# Reply to reviewers

## Reviewer 1

(4) As the author of the sva package, I have now updated the vignette to encourage users not to include group differences when using ComBat in response to the results of this paper. This change has been pushed to Github (https://github.com/jtleek/sva-devel) should propagate to the development version of sva in a couple of days, and should propagate to the release version on the next release.

We are most pleased to see such a quick update to the sva package, and have added a note in the manuscript noting that this has already been done.

## Reviewer 2

1. Most importantly, the authors do an excellent job defining the problem and illustrating it with practical real-data examples. However, they make no attempt to actually solve the problem. For example, I agree that most people (incorrectly) use the distributional assumptions in 3.10 in their 2-step approaches. However, can the authors derive an expression for the actual distribution of the data, and then use this to provide and validate a more appropriate (and justified) two-step procedure. I believe that this is crucial for transforming this paper from an editorial that describes and presents a problem to an research paper that actually solves one.

Som delvis svar på dette (samt 2 og 3) kan jeg se om jeg kan formulere mere eksplisitt skillet mellom at frihetsgradene i F-distribusjonen og det at F-statistikken blir skalert opp med en faktor.

The main problem, as we see it, is the two-step procedure itself, and the proper solution is not to rely on it for heavily group—batch unbalanced data sets.

We have pondered if there might be a two-step solution in which group and batch effects are estimated using a two-way ANOVA, and then batch effect is only partially removed: by just the right amount to ensure the remaining variability from the batch effect compensates for the missing variability in the group effect estimates. However, we decided this was not a reliable approach. It might work for one particular analysis, e.g. comparison of differences between two groups, but would be unreliable for other analyses.

For analyses that produce a statistic similar to the F-statistic, one might try to compensate for the problem by adjusting the statistic by a factor (like the factor v\_0/v). This is perhaps the most general solution we can offer, but its adequacy still needs to be evaluated in each separate case.

2. Alternatively, this problem can be considered in the context of degrees of freedom. In a one-step procedure (e.g. ANOVA) the residual degrees of freedom is reduced by every parameter that is included in the model. So if a model includes treatment and batch, the t-test (or F-test) for significance of treatment will be less than the case that excludes batch (as is done in the two-step). Therefore, could the ‘effective’ degrees of freedom equal to the one-step residual df be used in the two-step procedure? Would this correct the distributional issues in Figure 2?

It is true that the degrees of freedom in the F-distribution are affected by the ANOVA model. This may be an issue when either groups or batches are very small and the number of covariates in the model is large compared to the number of samples. However, we consider this a relatively minor problem relative to the inflating of the F-statistic by a fixed factor (v\_0/v) which remains constant even as the sample size increases.

3. Furthermore, could the uncertainty ratio of v\_0/v defined by Jenson’s be used to achieve an ‘effective’ degrees of freedom?

See 1 and 2.

4. The authors claim that the problem of underestimating uncertainty is independent of size. However, while I expect that the problem will not go away as sample size gets larger, I will be surprised if the impact of the problem doesn’t reduce as the sample size gets larger. The authors need to better justify this result with more technical/theoretical descriptions and with data examples (can be simulated if needed here).

We have added a simulation with different sample sizes to illustrate that the problem does not decrease with increasing sample size. Actually, when using ComBat on small samples, it will tend to shrink the batch adjustments, and so the problem will often be less pronounced on small samples than on big ones.

5. Figure 1 very confusing and not clear. I have looked at this several times and for several minutes and I still don’t understand what the authors are trying to portray. The authors should reconsider how these data are presented and revamp this figure to make it clearer.

Denne jobber vi med.

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6. In the Abstract, the authors state ”which mostly likely has contributed to false discoveries being presented in the literature” this is a strong statement and should be backed up by some concrete examples.

Bør vi moderere denne litt siden vi kun har ett konkret eksempel og det hadde vært galt også uten ComBat?

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7. The authors focus their concern on ComBat, but other methods that adjust for technical heterogeneity, such as batch mean adjustment (without covariates) or SVA may also have the same concerns if used in a similar two-step process. This should be more clearly described and discussed. The problem is not with ComBat in particular, rather with the fact that uncertainty can easily be underestimated in two-step approaches.

Dette bør vi imøtekomme: det er jo faktisk et vesentlig poeng både at andre metoder som forsøker noe lignende (med kovariater) og batch-korrigering av ubalanserte data uten kovariater er problematisk.

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8. I agree with the statement “…systematically underestimate the statistical uncertainties and exaggerate the confidence of group differences…” I believe that in the introduction the authors do an excellent job heuristically describing the problem with two-step approaches.

9. In many parts of the manuscript, the authors discuss the ambiguous idea of underestimating ‘uncertainty’, but the never precisely define what they mean by uncertainty (e.g. variance, degrees of freedom, etc). This should be more clearly defined throughout the manuscript.

Kanskje vi burde bruke et mere presist uttrykk som “estimator variance” eller noe lignende.

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10. In Section 2.1, the authors describe the full ComBat model, but then immediately reduce their theoretical discussion to cases with only one gene and no differences in variance. As such, I think most of this subsection is largely irrelevant and confusing (i.e. defining a more general model and then never using it). It would be better to just define the models used in the paper, and then give heuristic descriptions of the differences between this an ComBat.

We agree that equation 2.2 presenting the model used by ComBat is not required for discussing the effects of batch adjustment using the two-way ANOVA model. However, for readers either familiar with ComBat, or who want to refer to the ComBat article, we think it is convenient to present what that model is and how it relates to the simpler model we analyse. It also has some relevance for discussing the similarities and differences between the pure two-way ANOVA method and ComBat.

11. Sections 2.2 and 3.2 are contain most of the technically important material, but their both contain multiple ideas/themes/results. These would be much easier to understand in the authors split these into smaller more direct subsections.

Det er kanskje en god ide: bare legge til underpunkter som stykker opp avsnittene.

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Minor concerns:

1. In section 3.3.1, the authors should include more detail/description of the actual experimental design for this experiment (how many batches, how many treatment/control samples are there in each batch, etc).

2. In Section 3.3.1 it might be better (and more) to use p-value cutoff and not FDR…

In this kind of analysis, FDR has become the de facto standard. The main reason for this is that the number of tests (genes tested) is so high that the smallest P values tend to get quite small owing to the number of tests alone, while Bonferroni correction tends to kill off most findings.

We do agree that the number of genes with FDR below a given threshold is a bad statistic for assessing findings or comparing methods.

Still, we decided to follow the analyses provided in the publication by Towfic et al. as closely as we could. However, we do comment that the use of FDR makes the analyses more sensitive to false positives.

3. The “6+2, 3+4”, etc represenations are not clear and should be presented in a different way

Dette kan vi skrive om litt så det blir tydeligere.

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