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Title: Differential top-antitop cross-section measurements as a function of o
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We appreciate very much the detailed comments to our paper and the interest the referee has expressed in our measurements. We have tried to improve the text where appropriate and have answered the suggestions of the referee in a detailed way. The text of the present paper is the result of a long process in ATLAS. In particular some detailed sentences have been agreed on after some discussion within the collaboration and we would like to keep them even if they make the reading more demanding.

Referee report

The paper reports the measurement of differential cross section for top pair production at 7 TeV. A novel approach is used, consisting in measuring the cross section as a function of the kinematic properties of a ”pseudo-top” quark, reconstructed from jets, lepton and missing transverse energy in the event. Unlike observables based on kinematic fits, these observables are easy to implement, both at detector and generator level. This provides an important simplification for the community of theorists and experimentalists working on top quarks. This new approach is valuable and the paper has clearly the relevance needed to be published on JHEP.

The physics content is solid. Only a few substantial comments (listed below) emerged from reading the draft. The main issue with the current version of the paper is in the presentation. While the core of the paper is well written, the first part of the paper (abstract and first two sections) is difficult to read and excessively long.
I suggest the authors to consider rewriting it, trying to use shorter and more clear sentences.

**The presentation of the first part of the paper is the result of the wish within the ATLAS collaboration to present this new idea in a detailed way and to clearly explain the advantage with respect to previous measurements.*
We have added some clarifications in the text, but we would like to keep the detailed arguments even though it might make the reading more demanding. We have

shortened the abstract as requested and added some clarification in the introduction and section 2 that should make the purpose of this measurement more clear.

As an example, the pseudo-top reconstruction is mentioned at least three times without being actually explained. The reconstruction algorithm should be described as early as possible.

**The details of the top reconstruction algorithm are not the most important point. The main idea is to use an operational definition based on the truth particle. This is mentioned early on in the paper.*

Similarly, it cannot take four lines of text to say that semileptonic $t\bar{t}$ events are considered.

**This is a matter of taste and it depends on how it is assumed what the reader should know. In this paragraph we assume little knowledge of the reader and profit from the following:*

- *that the semi-leptonic ttbar decay is used*
- *that the treatment of electrons and muons from tau decays needs to be specified*
- *that we should introduce the main event signature*

We think that this is appropriate for the introduction.

Finally, the motivation for using the pseudo-top should be made more explicit. The advantage is not as evident as it should be. The key ingredient (the simplicity of the algorithm, with respect to other more complicated choices) should be discussed more directly.

**The main advantage of the pseudo-top idea is that the observable definition is based on stable particles. This idea is mentioned as the first point in the fourth paragraph where the idea is introduced. The simplicity of the algorithm is actually not the main point. In fact one could use any algorithm and we also do not claim that we have found the optimal one. A simple algorithm helps with the implementation, but this is not a real problem, if people are using Rivet. One could even consider to implement W mass constraints, a comparison of the leptonic and hadronic top masses or similar to improve the signal efficiency. This would slow down the comparison and make it more complex, but would not invalidate the approach.*

Nevertheless, we see your point that we should also discuss the role of the performance in the top reconstruction. We added now in the introduction of section 2:

"This operational definition should result in a good correlation between the pseudo-top object and the top parton for a given Monte Carlo generator. However, it is not necessary that the algorithms lead to the best possible correlation to the parton-level kinematics or the best signal to background ratio."

This was already mentioned in the draft at the end of section 2.3, but it is appropriate to include it also in the introduction of section 2.

We also make it more explicit in section 2 that we are dealing with studying QCD, i.e. the ttbar production mechanism by adding:
"...ongoing concern for QCD studies in particular for events with top quarks."

Physics comments:

- The algorithm defining the pseudo-tops is based on a set of arbitrary choices: (i) the decision of pairing the lepton to the closest b-jet in ΔR; (ii) the way multiple solutions are handled when computing the neutrino pz; (iii) the strategy chosen to avoid the problem of complex solutions for the second order equation for the neutrino pz calculation. As any arbitrary choice, some justification should be given.

