Response from McKitrick

I concur that Allen and Tett should have been invited to provide a reply. When I submitted my paper to *Climate Dynamics*, I requested the journal solicit a response, to be refereed alongside my comment. When I was notified that my paper was accepted and a reply had not been sought, I sent my critique to Allen and Tett and offered to delay publication until they had a chance to prepare their response. Neither replied, and I followed up a week later with the same offer. This time Tett replied and encouraged me to go ahead and publish, which I did. I hope that a formal reply will be forthcoming. The failure of the journal to solicit a reply is no impediment to them providing one now.

In his comments, Allen argues, in part, that my critique doesn't matter because the profession no longer relies on AT99, and he quips that it's like finding new design defects in the Model T Ford. The analogy is inapt: no one has built a Model T in nearly a century. AT99 by contrast has underpinned 20 years of climate attribution, and is still central, not only to IPCC reports and the climate literature, but to some claims at issue in climate litigation. Nor has it been superseded in the way Allen claims. Allen and Stott (2003) is basically the same as AT99, just with a different last step (called Total Least Squares instead of Ordinary Least Squares). It inherits all the same problems as AT99. And as I pointed out in my original paper,* use of regularization methods to obtain an invertible climate noise covariance matrix† solves a computational problem but not the underlying theoretical ones.

Allen also refers to the www.globalwarmingindex.org site, which implements a suggestion in Otto et al. (2015): a simple regression between temperature and radiative forcing estimates. Otto et al. (2015) do not test for well-known time-series specification errors, such as mismatched levels of nonstationarity between temperature and forcing variables, which can cause spurious inferences. Understanding and addressing such issues requires advanced estimation and testing methods: 'high school-level regression' methods aren't adequate for generating robust findings. But this is a topic for another day.

There remains, meanwhile, a need for Allen and Tett either to refute or concede the criticisms in my paper. I have argued that AT99 contains incorrect equations, makes untrue claims about the properties of its proposed estimator, and prescribes a statistical test that yields meaningless results. They can debate the extent to which AT99 matters, but first let's settle whether it is correct.

Allen is confident the greenhouse gas signal is now so clear that methodological criticisms are unimportant. Part of that 'signal' is the mountain of past applications of AT99 and related methods. But it is also noteworthy that some papers attempting three-way (natural, greenhouse-gas and aerosol) optimal fingerprinting yield anything but a clear signal.‡

Allen says the issues I raise regarding Equation (3), the Error Independence condition, are 'quite familiar.' Yet, as I pointed out, AT99 omitted any mention of it, nor has anyone ever discussed it in the optimal fingerprinting literature, nor has any test ever been implemented for its potential failure. He erroneously equates it to use of different climate models between the pre-whitening and hypothesis testing steps to generate noise covariances (which, contrary to his claim, I mention in Section 3). But that is a separate issue and does not contribute to establishing the necessary conditions, [N1–N4], identified in my paper.

As to whether I 'massively' overstate the weight that has been placed on the RC test, the literature is what it is. Allen might regret that so much reliance has been placed on it, but it is not an overstatement on my part to point out how heavily it has been relied upon. AT99 made a vague but far-reaching assertion about their test score's meaning. But it is not a test of the equality of covariance matrices (for the form of such a test see Gupta and Tang 1984), nor does it test the Gauss–Markov conditions, nor does it establish whether the model is correctly specified or not. The issue

* See, e.g., footnote 5 and elsewhere.

† e.g. Hannart 2016.

‡ See, for example Ribes and Terray (2013) and Jones et al. (2016).

of varying the truncation lag parameter is irrelevant in this context.

The discussion of the fluctuation-dissipation theorem raises issues that are not germane to my argument. It may very well be that a climate model that is inefficient at dissipating energy will exhibit greater internal variability, so models with high climate sensitivity will tend to generate noisier signal patterns, making it harder to achieve signal detection. This might be an issue if fingerprinting papers only ever use high-sensitivity models, but my critique does not reduce merely to a claim that the signal scaling coefficient variances are biased, it is that the invocation of the Gauss–Markov Theorem to claim fingerprinting results (including coefficient magnitudes) are valid was incorrect, and there has been no basis for the increasing levels of confidence expressed in the results of the method over the past two decades.

As to Allen's final point, that I didn't go back and re-do previous results, a single paper can only cover so much. I am surprised that the rather elementary errors I identified have sat unnoticed for so long. I have a second paper now under review critiquing the use of Total Least Squares and I have other work under way exploring the specific implications of the failure of the Gauss-Markov conditions in fingerprinting regressions. But one thing at a time.

References

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3. Hannart, A (2016). 'Integrated optimal fingerprinting: Method description and illustration'. *Journal of Climate* March 2016.

4. Jones, G., P.A. Stott and J.F.B. Mitchell (2016). 'Uncertainties in the Attribution of greenhouse gas warming and implications for climate prediction'. *Journal of Geophysical Research-Atmospheres* 121(12): 6969–92.

5. Ribes, A. and L. Terray (2013). 'Application of regularised optimal fingerprinting to attribution. Part II: Application to global near-surface temperature'. *Climate Dynamics* 41(11–12) 2837–53.

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