NLO QCD that is being tested, not the quality of the data.***

Thanks for the comments. We have followed referee´s advice.

**** define Run II and CDF II***

We have explored the solution adopted by previous CDF publications on this. We decided to avoid the use of CDF II but we consider Run II needs no special explanation.

**** First paragraph, last two sentences, "A proper .... "marginal" ...". This makes neither result look good, it implies that the development of the technique for making the hadronization correction described in this paper and reference [7] is obvious, when it is in fact innovative, and also implies that the previous data [10] from Run I was erroneous in some way.***

The first use of the kt algorithm at the Tevatron was reference 10. In that paper, the kt algorithm is compared to NLO QCD without initial state/fragmentation corrections. NLO QCD alone was found to differ substantially at low pt, this was noted in the paper and is a flaw in the theoretical understanding at the time, not the experimental result. The present authors have found a explanation for this discrepancy, which is one of the major reasons this paper deserves publication in PRL.

A suggested rewording of the last two sentences would be.

NLO QCD did not agree with a previous measurement [with lower energy and statistics] from the Tevatron [10] at low pt values. We find that the inclusion in the theoretical prediction of corrections for non-perturbative contributions removes this discrepancy.

The authors put special care in wording "marginal" as it was used by the original reference. However, we appreciate the sense implicit in referee´s comment and we revisited these two sentences following the spirit.

3) Page 12

Reference [7] breaks down the non-perturbative correction for the cone algorithm into hadronization and the proton-antiproton fragment interactions. What are the relative sizes of these factors for the kt algorithm?

At low pt, the effect of turning off the underlying event but not the fragmentation would translate into a C_had correction factor of 1.28. If, in addition, one turns off the string fragmentation the C_had value becomes 1.2. In principle, one cannot quote a separate value from turning off each process separately since to some extent fragmentation effects depend on the amount of underlying event considered.

In particular, hadronization (but not beam interaction effects) is also present in e+e- interactions, do the corrections seen at the Tevatron agree with those expected from e+e- and if not, is there an explanation? If they do agree, this gives added strength to the argument for the hadronization correction.

A proper comparison with e+e- fragmentation effects cannot be made since the underlying event and the fragmentation effects are, in principle, not independent. Of course, one can argue that the agreement with e+e- is somehow guarantee since to large extent string fragmentation parameters in the Monte Carlo have been tuned to e+e- data. A closer comparison can be made with results from resolved photo-production at HERA, where we observed similar fragmentation correction factors.

Is it possible to compare these results with the cone results in [7]?

This has been discussed inside the collaboration. A naïve direct comparison is not possible since one has to address correlations between measurements. In addition, a detailed discussion on the differences between algorithms and the effect on both data and NLO predictions would need to be considered. Therefore we feel this is out of the scope of the paper and in fact it will be subject of a future publication.

Page 13

The summary should reecho the improvements in the kt method made in this work and highlight their implications for future experiments, for example the LHC.

We consider that the summary already includes a clear statement on the importance of non-perturbative corrections. After careful considerations, we decided that no mention on implications for future experiments (LHC) should be included. We feel the conclusions in the paper should not speculate about the validity and future applications of the methods used in this letter for different hadron-hadron environments.