**We mention at the end of section 2.3 that it is not so important to use the algorithm with the best performance. Therefore a detailed discussion of the choices is outside the scope of the paper. It is important is that we exactly define how the variable is constructed. The choices have been made after some optimisation to get a good correlation to the top parton.*

The studies are summarized in section 5.3.2 of a recent thesis:
http://dpnc.unige.ch/THESES/THESE_WATSON.pdf
Since the signal efficiency is not the main point, we do not wish to mention this in the paper and rather leave this to a dedicated paper on an optimal top reconstruction algorithm where operational definitions can be optimised and compared to kinematic fitters.

Moreover, the implications on the top reconstruction (e.g. the match to the top quarks) should be described.

- The presented algorithm has some similarity with previously used algorithms of $t\bar{t}$ reconstruction. The derivation of the neutrino pz is very similar to what described in http://arxiv.org/pdf/1112.5100v2.pdf while the association of the $W \rightarrow l\nu$ candidate to a b-jet (to form the top candidate) is much simpler in this study. In particular, generator- level information is not used in this paper. A direct comparison of the method with previously used methods is mandatory, in order to show the novelty of the approach.

**The optimal kinematic top reconstruction algorithm (in the resolved regime) is an evolving topic with a long history that started already at the Tevatron. Actually most (all) studies are documented at most in theses and the publications usually just state what is done for the measurement. For instance, the treatment of the longitudinal momentum of the neutrino was already discussed by Yuan in 1989 (ANL-HEP-PR-89-44).*
We therefore refrain to give references for the various ingredients that are used in the pseudo-top definition.

The b-jet associations mentioned in arxiv:1112.5100 make extensive use of probability densities exploiting the mass constraints.

The method in the mentioned publication uses a MC model input to find the best ttbar combination and is therefore likely to give a better performance, but it would make more difficult to use as pseudo-top definition since the probability density used to define the likelihood would need to be published together with the data points.

- On Fig.2, one can appreciate the existence of two populations in the plots. One population is strongly correlated, the other looks basically uncorrelated. The color code in the plot does not allow to appreciate in a precise way the relative size of the two components. Given the fact that the z axis has a linear scale and that the second component is distributed on more bins, the two components might be closer in size than what the text implies (the authors refer to a ”strong correlation” in the text, which is strictly speaking correct only for the first component). This looks like a contradiction.

**We have looked at the difference between the particle and parton level and the difference between the reconstructed and the particle level in a simple 1-dim histogram. We define the highly correlated peak region with +25 and -25 GeV*

around 0 which corresponds to about 2 sigma of the pseudo-top resolution for the case where there is a geometrical match between the top parton and the pseudo-top.
In this region we have about 50% of the events. Correspondingly this means that outside this region we also have 50% of the events. Please, note that all events enter and there are no fiducial selections applied.

The correlation between the detector and particle level is much better.
We agree that we need to more specific on the top reconstruction rather than just saying strong correlation and add the following sentence in the paper:
"About 50% of the events show a strong correlation between the top quark parton and the particle-level pseudo-top. These are the cases the correct combination of the final state object was found to reconstruct the particle-level pseudo-top"

Please, note that the quality of top reconstruction is not critical in this analysis, since the background is rather low such that a meaningful data to MC comparison can be made. This we also make now clearer in the text.

We also took out the word "strongly" when we talk about the parton- to particle-level correlations in the conclusions and introduction.

- As mentioned in the text, $t\bar{t}$ MC samples used for the study are generated assuming mtop = 172.5 GeV. The cross section for $t\bar{t}$ production is computed for the same mass value. What is the impact of fixing a mass value? What happens if this value is changed? Without this information, assuming a mass value sounds like a circular argument, at least for those interested to bound the top mass from the production cross section. Interesting cancellations and biases could happen between the cross section and mass measurements, as described for instance in http://arxiv.org/pdf/1410.7025.pdf.

