-- AustinBaty - 2019-08-07

Matt Herndon

Measurement of Z boson yields and azimuthal anisotropy in 5.02 TeV PbPb collisions with CMS

Organized by line number in the AN and where appropriate with the corresponding line numbers in the paper.

35) (line 18 in the paper draft)
The discussion does not make clear whether the uncertainties are due to the assumptions about the nature of the heavy ion collision, or whether the uncertainties are due to the calculations done within the Glauber Model, or other issues. Please clarify.

The uncertainties in the Glauber model usually come from assumptions made on the nuclear structure of the ions (radius, skin depth, etc.), so one can argue that this is the same as an assumption about the nature of a heavy ion collision. I have removed 'in the Glauber model' here and just changed it to 'assumptions about the initial state of a heavy ion collision.' I hope this is less confusing.

52) (line 26 of the paper draft)
Please include a reference for the "scalar-product" analysis method which explains the methed, the various quantities that are defined and calculated and explains the physics importance of these calculations and ideas in the context of Heavy Ion physics.
After some searching: <a data-saferedirecturl="https://www.google.com/url?q=https://arxiv.org/abs/1209.2323&source=gmail&ust=156529782 href="https://arxiv.org/abs/1209.2323" rel="noreferrer" target="_blank">https://arxiv.org/abs/1209.2323 seems to be a standard and comprehensive reference.

I have included this reference where appropriate in the paper draft.

Please include the reference above or equivalent reference at this line and spend a line or two describing the physics of each of the quantities you measure. The discussion of the physics should make plane what the expectations are for Z boson production as opposed to particles that experience hydrodynamic flow.

Some discussion is given later, but this is really material that should be in the introduction.

I have slightly expanded the discussion of hydrodynamic flow a bit in the introduction. In general, the introduction was reworked a bit in order to have a better flow and to present the arguments for measuring the Z flow more clearly.

59) (in the paper starting at line 62)
There are some details of the MC generation that can be extracted from Table 2. However, you should explicitly document what MC are used, what PDFs (or equivalent nuclear distributions) and what tune of the underlying event with appropriate references.

A much greater level of detail is now given in the paper draft with appropriate references and MC versions/tunes.

68) (line 75 in the paper)
For my information.
I don't understand the origin of this bias in the MC. You are also simulating a hard process in the MC. Is there

something non-physical about the way that the MC simulates events with Z bosons that leads to an incorrect centrality distribution? Please explain.

The way hard probes are simulated in our MC is not physical, because a single hard probe is injected into a separately generated underlying event background. This means there is by definition 1 hard probe per MC event, regardless of the event centrality. However, in data, hard probes such as jets and Zs are more likely to occur when the two nuclei collide in a head-on manner, because many nucleons will collide with each other at the same time and give a higher probability of a hard scattering happening. I have tried to clarify this in the paper.

79) (line 71 in the paper)
The details of all these filters are not given. For instance pclusterCompatibilityFilter is not described beyond general terms. A reference should be given where they are described in detail.

More detail on what these filters do has been included in the paper draft.

119) (line 78 in the paper)
A reference should be given that defines the nuclear overlap function TAA

The standard HI reference for the Glauber model/TAA is now given in the paper draft.

190) (line 116 in the paper)
Typically an effect like this, especially if it is large, would be accounted for by some type of unfolding procedure. This would allow you to iterate the procedure in a controlled way to account for the fact that if the measured pT spectrum is has a different slope than the predicted one the corrections factors would be different. At the very least if you don't unfold the data it is necessary to assess the bias in the measurement and assign a systematic uncertainty, though this is not the preferred solution. To do so you would recalculated the corrections factors reweighing the MC to match the observed pT distribution in the data and compare to your current results.

We now use a matrix inversion (recommended by the stat committee) unfolding procedure to correct for this effect. The MC has also been reweighted to be more similar to the data distribution.

206) (line 123 in the paper)
In collisions involving matter, specifically protons, neutrons and no antiprotons, you would not expect the number of same sign and opposite sign pairs to necessarily be the same. For instance same sign positive pairs may be preferred However, it could be easily checked and corrected for if they are not the same by running a large QCD MC through your selection.

