

## REJOINDER

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We heartily thank Michael Stein and Brad Efron for selecting our paper for discussion and for the tremendous task of recruiting and editing 13 discussion articles on this controversial and timely topic. We are grateful for the opportunity to receive feedback on our work from such a large number of knowledgeable discussants whose work and fields of expertise are so broad. The fact that our paper was of interest not only to academic statisticians and climate scientists but also economists and popular bloggers<sup>1</sup> bespeaks the importance of the topic.

We thank all 13 discussants for the time they put into considering and responding to our paper. Each one deserves a detailed response, but time and space constraints make that impossible. We therefore acknowledge the favorable tenor of the discussions generally if not specifically.

The discussion has great value, particularly for raising points of contrast sometimes about fundamental issues. For instance, Wahl and Amman (WA) note “there is an extensive literature contradicting McShane and Wyner’s (2011a) assertions about low or poor relationships between proxies and climate.” On the other hand, Tingley asserts “each proxy is weakly correlated to the northern hemisphere mean (for two reasons: proxies generally have a weak correlation with local climate, which in turn is weakly correlated with a hemispheric average)” and Davis and Liu (DL) state “there is just not much signal present.” This contrast can be explained at least in part by context. Our paper addresses the specific task of reconstructing annual temperatures over relatively short epochs during which temperatures vary comparatively little. Nevertheless, such contrasts are suggestive of the important frontiers for research and we hope our paper and this discussion will lead to advances on these fronts.

In this rejoinder, we aim to do three things. First, we respond to the detailed and highly critical discussion of Schmidt, Mann, and Rutherford (SMR). Next, we reiterate our key findings while targeting themes that emerge from multiple discussants. Finally, we conclude with a more in-depth response to Tingley and Smerdon who address the same broad issue. The discussions of SMR and Tingley are noteworthy because they take a “scientific” approach as opposed to the “statistical” approach taken by many of the other discussants (e.g., DL and Kaplan),

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<sup>1</sup>Steve McIntyre of [www.climateaudit.org](http://www.climateaudit.org) and Gavin Schmidt of [www.realclimate.org](http://www.realclimate.org).

thereby highlighting some of the differences between various approaches to data analysis [Diaconis (1985)] and pointing to some of the weaknesses of the former in high uncertainty settings such as proxy-based global temperature reconstruction. Smerdon's discussion, on the other hand, heeds both scientific and statistical considerations.

Some of the discussants chose to highlight questions and problems related to the introduction and history [Nychka and Li (NL), WA]. Others reflected on approaches outside the scope of our expertise (Hölmstrom). While these are interesting topics, they are also tangential to the central issue of our paper—the *uncertainty* of proxy-based global temperature reconstructions (i.e., the second moment rather than the first). In our rejoinder we will focus more narrowly on this topic.

This short form version of the rejoinder should be read as a summary document. A fuller version, which contains the supporting details and figures for the claims made here, can be found as a supplementary information (SI) document at the *Annals of Applied Statistics* supplementary materials website [McShane and Wyner (2011b)]. The short and long documents are divided into the same sections for easy reference and the reader interested in the full treatment can read the long document alone without reference to the short one. As with our paper, code to implement all analyses conducted for the rejoinder is available at the *Annals of Applied Statistics* supplementary materials website [McShane and Wyner (2011c)].

**1. Rejoinder to SMR.** Broadly, SMR engage in a two-fold critique of our conclusions. First (SMR Figure 1), they aim to show that our 1000-year temperature reconstructions based on real proxy data are flawed, allegedly because we miss important problems in a subset of the data. Second (SMR Figure 2), they argue through a simulation study that the RegEM EIV (Regularized Expectation-Maximization Errors-In-Variables Algorithm, referred to throughout this rejoinder as RegEM, RegEM EIV, and EIV) method is vastly superior to the methods examined and applied in our paper. We show that this is not true.

