

***Interactive comment on* “The direct inversion method for data assimilation using isentropic tracer advection” by M. N. Juckes**

Anonymous Referee #2

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Reply to interactive comment (ACPD, 5, S3713, 2005) on “The direct inversion method for data assimilation using isentropic tracer advection”, MS-NR acpd-2005-0192, by Juckes.

First of all, as I said in my original review, “this is a potentially important piece of work”. In fact, I think it is important that the paper is published. However, before this is done, I think the author should address the comments of the reviewers. I say this, because I think addressing these comments will improve the paper and make it more accessible to the data assimilation community.

I will try clarifying some of my previous comments and addressing the queries made

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by the author in the interactive comment.

(1) I think that it would help if the author related their approach to data assimilation methods that, if not “more established”, at least, in my view, are well-known in the literature (e.g. they are discussed in books and peer-reviewed papers). The method I have in mind is weak-constraint 4d-var (strong-constraint 4d-var can be seen as a special case of weak-constraint 4d-var). In particular, as I mentioned in my original review,

“Eq. (5): I think that, roughly speaking, the 3 terms on the RHS of the eqn. are, analogous to, JO (observations penalty term), JQ (model penalty term) and JB (background penalty term), that would arise in weak constraint 4d-var. Is this indeed the case? If so, it would clarify the meaning of Eq. (5) to state this (the comments on Eq. (6) hint that this is indeed the case, but I think more explanation is needed).”.

It would help if the author confirmed if this was indeed the case. If not, it would help if the author explained how these terms were different from weak-constraint 4d-var. The “fact answer” the author provides in the interactive comment would be a starting point. This would address my concern.

(2) The comment on “too much detail” (in the appendices) is a personal opinion. I found this detail difficult to follow; it also detracts from what I think is the main message of the paper: a potentially novel data assimilation approach which should be communicated to the data assimilation community. The final decision on what constitutes too much detail should be made by the editor. Although I would prefer that there was less detail in the appendices, this should not be a bar to publication if the author thinks the detail is justified.

(3) I am well aware of the discussions of the author’s work in the UK data assimilation community. This is why I think it is important that the paper should be published (subject to changes which should be easy to implement). Based on what the author states in the interactive comment, I am happy to see the paper as a novel method presented

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through an application. I may be doing the author a dis-service, but I felt this did not come through as clearly as it should have done in the paper. If the author explains the approach thus in the introduction (and elsewhere where needed), this particular concern of mine would have been addressed.

Because I think the author is now addressing my general comments, I have changed my recommendation to "accept subject to minor revisions". This assumes the author will proceed as suggested by my response, and as indicated in the interactive comment from the author.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8879, 2005.

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