We checked this in a QCD sample, but found no significant difference between the opposite and sign sign yields. However, the conclusions are somewhat limited by the statistics of the high-mass pairs in this sample (only ~10 of same and opposite sign in a sample of 100k events). This procedure seems to be somewhat standard, as it has been used in the previous CMS analysis (HIN-13-004) and more recently by the ATLAS collaboration in their analysis of 2015 PbPb data. Given that this background is 1% or less, we believe the treatment here is sufficient.

Do these backgrounds have a specific shape in the variables you measure? If you are subtracting them as a function of a variable or variables you should include that information here. Otherwise you should state what you do more explicitly.

The subtraction is done as a function of whatever variable is being measured. The exact figures showing this are in the Analysis Note. We will try to clarify this in the paper draft.

Has it been checked whether W + random leptons contaminate the same sign sample and should be subtracted off or otherwise accounted for since you separately account for that background?

Examination in MC samples indicates that this contamination effect is largest for the W + jets MC background. Here, the same-sign pair yield is around 50\% (28\%) of that of the opposite-sign yield in the dielectron (dimuon) channel. This indicates that any possible contribution to the data-driven same-sign background (and therefore any oversubtraction) would be ~0.1\% or less of the total Z yield in each channel. Because this is very small compared to other uncertainties in this analysis, we have not corrected for this effect.

212) (line 129 in the paper)
You design a selection to remove EM background but you make no comment on whether there is any expectation of residual contamination after the selection. If the remaining EM background contribution is expected to be negligible you should state that and give some proof in the form of a study or reference to justify that statement. If it isn't negligible than you should estimate the contribution of the background.

We now choose working points for the EM background cut that correspond to 90% background rejection, based on studies of EM processes in the STARLIGHT MC generator. The remaining 10\% is a <0.1% contribution to the total yield.

238) (line 148 in the paper)
The procedure you describe here seems to be aimed a normalizing the MC based background to a more data driven estimate of the cross sections based on all the non data driven background. Is this procedure meant to account for issues like higher order QCD corrections, specific issues in the heavy ion collision modeling, or reconstruction issues? You should give an explanation for why you do it this way. If the reason is higher order QCD corrections then normalizing ttbar production to a Z boson production process would not seem justified given tha ttbar production is largely gluon initiated. Also, depending on what you are trying to correct for treating the electron and muon channels separately doesn't seem like a good idea unless the main effect you are correcting for is a reconstruction issue.

This is not meant to account for higher-order QCD corrections. The reason this is done this way is because one cannot simply divide the xsection by the luminosity to get the expected yield of events in heavy ions - an additional factor of the 'effective nucleon luminosity per nuclear collision' (TAA) is needed, which has its own uncertainty. Regardless, the uncertainty introduced by either method should be covered by the sizeable (20\%) uncertainty quoted for this effect. The reason it is done on a per-channel basis is because the detector resolution is different for the two channels.

I have tried to simplify the explanation in the paper to make this less confusing than it is now.

257) (163 from the paper)
You've referred to the scalar product method as both modern and well established. Perhaps it's better to describe it by discussing what biases it eliminates and remove the adjetives that only describe it in general terms.

I have removed these terms from the paper draft.

line 270) (line 175 from the paper)
You state that the Z boson reconstruction efficiencies are accounted for by applying weights in the calculation of it's Q vector. Shouldn't this also be done for the Q vectors calculated from HF and tracker activity.

It seems that maybe that this effect is taken into account by centrality calibration discussed in the next section. If so it would be good to state what corrections and procedures are used in calculating the (non Z) Q vectors

here in this section.

You are correct that this is already accounted for with a calibration. I have added short section in the data samples section specifying that the global event Q vectors are flattened and recentered (with some references to explain what that means).

line 306)
10% uncertainty seems fairly agressive for the ttbar contribution. However, that depends on how it's cross section was determined which is not well described since the MCs are not described in detail

This has been changed to 20%. These backgrounds are very tiny compared to the total yield in the analysis, and this uncertainty will remain negligible unless it is a >100% variation.