Before embarking on our discussion of their work, we must mention that, of the five discussants who performed analyses (DL, Kaplan, SMR, Smerdon, and Tingley), SMR was the only one who provided an incomplete and generally unusable repository of data and code. The repository created by SMR *specifically for this discussion* was, like that of the other four discussants, graciously provided and quite usable. However, we lacked clear and easily implementable code (i) to fit RegEM EIV ourselves and (ii) to draw new temperatures and pseudoproxies from their simulation model. Code for these purposes is archived by Mann at <http://www.meteo.psu.edu/~mann/PseudoproxyJGR06/> and <http://www.meteo.psu.edu/~mann/supplements/MultiproxyMeans07/>.

Among other things, the RegEM EIV fitting procedure cannot be executed by a straightforward function call as is typical for statistical code libraries. Rather,

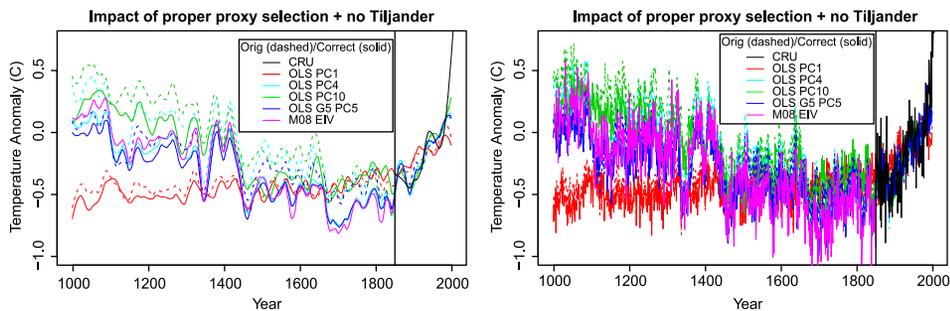


FIG. 1. *Reproduction of SMR Figure 1a. The left panel gives smoothed fits thereby reproducing SMR Figure 1a whereas the right panel gives unsmoothed fits. Results using the reduced set of 55 Mann et al. (2008) proxies (excluding Tiljander) are plotted with solid lines whereas results using the full set of 93 proxies are plotted with dashed lines. Two features stand out from these plots. First, the differences between the fit of a given method to the full or reduced set of proxies are quite small compared to the annual variation of a given fit or compared to the variations between fits. Second, the RegEM EIV methods produce reconstructions which are nearly identical to those produced by OLS PC4 and OLS G5 PC5. Compare also with SMR Figure S2 which is similar to the bottom panel but excludes RegEM EIV.*

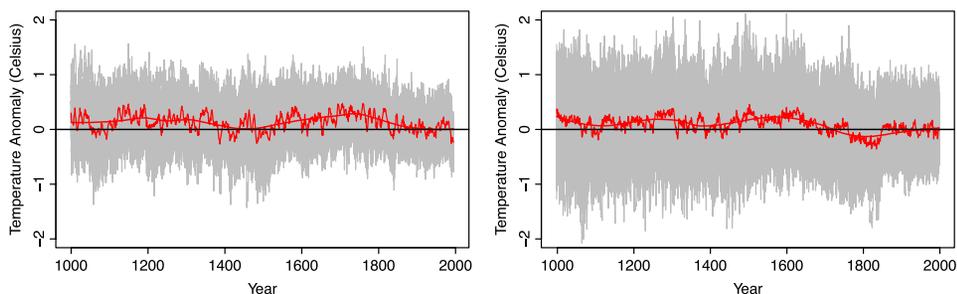


FIG. 2. *Difference between our Bayesian AR2 + PC10 model of Section 5 and various other models. The left panel gives the difference between our Bayesian AR2 + PC10 model fit to the network of 93 proxies dating back to 1000 AD and the original Mann et al. (2008) RegEM EIV fit to the network of 59 proxies dating back to 1000 AD. The right panel gives the difference between our Bayesian AR2 + PC10 model fit to the network of 93 proxies dating back to 1000 AD and the model of SMR Figure 1b (i.e., the same Bayesian AR2 + PC10 model but fit to the network of 55 proxies dating back to 1000 AD instead of the network of 93 proxies). As can be seen, there are no statistically significant differences between these two models and our Bayesian AR2 + PC10 model fit to the 93 proxies. The annual difference is given in red, the smoothed difference in thick red, and annual uncertainties bands are given in gray. The right plot has wider intervals because the uncertainty in both models is accounted for. Since we lack uncertainty estimates for RegEM, the left panel uses only the uncertainty estimates of our Bayes model. Compare to SMR Figures 1b and 1c as well as figures in the SI.*