**For data to MC comparisons the important part is that the top mass is fixed, i.e. the pseudo-top cross sections can strictly speaking be only valid for a MC prediction that is using a top mass value of 172.5 GeV. For a meaningful data comparison the top mass value can not be changed. This is not a circular argument, but a limitation of the present measurement. It also implies that the top mass can not be determined from these cross-section measurements. For this the dependence of the acceptance on the top mass would need to be provided.*

We have added a sentence in Section 9 that the data to MC comparison with a MC generated with a top mass of 172.5 GeV. Nevertheless we have investigated how the
cross sections depend on the top mass using Monte Carlo samples generated between 165 and 178 GeV and repeated for each sample the corrections for detector effects. The dependence on the top mass is linear and it is sufficient to quote a slope. The dependence on the top mass is rather small about -0.1-0.3%/GeV for most cross-sections. We have added two sentences how each cross section depends on the top mass.

- On page 14, the authors describe a set of scale factors used to convert the W+2jets event yield with b quarks in the corresponding W+jets yields in the selected sample. It is not clear from the text if these scale factors are inclusive, or if they are measured separately for different bins of the reference variable (pT , η, etc), as a function of which the differential cross section is given. To have a bin-dependent scale factor, one would need to reconstruct pseudo-tops in the W+2jets sample. But one cannot define a hadronic pseudo-top using only one jet. Are then these scale factors only normalization factors for the W+jets, while the shape is taken from MC? A more clear explanation of the scale-factor usage would be helpful.

  *The scale factors are simple numbers that change the normalisation of the individual W+jets process in the MC. They are calculated per process like the (W+2jets) process in Alpgen. The pseudo-top observables do not enter in the background estimation.

  We clarified in the text as follows:
  After the sentence “The values of NW+ + NW- are independently determined for W+4 jets and W+5 jets events.” and continue “and are applied as inclusive scale factors to the Alpgen W+jets processes.”

  For the W+bbar process the existing sentence should then be clear:
  “As a result the Wbbar+jets process is scaled up by…”

- In Fig.3, the data/prediction ratio is often larger than 1 on the tail of the pseudo-jet pT. To understand if this trend is covered by the quoted systematic uncertainty, one would have to know which fraction of the quoted uncertainty originates from bin-by-bin correlated effects. Could the authors clarify this point?

In Fig 3 we compare the reconstructed distributions. The main purpose of this figure is to show that the MC is roughly able to describe the data. Deviations in shape are not a problem, since we use a matrix-based unfolding procedure that is able to get the correct truth level even if the MC used in the unfolding has a different shape from the one used for testing (or data). Such tests are performed, e.g. when the systematic uncertainties are evaluated, as for instance Powheg+Pythia and Powheg+Herwig give different pt-shapes as can be seen in Fig.11. Actually for the fully corrected cross-section measurement the bin-to-bin correlated effects can be taken into account since detailed tables with all systematic uncertainties are provided on HepData.

- Looking at Fig.8, it is not clear to me why the single-pseudo-top variables are better predicted than the pseudo-$t\bar{t}$ pair. Could the authors discuss further their statement at the bottom of Page 28?

  Yes, we have taken out this sentence.

Style comments:

- The abstract is too long. The authors should reduce the amount of details given at this early stage of the paper.

We have reduced it by 5 lines by removing some of the details but without loosing the main content. Thank you for the suggestion.

"Various differential cross-sections are measured in top-quark pair ($t\bar{t}$) events produced in proton–proton collisions at a centre-of-mass energy of $\sqrt{s}$ = 7 TeV at the LHC with the ATLAS detector. These differential cross-sections are presented in a data set corresponding to an integrated luminosity of 4.6 fb^{-1} . The differential cross-sections are presented in terms of kinematic variables, such as momentum, rapidity and invariant mass, of a top-quark proxy referred to as the pseudo-top-quark as well as the pseudo-top-quark pair system. The dependence of the measurement on theoretical models is minimal. The measurements are performed on $t\bar{t}$ events in the lepton+jets channel, requiring exactly one charged lepton and at least four jets with at least two of them tagged as originating from a b-quark. Differential cross-section measurements of the pseudo-top-quark variables are compared with several Monte Carlo models that implement next-to-leading order or leading-order multi-leg matrix-element calculations."