Figure 24) Figure 3 in the paper)
Why do you only show the Glauber uncertainties rather than a Glauber model prediction with uncertainties.

The Glauber model iteslf is only used to calculating the value of TAA, the nuclear thickness function, in heavy ion collisions. It does not model or make any predictions about the yields of particles themselves. If one wants a prediction, the Glauber model must be convolved with some other MC generator model that calculates particle yields.

Anne-Marie Magnan

Major concern: you are considering the lepton reconstruction and identification efficiencies as uncorrelated between the two leptons of the same type. To my understanding, when estimating systematic effects in the tag&probe measurements, we vary all events up and down in a correlated way. There is indeed a part of the systematics which comes from the "pass/fail" fit and that would make individual pT/eta bins independent. But I believe it is very difficult to separate the different contributions and in the end it is more conservative to take the resulting systematics on the data/MC SF varied in a correlated way for all leptons. The different sources can however be varied independently. I.e., the reconstruction eff up/down at the same time for both leptons, then independently the identification eff up and down, then independently the trigger eff up/down, and finally these 3 added in quadrature.

Given your final systematics is dominated by this lepton systematics - it could have a significant impact !

You are correct that there could be some correlations which are difficult to disentangle, so I have taken your advice and varied the *systematic* uncertainties in a correlation fashion between the two daughter leptons (but do the different sources independently). The *statistical* uncertainties come directly from the TnP fits. For these, the uncertainty should be fully correlated if the two daughters use the same fit (same TnP bin) but should be uncorrelated if they use a different fit (different TnP bin). Thus, for Reco and ID statistical uncertainties I do the variation in a correlated (uncorrelated) way if the daughters are in the same (different) TnP bin. For the trigger statistical uncertainties I just assume correlated to be conservative, as this uncertainty is negligible compared to other sources. The net effect of this change is that the uncertainties increase by around ~1% in the muon channel and ~2% in the electron channel.

Comments on AN v5

l144 TAA values taken from 2015 PbPb: can you argument a bit why you expect it would not have changed for 2018 data ?

These TAA values were updated after the pre-approval to new values calculated for 2018 data. In general, the Glauber values should only be a function of the ion species, the collision energy, and the parameters used to model the ion (radius, neutron skin depth, etc). This means they do not depend on things like the LHC run

period or detector configuration. However, the 2018 values are now slightly different than the 2015 values because the parameters used to model the ion were updated to use better estimations from some recent publications. I have simply removed the reference to the old 2015 values in the AN, and mentioned we got the new ones from the appropriate heavy ion sub-group.

l152 can you explain a little bit better what is this vertex probability and why this cut ?

The vertex probability is calculated by looking at the goodness-of-fit of the vertex created by the two daughter muons (with a vertex having a lower chi2 having a higher probability). This is a very loose cut that was found to not affect the signal distributions very much, but cuts down the same-sign background by a factor of ~2. I have added a short explanation into the AN.

Fig 16-18 wrong labels for x-axis , "mumu" instead of "ee"

fixed

l321-323 what matters is the resolution compared to the bin size....

clarified

l338 mention here that you don't have any regularisation -> by the way, this is kind of "lucky", and I wonder if you tested with small regularisation, just to check it doesn't affect the result unfolded/raw ratio much and neither the uncertainties ?? The "folding" exercise you do in appendix is kind of granted to work and is not really a closure test at all....

I tried doing a check with a iterative d'agostini (Bayes) unfolding, and the result was quite similar to the matrix inversion procedure. The only large differences are in the lowest two bins, which have large statistical uncertainties. Furthermore, the number of iterations used before stopping was not fully optimized in this cross check (this can take a very large amount of study), so it is possible that the differences could be improved by tuning the Bayes unfolding. Overall, we do not see a need to move away from the matrix inversion unfolding, given that these low-pt bins have large uncertainties.

l677 you mention the maximum pT of the measurement: how was this value decided ?