the archives consist of a large number of files layered on top of one another and, despite a major effort on our part, we were unable to replicate published results within the publication time constraints of this rejoinder. That said, independent

researchers have, after important modifications,<sup>2</sup> successfully run code from the first URL (Jason Smerdon, personal communication). Consequently, throughout this section, we work with RegEM  $\hat{y}$ 's (pre-fit by SMR to both real and simulated data) as well as one particular draw of the data from their simulation which were provided.

1.1. *Proxy data: Full versus reduced network of 1000 year-old proxies.* SMR allege that we have applied the various methods in Sections 4 and 5 of our paper to an inappropriately large group of 95 proxies which date back to 1000 AD (93 when the Tiljander lightsum and thicknessmm series are removed due to high correlation as in our paper; see footnote 11). In contrast, the reconstruction of Mann et al. (2008) is applied to a smaller set of 59 proxies (57 if the two Tiljander series mentioned previously are removed; 55 if all four Tiljander series are excluded because they are “potentially contaminated”).

The process by which the complete set of 95/93 proxies is reduced to 59/57/55 is only suggestively described in an online supplement to Mann et al. (2008).<sup>3</sup> As statisticians we can only be skeptical of such improvisation, especially since the instrumental calibration period contains very few independent degrees of freedom. Consequently, the application of ad hoc methods to screen and exclude data increases model uncertainty in ways that are unmeasurable and uncorrectable.

Moreover, our interpretation of SMR Figure 1 is quite different. We see the variation between the larger and smaller datasets as relatively small with respect to the variation among the models. The appearance of a difference in SMR Figure 1a is especially magnified because those reconstructions are smoothed. Smoothing exaggerates the difference and requires careful adjustment of fit statistics such as standard errors, adjustments which are lacking in SMR and which are in general known only under certain restrictive conditions. In contrast, consider the right panel of Figure 1 which is a reproduction of SMR Figure 1a without smoothing. The difference between a given model fit to the full dataset or the reduced data set is clearly dwarfed by the annual variation of the fit; the full and reduced set of proxies yield inconsequentially different reconstructions. It thus seems to us the main point of Figure 14 of the paper (which SMR Figures 1a and S2 roughly mimic) stands: various methods which have similar holdout RMSEs in the instrumental period produce strikingly different reconstructions including “hockey sticks” (such

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<sup>2</sup>A bypass of the function used to generate new pseudoproxies during each run (pseudoproxytwo.m) is required since this module appears to be inoperative.

<sup>3</sup>The Mann et al. (2008) Supplementary Information contains the following note: “Tree-ring data included 926 tree-ring series extracted from the International Tree Ring Data Bank (ITRDB, version 5.03: [www.ncdc.noaa.gov/paleo/treering.html](http://www.ncdc.noaa.gov/paleo/treering.html)). All ITRDB tree-ring proxy series were required to pass a series of minimum standards to be included in the network: (i) series must cover at least the interval 1750 to 1970, (ii) correlation between individual cores for a given site must be 0.50 for this period, (iii) there must be at least eight samples during the screened period 1800–1960 and for every year used.”

as the red one in Figure 1), “inverted check marks” (such as the green), and things in between (such as the blue and purple). In short, while SMR allege that we use the “wrong” data, the result remains the same (also see SI).

We have two additional findings. First, as shown in Figure 1, the RegEM reconstruction is *nearly identical* to OLS PC4 and OLS G5 PC5. This is particularly interesting in light of the performance comparisons of SMR Figure 2. Second, SMR Figure 1a and our Figure 1 given here do not account for uncertainty in the model fits. When such uncertainty is accounted for, as can easily be done for the models in SMR Figures 1b and 1c, we see that the difference between the reconstructions produced from the larger data set of 95/93 proxies and the 59/57/55 are negligible with respect to overall uncertainty (see Figure 2; see SI for more details).

