Dear Dr Loder

Thank you for your second round of reviews and your offer of provisional acceptance.

It has been fascinating seeing how the paper has been read by the reviewers. While our goal with the responses has always been to address in full the reviewers' points, there has been a lot of learning about the process of authorship on the way.

We have thought carefully and decided against using imputation, largely for the reasons that we had already set out. We note that we have the support of two out of four of this round's reviewers, and that AW is in favour of imputation, but acknowledges contrary arguments, and that DH is silent on the issue.

We are not clear that this statement relates to our paper:

> Perhaps I missed it, but I do not see anywhere mention of the short duration of included trials. There was very strong sentiment among the clinically active physicians at the manuscript meeting that the duration of most trials was too short and in some cases the doses of drugs too low to have much effect. You may not agree with this, but can you please acknowledge that possibility? Of course it can be argued that the manufacturers designed the trials and could have studied longer duration treatment or higher dos

On the assumption that it does apply to our paper, we have altered the relevant sentence on p8 to read:

> The pre-specified primary efficacy variables were: change in total Hamilton Depression Scale (HAM-D)[16] score from the beginning of the treatment phase to the endpoint of the acute phase; and the proportion of responders at the end of the eight week acute treatment phase **(longer than many antidepressant trials)**. Responders were defined as patients who had a 50% or greater reduction in the HAM-D or a HAM-D score equal to or less than 8.

Prompted by your reviewers, we have reworked the title and abstract to better reflect our view that our paper is as much about authorship and the authority of published conclusions as it is about the specifics of Study 329.

There is an important point related to blinding on which there appears to have been some confusion, hopefully now clarified.

Dr Henry appears concerned that non-blind coding of Serious AEs might have affected the findings. As per our previous letter, we are of the view that the original allocation needs to be blind – not the coding. The SAEs were coded blind. There were 6 "extra" non-serious events described within the narratives that were left uncoded or were coded and never transcribed. It was not possible to be blind to these, because allocation status was written into the narratives.

At least one of the missing events was a failure to transcribe 'Withdrawal Syndrome'. GSK had coded 'Withdrawal Syndrome' and 'Migraine' for one patient but only copied over 'Migraine' to Appendix D. Something similar may have happened for the other events – but this is less clear.

For those who think blind coding is important, we have had two MedDRA trained coders review a set of redacted SAEs. Both coders pulled out the additional 6 events that GSK had either left uncoded or not transcribed.

We can supply the redacted SAEs to BMJ and will make them available online with the manuscript.

We hope that our responses, covered in more detail in our 'response to reviewers' document, will allow BMJ to proceed towards publication. If accepted, it would be great to get an indication from you for likely publication date.

Yours sincerely

Jon Jureidini
On behalf of the 329 RIAT team