We have a few Z bosons (10-15 I believe) above 200 GeV, but the statistical uncertainty for the 140-200 GeV bin is already around 20%, so we did not believe it made sense to include another bin at higher pT.

Comments on paper v2

- you did not run a spell-checker, this is an easy way to catch easy typos ... Few lines I could catch: 95,102,108,109,126,222,268,279,308,309,338,345,

I have run a spell checker now.

- the draft reads very well up to section 5 and then it degrades quickly Trying to give more specific examples below, but generally the sentences are much more "analysis note-like" from section 5 when I felt the paper was quite well-written up to there.

- please try to banish the word "cut" , and expressions like "there is", "there are", and complicated sentences when a simple "subject-verb-complement" can do very well. More exemples again below.

27 and 201 only two places in the paper with "we" -> replace with indirect sentences.

fixed

27 pair **of** electrons

fixed

28 "compare with predictions" --> do we ? Not at the moment....

model comparisons have now need added

31-32 used **to** constrain

fixed

66-73 you have a mixture of past and present tense. Make a choice: I'd stick to present.

changed to present tense

85 twice "impact"

fixed

99 direction -> direct

fixed

109 first sample -> signal sample ?

changed

114 similar samples -> rephrase as "Same generator and settings" ?

changed

120 ME-level -> ME level

fixed

126 primary vertex's z position -> z position of the primary vertex (I believe the CMS guidelines say something like no more than 3 nouns...)

changed

146 add the overall efficiency, like you quote l161 for the electrons ?

I have added a statement about the selection efficiency and the fact that the misreconstruction is negligible.

148-150 this calls for either saying why inefficient, or just remove this as "too detailed" for the paper. You correct for the missing acceptance in the end so not strictly speaking mandatory to explain in paper ??

I have just removed this sentence. The missing acceptance is corrected for as you said.

155 Cuts -> Selection criteria

changed

169 data and MC -> data to the MC.

changed

179 criterion -> criteria

changed

180 window 60 < Mll < 120 * GeV*

fixed

182-184 these numbers do not correspond to numbers in tables 8,9 of AN, shouldn't they ?

The numbers given here are the raw counts before efficiency corrections, the ones in the table are after efficiency corrections. This is why the two channels are close to each other in the tables, but not the two figures given in the paper. I am giving the uncorrected numbers because that is what has been done in previous analyses from CMS, and the corrected numbers are in principle available in the results.

194 for detector inefficiencies -> for these detector and selection inefficiencies

changed

195 There are multiple .. that can --> Multiple backgrounds can create...leptons. These backgrounds are subtracted....

changed

Whole paragraph could benefit from better writting.

I have reworked the paragraph a bit.

199 "There are" --> rephrase.

fixed

202 equal of -> equal to

fixed

203 proxy of -> proxy for ?

fixed

205-206 rephrase with explanation why, like in the AN. One more paragraph which could benefit from better writting.

I have rewritten this paragraph

211 0.2% (1%) --> are these numbers for mumu (ee) ? To be added....

I added a phrase to clarify that this is for mumu (ee).

222 cut -> just selection

changed

224 the an -> an

fixed

225 bosons decays by --> not clear. Just "bosons is 0.3%" ??

This sentence was meant to indicate that the application of the EM-rejection cuts also lowers the signal efficiency by 0.3%. This has been clarified.

234 W bosons decaying to a single lepton being ...

fixed

240 opposite sign distribution -> opposite-sign distribution ... though should be opposite-sign events or sample .... from then on many sentences don't have the correct subject, analysis-note-like style ....

fixed. I have tried to clean up the language from this point forward.

248 efficiency -> resolution ?

fixed

252 add "hence no regularisation needed" ?

I do not think the lack of regularisation needs to be discussed here, as it could just confuse the readers with a somewhat technical detail. We say that we use a matrix inversion procedure, which implies no regularisation is applied.

