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REPLY COMMENT

Are temperature trends affected by economic activity? Reply to Benestad (2004)

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We appreciate Dr. Benestad's interest in our paper, and we note that he does not dispute our choice of data or our methodology (Benestad 2004, this issue). However, his argument on the heteroskedasticity adjustment is not relevant. He writes:

They used a number of non-climatic factors and the econometrics program SHAZAM (White 1993) to derive a heteroskedasticity-consistent covariance matrix (White 1980) for modelling the statistical relationship between the trend estimates and non-climatic factors.

This makes it sound like the White's matrix is the model. It is not: it is just a weighting applied to the variance-covariance matrix of the coefficients of the model, and it is derived from the regression residuals, not from the non-climatic subset of the explanatory variables as Benestad says. He then adds:

Ordinary Least Squares (OLS) models may produce biased estimates, and the presence of heteroskedasticity in the residuals may be an indication of model misspecification such as incorrect functional form. The SHAZAM model therefore ought to give unbiased estimates of the coefficients describing the relationship between a number of factors and the temperature trend.

Using OLS in the presence of heteroskedasticity does not cause biased parameter estimates, nor does using the White's matrix adjustment change the functional form. Heteroskedasticity can produce *inefficient* estimates in small samples (i.e. the estimated variance can be too high). The correction applied has nothing to do with bias or functional form. These items are all discussed in the Kmenta book referenced in the paper itself.

After a close replication of our results Dr. Benestad proposes a rather odd test of robustness: half the data are discarded, and more than half of the predictor variables are discarded, and on this basis he tries to predict the behaviour of the dependent variable in the other half of the data set. Moreover, he threw out the northern hemisphere data, which are arguably the better quality data. So he is trying to use the worst half of the data set to predict the better half while using only a subset of explanatory variables. We are unaware of any paper in the refereed applied climatology literature that has performed the test suggested by Dr. Benestad; indeed, if he has ever seen such a test applied anywhere in a published atmospheric science paper he should have provided an example, which he did not.

In his introduction he argues that we failed to model spatial autocorrelation and if we had done so our *t*-statistics would have been much lower. In the conclusion he reiterates this point as if it has been proven. In between he presents no such model, no such re-estimation and no evidence for his assertion. All he presents is a pair of correlation coefficients between temperature and GDP drawn from a subset of the whole data set that have no bearing on the spatial autocorrelation question.

The main conclusion of our paper is that non-climatic signals in raw station data carry over to IPCC gridded data despite previous assertions that they have been removed. We were very careful to argue that the precise magnitude of this effect remains unknown, and that it should be the subject of refined research. But Dr. Benestad's comment does not raise a single piece of evidence against our central conclusion. Indeed, he does not discuss the issue of uncorrected contamination in the IPCC gridded data at all.

In addition to our own statistical results, we provide a long list of papers that have also found evidence of non-climatic signals in station data. Yet Dr. Benestad asserts in his conclusions that '*There is therefore no evidence suggesting that the temperature trends are*

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systematically influenced by non-climatic factors'. Dr. Benestad cites only the IPCC volume, whose unsupported assertion on the subject is precisely the subject of our paper, and an obscure paper from a conference volume that appears to be a pressure-based paleoclimatic study over the 19th and 20th centuries, and whose relevance to the conclusions of our paper is nowhere explained.

Dr. Benestad's entire case rests on his failure to identify a significant effect in his own model, which is completely different from ours but which he thinks should be preferred to the one we used. Any such argument requires, at a bare minimum, reference to basic model evaluation criteria, such as adjusted R-squared statistics or Information Criteria (Akaike, Bayes, etc.), none of which he provides. If he had properly specified and implemented a spatial autocorrelation model he should have done an *F*-test on the restrictions that would yield our model as a nested version of his. Only on this basis could he claim to have a superior model specification. As it stands his conclusion has no supporting evidence.

Despite our substantive criticism of Dr. Benestad's comments, we would like to thank him for his interest in our research, and look forward to further constructive scientific debate.

LITERATURE CITED

Benestad RE (2004) Are temperature trends affected by economic activity? Comment on McKitrick & Michaels (2004). Clim Res 27:171–173

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