Response to referee report for SAIP2017: Article 118

I thank the referees for a fair and comprehensive review of my article for potential publication in the proceedings of SAIP2017. The edits that were suggested have all been implemented. Details about each edit are discussed below. For each suggested edit, my response is written in red.

Referee 1:

1. p2 "....labelled as th and tth, respectively" – Done.
2. p2 "...scatter processes were produced" – Done.
3. p3 "...then plotted as a function of..." – Done.
4. p5 "...from the best value of..." – I have rather reworded this to indicate that the 1σ band is around the mean value of the fit. This should be more clear.

Referee 2:

1. "The author should cite relevant references for the discovery of the Higgs in the first line." – Done.
2. "The author should provide an overview of relevant references (or perhaps some review articles) for the "plethora of models" referred to in the second sentence." – A comprehensive recent review has been cited.
3. "The author should provide some insight and a citation as to why $m_S \in [130, 200]$ GeV." – This has been added in the first paragraph, so that its discussion later in the paper isn't a surprise to the reader.
4. The author should address the grammatical inconsistency in the first sentence of the second paragraph: "the majority... have all... " – Removed the word "all".
5. "In the last sentence of the last full paragraph of p. 2, the author should clarify in what way measuring WW in the final state is truly unique to the production of a Madala boson. Can literally no other SM process yield WW in the final state?" – This point was a bit ambiguous. I have clarified it to state that the production of 2 same-sign leptons becomes non-negligible, which is a highly suppressed Standard Model process.
6. "The footnote on p. 2 is unnecessary and should be removed." – Done.

7. "In the third full paragraph of p. 3 the author should justify or provide a citation for the claim that "the kinematics are not significantly sensitive to the change in the mass of S." In particular, the author should describe why their results are insensitive to the mass of S when the mass chosen puts it *exactly* on-shell for the $H \to Sh$ process: it's difficult to believe that, for instance, a 5 GeV S would give the same results in Fig. 2 because then the decay products would be moving away with an additional 100 GeV of energy." – It is true that my wording was quite lazy here, and it has been fixed. The kinematics aren't really the issue here, rather the acceptance into the preselection region of the CMS analysis. I have made this clear in the text, and stated that the ability of the process to produce a final state with two same-sign leptons is not sensitive to changes of the S mass in the proposed region. I have also made a paragraph split here because the paragraph ends up quite long with the additions.

8. "The author should address whether they took $m_S = 145$ GeV as written in the text of the manuscript or $m_S = 140$ GeV as written in the plots in Fig. 2." – The text was erroneous, the actual mass point considered was 140 GeV. During the time of writing, it is possible that I mixed the numbers up due to some studies on the sensitivity of the mass of S to the acceptance into the chosen analysis's selection criteria. This has been fixed.

9. "In the last paragraph of the Discussion "more a" should be "a more"." – Done.

10. ""Most important, the author must address the extremely problematic result that $\beta_g^2 < 0$ for the tri-lepton channel. The author notes that the tri-lepton channel errors are large; however, the errors in the tri-lepton measurement surely were propagated through to the uncertainties on β_g^2 as shown in Table 2. Not only is that value of β_g^2 unphysical, but it's also 4σ away from the best fit value from the $e\mu$ channel. In particular, the author should assess the extent to which the various values of β_g^2 are in tension with one another for the three channels, perhaps through a χ^2/d.o.f. analysis." – I agree that my treatment of this result did not get the attention it deserved. The good point you raised warranted some more discussion in the first paragraph of the discussion section. The real issue is the statistical limitations of categorising the final result by lepton flavour and multiplicity. One should focus on the combined result rather that the individual category fits, which behave more like statistical fluctuations around the best fit value. This is the best we can do, given the limited nature of the dataset. It does sound quite condemning that one category is around 4σ away from the other category's best fit value, however if you take into account the uncertainty of **both** categories, it sounds less dire. Even better would be to consider one category's compatibility with the combined best fit value and its uncertainty; in this case the results are not as spread out as they might seem. In any case, the discussion has been

included in the new version of the manuscript, and not merely brushed aside as I had done before.

I trust that with the edits made to the short paper, it will be allowed to be included in the book of proceedings for SAIP2017.

Regards

Stefan von Buddenbrock