254 spell out 10x

fixed

fig 1 add ratio plots, if possible with stat+syst uncertainties on backgrounds . Left should be Mee. Caption: Dilepton mass distribution...

Ratios added. I have not added systematic uncertainties because I feel that it crowds the plot and makes it more unclear. The purpose of this plot is to show the good agreement between data and the MC+background expectation, as well as to illustrate the contributions of the various backgrounds.

258 add also the number for muon eta<2.1

I have updated this to be consistent with us only showing 1 muon result (|y|<2.1).

267 at **least** three

fixed

273 For account -> To account

fixed

274 Eqn -> Eq. or I actually rather like to spell out "figure" and "equation" in full actually...

I checked the CMS rules on this, and they prefer Eq. and Fig. rather than spelling it out fully. I have moved to this convention.

278 "There is a modeling..." -> "The modeling .... is related to..."

fixed

289 is varied -> are varied

fixed

308 remove, it repeats the previous line (or rather previous line is enough to understand what is done)

removed

312 this is 0.5% +/- 0.5% so relative uncertainty of 100%, is that correct ? Maybe confusing if it is quoted as relative like others or absolute...???

I have rephrased this to make it more clear that it is 0.5+/-0.5%

313 cut -> selection criteria

changed

313 working point*s*

changed

315 cuts -> requirements

changed

317 production -> bosons

fixed

318 The EM events -> The number of EM events ...is ...

fixed

319 cut variation -> remove or rephrase ... Another candidate paragraph for better writting.

I have rephrased this.

324 addition -> additional

fixed

326 in quadrature is -> in quadrature to

fixed

335 measurement..measured.... rephrase

fixed

335-338 fairly standard procedure, maybe no need to explain ? just quote "uncertainty from MC stat in response matrix" ??

removed this.

346-347 To combine .... combined --> rephrase

removed 'to be combined'

355 "form a single data point" --> rephrase, simpler sentence !

rephrased

374-375 remove as ATLAS pp point is gone now in fig 3 ?

removed

376, 380 Ref -> figure

changed

377 cuts ...

this sentence has been removed

378 there is ....

this sentence has been reworked.

figure captions: improve with adding what are error bars etc....

I have added more information to the captions.

384 from 2018 -> collected in 2018

fixed

386 "The rapidity and pT spectra constrain MC generators" --> first time this is mentioned, what do you mean ??

This is now discussed in the results section

388 for <50% events --> mention also that number in the results section: the conclusion should just repeat things already said before....

This is now discussed in the results section

Wei Xie

- abstract: "The yields in various centrality bins are compared to
Glauber model predictions of the production rates of hard probes not
modified by the presence of a hot medium.".

I couldn't find this prediction on the plots. Does it mean
something different?

This has been added to the figures now, it was missing previously because I had contacted the authors of the model and was waiting for some feedback.

- L19: "analysis" --> "analyses"

fixed

- L83: "The degree of overlap between the two lead ions (centrality)".
This is already defined in L9. Pick one of the definition and replace
this sentence with just "centrality".

I have kept the first instance and changed the instance here at L83.

- L90: need a brief clarification why |eta|<0.75 is chosen

- L126: "beter" --> "better"

fixed

- L179: "criterion" --> "criteria" since it refers to all selection .....

fixed

- Fig.1: what's the reason for choosing mass range of 60-120 GeV? At
least for dimuon channel, the s/B ratio is very high at mass=60 and 120
GeV/c.

This is the same mass window that was used in previous analysis (HIN-13-004), and is wide enough to contain most of the Z peak. Although you are correct that the s/B ratio at 60 GeV is still quite good, many backgrounds start to grow heavily in the low-mass region and therefore this region becomes sensitive to the shapes of these backgrounds. We do not believe the additional statistics added by opening the mass window would be worth complicating the analysis by having to validate our background subtraction in the more-challenging low-mass region.

- L185-194: related to question on Fig.1 above, is there a cuts on
mass range in the numerator or denominator when calculating the
efficiency? There certainly shouldn't be any mass range cuts on
denominator.