- The authors introduce a symbol for QCD calculations and a pQCD symbol for perturbative QCD. But the calculations the authors refer to at the end of the first paragraph are also perturbative, aren't they? If so, the distinction looks somehow artificial and to some extent misleading.

**The abbreviation refers to perturbative QCD as such and not a specific calculation like fixed order. To avoid confusion we have removed the symbol pQCD.*

- The English is not always fluid.
  **The present text was corrected by the ATLAS english style expert reading every ATLAS publication.*
  So we are reluctant to change the text at the present stage.

  For instance, I am not sure about ”definition where” being correct.
  **This we would like to keep.*

  Similarly, ”within DR ¡ 0.1 between ...” does not read well.
  **We introduced a new sentence:*
“”The electron and muon four-momenta are defined to include any photons not originating from hadron decays that are found within dR = < 0.1 with respect to the lepton direction.”

Or, ”is as close possible” does not sound right when talking about two selections that the authors want to be as close as possible (to each other). It should be ”are as close...”.

**We were talking about “the kinematic selection of … and of … is as close as possible”. This is ok.*

Nevertheless we have changed this to:
“The kinematic selections of … and of ...are as close as possible.”

These are only examples of the fact that the text needs a deep revision, at least in the first two sections.

- The way it is currently structured, the introduction offers a solution to a problem before describing the problem. At this point, the need to use pseudo-tops (and what pseudo-tops are) is really unclear. As said already, I strongly encourage the authors to rewrite the introduction in a more clear way.

**The logic of the introduction is*
- *review previous inclusive ttbar measurements and the physics interest*
- *motivate differential ttbar cross section measurement*
- *Explain the signature and event topology*
- *Explain the pseudo-top quark idea*

We agree that it would be better to also expose the problem with parton-based measurement in a more clear way. We therefore added a sentence before introducing the pseudo-top:
"The model dependence ttbar differential cross section measurements presented at the level of top quarks, i.e. corrected for parton shower effects and hadronisation, has been an ongoing concern."

- The text could be made shorter in several places, using less words to say the same thing. For instance "the concept of ... definition" could be changed to "the definition of" without changing the meaning of the sentence at Page 3.

**We would like to keep this sentence as it is. It is really the concept that is extended and not the definition. The concept in previous measurements is to introduce fiducial cross section based on stable particles. Here we extend this concept to the kinematic properties. Actually a definition is a definition and can only be changed, but not extended.*

- The text between the beginning of Sec.2 and the beginning of the sub- section 2.1 has little or small new information. Pseudotops are referred to once more, but the definition is still not given.

**This text is an introduction that to the detailed information that is given in the subsections. Here, we review in more detail then it was possible in the paper introduction the concept of fiducial measurements and particle-based definitions. We also explain the concept of the pseudo-top and give an overview of what is coming in the subsections. A detailed definition is not needed here. We think that this paragraph is need to make the advantages of the new concept clear. A repetition of some arguments given in the introduction is intentional.*

- The authors introduce several symbols (e.g. the eta, pt, etc of the pseudo-tops). They should make the effort to use these symbols consistently (see for instance page 5 after the first block of items). Otherwise, why introducing the symbols at all?

**We have extended the sentence starting with "Once the…" and use the symbols.*

- Also the captions can be made shorter. For instance, see the caption of Fig.2 The sentence "Monte Carlo study showing the correlation..." is repeated twice in a few lines. A more compact sentence could be used.

**We have followed this suggestion and introduced a shorter caption*