Empirical Bayes selection of wavelet thresholds: comments for referees

We are very grateful to both referees for their careful reading and helpful comments. We have considered every one of these, and our reactions are as follows. We believe we have taken account of all the points made, and we hope that the paper is now acceptable for publication.

Referee 1

Main comments

1. Action needed

2. We are grateful to this referee for suggesting a more careful comparison of the various methods in the tables. We have included, in the text, some discussion on the relative MSE as the referee proposes. In the translation-invariant case, the FDR(0.05) method indeed slightly outperforms the Laplace median (95% as opposed to 93%). But the results are stronger in the other direction in the standard case, with FDR(0.05) scoring 82% and Laplace median 90%, and Laplace mean over 92%. In addition the empirical Bayes methods also do better on the ion channel example. The NMR example has been removed in response to the other referee’s suggestions, but we have applied FDR to the inductance plethysmography data, as this referee requested, and the results are in line with our general conclusions. Overall, we have softened somewhat the claims made for our method as opposed to the FDR(0.05) method. Our overall conclusion now reads ‘Other methods improve slightly on empirical Bayes in some cases, but at the expense of somewhat more substantial underperformance in others, at least on the cases we have considered.’ We trust that this slightly more tentative conclusion is a better scientific description of the work we have carried out.

3. This has now been done. The results for the Gaussian prior, reported in the paper, are not competitive with the heavy-tailed priors.

4. We have carefully considered whether the paper should be rearranged, but have decided that we really would prefer it to remain in the present order. We realise that this is a slight departure from the standard convention but it is our firm belief that the paper is more readable in the form we have retained, because a more casual reader can read continuously to the beginning of the theory and then give up. We were encouraged to use this order in our previous paper ‘Needles and straw in haystacks’ and we genuinely believe it to be preferable. The Editor made it clear that it would ultimately be up to us whether or not to change the order.

Detailed comments

1. We have now used $\sigma_E$ for the error throughout. Another ambiguity of notation has been resolved by using $\vartheta$ for the value of the mixing parameter in equation (11).

2. These have been changed to $\mu$.

3. A definition of N and of $\theta$ has been included in Section 1.2.

4. Definition has now been included.

5. This has been attended to, by making it explicit when renormalized data are being considered.

6. Corrected.

7. Yes the referee is correct. We have included a sentence to this effect.

8. This figure has been removed in response to Referee 2.
9. Plot included as requested

10. The definition of $A$ has been clarified

**Referee 2**

1. This has been addressed by minor rewording. Relatively, the decomposition at coarser scales is not as sparse as at finer.

2. Action needed

3. Change made as suggested.

4. A sentence has been inserted to explain.

5. Sentence inserted explicitly making clear that we have not considered this issue but it remains an interesting topic for future work.

6. Dependence on $w$ made clear, which should deal with this point.

7. The paragraph setting out the as-if-independent approach has been rewritten to explain the rationale more clearly. Since in the translation-invariant case $K_j$ is the same at all levels, the effect of using the Benjamini-Yekuteli approach would be to do the same calculations but merely to recalibrate the parameter $q$ in the same way at every level. Some comments on this matter have been included.

8. corrected

9. Change made as requested

10. Unfortunately the software to implement the Luo-Wahba procedure is not readily available; as suggested on the web version of the paper, we contacted Luo who explained that the software is only in Fortran and is not publicly available from a web page; he said he would email it but did not do so. The main purpose of our comparison with a spline method was to compare with a widely used “public” method, so we feel it is more appropriate to retain the standard `spline.smooth` procedure. Of course we totally agree with the referee that it is not at all surprising that this method does not work well on the examples considered, and we have inserted some text to this effect. On the matter of the use of a different FDR approach, see our response to point 7 above.

11. Examples removed as suggested, and appropriate minor rewording carried out. The AWS method of Polzehl and Spokoiny has been included in the comparison for the ion channel data; its performance is not as good as the spline method.


13. In fact the paper by Delyon and Juditsky (1996) itself refers to an earlier version of Donoho et al (1997), but a sentence has been inserted.

14. We think the referee may have made a mistake here, and we believe that what we wrote originally is correct.

15. Done

16. Thank you!