There is a mass range cut for the numerator (reconstructed candidates), but no mass range cut is applied for the denominator (gen Z's). Thus, we are correcting back to all Z's produced in the MC sample.

- L207: "0.5%" does it has large pT dependence? Naively the higher the
pT, the higer the chance of mis-identification

The charge swapping as a function of pt is shown in the analysis note in Fig 9, top right panel. There is no pt dependence seen for the MC sample. I believe that because this is already a relatively high pt (>20 GeV), the major effect that leads to charge-swapping is the scattering off detector material, rather than changes in the

bending radius for different pt electrons.

- Fig.1 left panel: x-axis title: --> m_{ee}

fixed

- Fig.1: Are the orange Z->ee and Z->uu from MC or from "Data - background". It seems to be the former. In that case, is the "MC Z->uu and Z->ee" or or the "Data - background" used for yields calculation. Is the MC is used, then we need a pull plot under each panel to show the quality of describing data. If the "Data - background" is used, then the "MC Z->uu and Z->ee" is not needed to be in the plot.

The orange are from MC. We use 'Data-background' to calculate the yields, but believe that the current presentation of the plot is necessary to give confidence that the expected contributions of signal MC + background reproduce the shape of data. A ratio between data and MC is now provided, and I have expanded the discussion of Figure 1 so that this is more clear.

- L306-307: need a brief description of the justification on why 20% is choosen

Checking the following references for each background should indicate that this 20% number is larger than current theoretical (at NLO) or experimental uncertainties, and is actually quite conservative. In any case, this uncertainty is negligible. I have added a sentence with these references.

ttbar (fig 13): https://arxiv.org/pdf/1311.0283.pdf

Z/gamma (fig 4 around the Z pole mass): https://arxiv.org/pdf/1812.10529.pdf

W (this one compared to NNLO and not NLO, but the uncertainty on the W is <5% so there is a large safety factor here to account for the difference): https://arxiv.org/pdf/1402.0923.pdf

- L355: need a reference or a brief description of the method used for combining the data.

We use the Best Linear Unbiased Estimate (BLUE) method. I have added a reference.

- L366-368: The discussion here is a bit too qualitative. It just says "could be a indication". Then it also could mean due to other effect, e.g. medium, or PDF. In this case, it's hard to justify it's role as a proxy of T_AA calculation. Since this is probably the most important physics message for the paper, the discussion need to be significantly expanded. For example, including model calculations, etc and if possible, try to make the conclusion stronger then "could be an indication" because this is already a precision measurement.

I have added a significant amount of discussion about this in the results section.

- L376: Ref.4 --->Fig.4

fixed

- L380: Ref.5 --> Fig.5

fixed

- Fig.3: missing x-axis title. Move y-axis title

fixed

- Fig.3: in the legend: "---- Glauber uncertainties", do you mean " []
Glauber uncertainties" ?

This is fixed, as the Glauber uncertainties are now shown by gray boxes.

- L394: "This reinforced the conclusion....". This need to be more
quantitative or need to be tone down, because we only say "could
indicate" from the yield plot that Z is not affected by medium. If we
can make the stronger conclusion than "could indicate" from the yield
plot, we can adjust the conclusion here correspondingly. Can we have a
model for Z v2? Could the small Z v2 due to the fact that Z is too heavy?

I have added a large amount of discussion about this in the results section. We have added models for the spectra and yields, but don't have a model for the v2. In general, the Z is expected to not experience the full QGP evolution because it's lifetime is around 0.1-0.3, while the formation time of the QGP is 0.3-1fm. The Z and its daughters also have no color charge, so I think it is difficult to see how they could get any final-state modification.

- all Figures: the legends and title are too small. The size need to
be significantly increased.

I have tried to improve the size of the figures' titles and legends.

This topic: Main > HIN19003ARCCommentsV1
Topic revision: r41 - 2020-01-25 - AustinBaty

Copyright &© 2008-2020 by the contributing authors. All material on this collaboration platform is the property of the contributing authors.
Ideas, requests, problems regarding TWiki? Send feedback