1.2. *The selection of principal components.* SMR Figure 1c replots our Bayes model (Figure 16 of the paper) with two differences: it uses the reduced dataset of 55 proxies and only four principal components. There are no statistically significant differences between the resulting model and our original one (see SI), yet SMR allege that “ $K = 10$  principal components is almost certainly too large, and the resulting reconstruction likely suffers from statistical over-fitting. Objective selection criteria applied to the Mann et al. (2008) AD 1000 proxy network, as well as independent “pseudoproxy” analyses discussed below, favor retaining only  $K = 4$ .”

SMR are wrong on two counts. First, the two “objective” criteria they suggest select differing numbers of principal components. Second, each criterion has multiple implementations each producing different results. As is well known to statisticians, there is no single objective way to resolve these discrepancies. Furthermore, the PC selection procedures that SMR prefer select “significant” PCs based entirely on the matrix of predictors without considering the response variable. To protect against overfitting, the selection process should in some way take into account the relationship between the predictor and the response [see also Izenman (2008), Hastie, Tibshirani and Friedman (2009)]. Compounding matters, SMR implement their allegedly objective criteria in nonstandard and arbitrary ways and several times in error.<sup>4</sup> When correctly implemented, the number of principal components retained varies across each “objective” criterion from two to fifty-seven. Using ten principal components, therefore, can hardly be said to induce the “statistical over-fitting” claimed by SMR.

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<sup>4</sup>They appear to mistake the squared eigenvalues for the variances of the principal components which leads to a thresholding of total variance *squared* instead of variance. We provide complete details in the SI.

1.3. *Simulated data.* SMR Figure 2 (along with SMR Table S1) purports to show that the Lasso (applied in Section 3 of our paper) and the variety of principal component methods (applied in Section 4) are fundamentally inferior to the RegEM EIV method of Mann et al. (2008) and to thereby challenge our assertion that various methods perform similarly (based on Figures 11–13 of the paper). RegEM is considered to be a state of the art model for temperature reconstructions in the climate science literature [Mann et al. (2007, 2008), Lee, Zwiers and Tsao (2008)].

SMR Figure 2 is based on data simulated from National Center for Atmospheric Research (NCAR) Climate System Model (CSM) as well as Helmholtz-Zentrum Geesthacht Research Centre (GKSS) European Centre Hamburg Ocean Primitive Equation-G (ECHO-G) “Erik” model. We see several problems with this simulation:

(1) While we can vaguely outline the process which generated the simulated temperatures and pseudoproxies, the details are buried in layers of code at various supplementary websites and therefore are not reproducible.

(2) In contrast to the methods of Sections 4 and 5 of our paper which are transparent, RegEM appears to be a classic, improvised methodology with no known statistical properties, particularly in finite samples or when assumptions are violated. For instance, the “missing at random” assumption [Little and Rubin (2002)] likely fails to hold here [Smerdon, Kaplan and Chang (2008)]. Further, there are enormous numbers of variations on RegEM (e.g., RegEM-Ridge, RegEM-Truncated Total Least Squares, etc.) each with their own associated tuning parameters and no firmly agreed upon methods for tuning them [Smerdon, Kaplan and Chang (2008), Christiansen, Schmith and Thejll (2009, 2010), Rutherford et al. (2010)]. Consequently, we cannot rule out the possibility that RegEM was tailor-made to the specific features of this simulation,<sup>5</sup> particularly since the same simulations have been used in repeated studies.<sup>6</sup> This is an especially important

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<sup>5</sup>This is suggested by the fact that RegEM performs nearly identically to OLS PC4 and OLS G5 PC5 on the real proxy data (see SMR Figure 1a and our Figure 1; see also SMR Figure 2 and our corrected versions in Figure 3 and the SI) but substantially better on the simulated data (see SMR Table S1 and our corrected version in the SI).

