Reviewer's report

Title: An intercalated BSc degree is associated with higher marks in subsequent medical school examinations

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Reviewer: Olle Ten Cate

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Re-review of An intercalated BSc degree is associated with higher marks in subsequent medical school examinations

As I commented in the first review, I think this is a relevant and interesting topic that merits investigation.

Generally spoken, the authors have used an adequate approach. And still, from the data presented, specifically in Table 3, the point that intercalation causally affects improved academic performance after return to medical school is not strongly made, despite the additional multivariate analyses presented in Table 3. Let me try to explain.

Before doing this, let me state that I must agree with the authors that the correction for multiple significance tests, i.e. to set significance level on 0.01 is probably correct. That is not, as the authors state, “because Bonferroni is known to be extremely conservative”, but simply because the dependent variables are only those from year 4 and 5. These are 6 significance test, and p<0.01 should then be about adequate to convince the public of differences found. This leads to a strong recommendation to split table 3 in two parts: the information before the intercalation intervention (year 1-3) and the actual dependent variables (year 4 and 5 data). Putting them both into one table is a bit confusing.

But then, if one would compare data from table 3A and table 3B it readily appears that IC student are better both before and after intercalating. So, what readers try to estimate is whether the differences in years 4 and 5 remain over and above differences shown in years 1-3. That is not so easy and it would have been more reader friendly to present mean scores and estimated effect sizes, but the authors have chosen to use the Aberdeen model of CAS bands. Anyway, the data presented do not readily convince. This should force the authors to supply additional argumentation, and they have used multivariate analyses to do so. However, the description of both the exact method and the detailed results of these analyses is not elaborated much. Which covariates were exactly entered into the analyses? And what was the outcome (apart from odd ratios)? The unusual Nagelkerke R2 is still not explained well. Readers should be able to interpret the figures in table 4, otherwise it is no use to present them, but no explanation is given. [at one point I even considered to call Nico Nagelkerke, as I collaborated with him 20 yrs ago in Amsterdam, but decided that would take to much of my time]. The point is that the authors have not targeted well at the
expected readers’ level (which are not statisticians). It also does not help to convince in the discussion with “...we ‘crucially’ adjusted for performances in early year..”, if report does not show in detail how it was done.

Next, one confounder could easily have been removed. Both groups difference considerably, because the non-IC groups includes students with a degree. Most likely they are older and more mature. My recommendation therefore would be to remove those students from the control group and run all analyses again. Most likely, the two groups would be much more comparable. Then differences found in the dependent variables would be better interpretable.

In conclusion, I feel sympathy for this paper, but think it still needs more attention before publication, to strengthen the message.

Olle ten Cate

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

'I declare that I have no competing interests'