<sup>6</sup>For a review of papers using these simulations, see Smerdon, Kaplan and Chang (2008) who state in their opening two paragraphs: “Rutherford et al. (2005) used RegEM to derive a reconstruction of the NH temperature field back to A.D. 1400. This reconstruction was shown to compare well with the Mann, Bradley and Hughes (1998) CFR. . . Mann et al. (2005) attempted to test the Rutherford et al. (2005) RegEM method using pseudoproxies derived from the National Center for Atmospheric Research (NCAR) Climate System Model (CSM) 1.4 millennial integration. Subsequently, Mann et al. (2007) have tested a different implementation of RegEM and shown it to perform favorably in pseudoproxy experiments. This latter study was performed in part because Mann et al. (2005) did not actually test the Rutherford et al. (2005) technique, which was later shown to fail appropriate pseudoproxy tests [Smerdon and Kaplan (2007)]. . . Mann et al. (2005) used information during the reconstruction interval, a luxury that is only possible in the pseudoclimate of a numerical model simulation and not in actual reconstructions of the earth’s climate.”

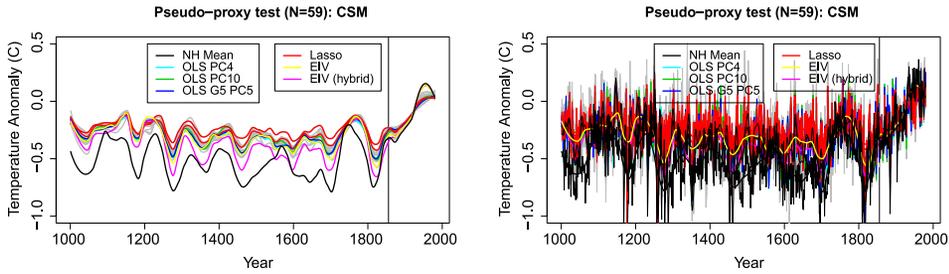


FIG. 3. Corrected version of SMR Figure 2a. The left panel gives smoothed fits as in SMR Figure 2a whereas the right panel gives unsmoothed fits. The annual variation of each method’s fit dwarfs the difference between methods. See SI for corrected versions of SMR Figures 1b, 1c, and 1d.

point since it is common to find that some methods work well in some settings and quite poorly in others.

(3) SMR make absolutely no attempt to deal with uncertainties, either for a given draw of data from the simulation or across repeated draws of the simulation even though there is considerable variation in both [see Burger and Cubasch (2005) for variation of fit conditional on data and Christiansen, Schmith and Thejll (2009) for variation of fit across draws of a simulation].

(4) How relevant are the results of the simulation to the real data application (i.e., Berliner’s point about the “need to better assess” these large-scale climate system models, something we return to in Section 1.4 below)?

Fortunately, we are able to use the data and code provided to us to rebut SMR’s findings. Before proceeding, however, we must note a troubling problem with SMR Figure 2. Visual inspection of the plots reveals an errant feature: OLS methods appear to have nonzero average residual *in-sample*! Upon examining the code SMR did provide, we confirmed that this is indeed the case. The culprit is an unreported and improper centering of the data subsequent to the model fits, resulting in biased estimates and uncalibrated confidence intervals.

SMR Figure 2 does not plot raw Northern Hemisphere annual temperature but rather NH temperature anomaly, that is, NH temperature minus the average NH temperature over a subset of the in-sample period (defined to be 1900–1980 AD for the CSM simulation and 1901–1980 for the GKSS simulation). This centering technique is common in climate science and simply represents a change of location. However, SMR fit the various OLS and Lasso methods to the raw (uncentered) temperature over the full calibration period 1856–1980 AD. In order to center the predictions, rather than subtracting off the mean 1900–1980 (1901–1980) AD NH temperature, they subtracted off the mean of *each model’s fitted values* over 1900–1980 (1901–1980) AD. This erroneous and nonstandard centering results in a substantially biased predictor with an overestimated RMSE. We refit the models to centered rather than raw temperature and the RMSEs were about 15–20% lower than in SMR Table S1 (see SI). Furthermore, the differences between the various methods were dramatically reduced.

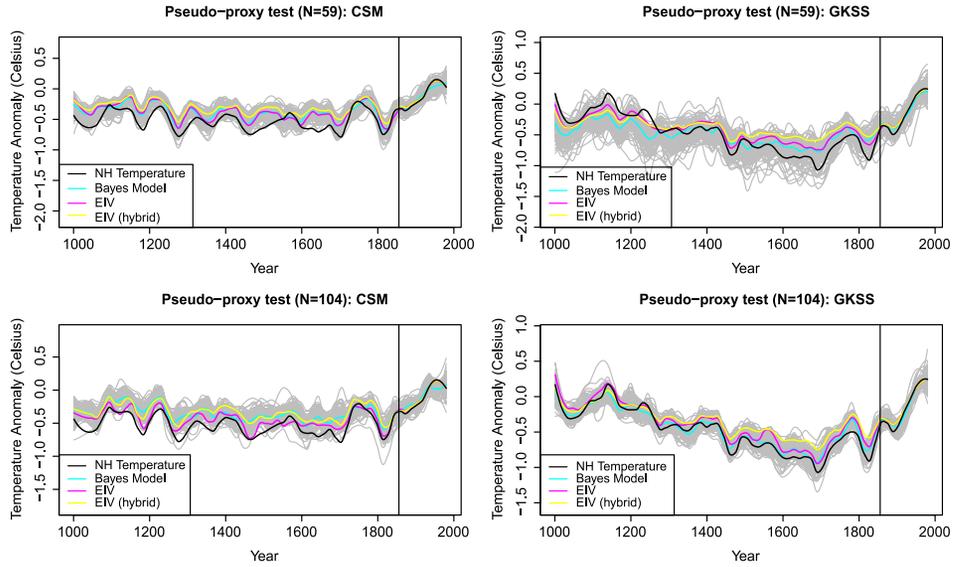


FIG. 4. Bayesian AR2 + PC10 model of Section 5 applied to simulated data and smoothed. As can be seen, the Bayes model appears to perform similarly to both RegEM EIV methods. Furthermore, the confidence intervals of the Bayes model (gray) appear to be calibrated. We give the unsmoothed version of this figure in the SI.

Additionally, SMR make no attempt to grapple with standard errors. As a first step to address this, we replot SMR Figure 2a appropriately centered in Figure 3 (for the other three panels, see SI). In addition to including a corrected version of their smoothed plot, we also include an unsmoothed plot. As with the real data plotted in Figure 1, the differences across methods are dwarfed by the annual variation within method. Thus, differences among various methods do not appear so large.

We can improve on SMR Figure 2 and our own Figure 3 substantially by drawing on our Bayesian model of Section 5. To that end, we fit the Bayesian AR2 + PC10 model to the simulated data provided by SMR and include smoothed sample posterior prediction paths in Figure 4 (we include an unsmoothed plot in the SI; since SMR prefer four principal components, we include plots for a Bayesian AR2 + PC4 model in the SI and note that the four and ten PC models perform almost identically). As can be seen, our model provides a prediction which is almost identical to that of two RegEM fits. There are no statistically significant or even practically important differences between our model's reconstruction and that of RegEM. Furthermore, the confidence bands provided by the model generally include the target NH temperature series (and always do when unsmoothed), thus suggesting the large uncertainty bands of our Section 5 are appropriate and not too wide.

In addition, our Bayesian models outperform RegEM EIV in terms of holdout RMSE (see SI). In fact, they even outperform the hybrid version of RegEM EIV in two of the four simulations. In a sense, this is not even a fair comparison because the hybrid method makes use of annual as well as smoothed (lowpassed) proxy data. Indeed, it is possible to make a hybrid version of any method, including our Bayesian method, and such hybrids would be expected to perform better than the nonhybrid version shown here. However, “in practice, whether or not the hybrid procedure is used appears to lead to only very modest differences in skill” [Mann et al. (2007), page 3].

A final point worth noting is that this demonstration accounts only for the uncertainty of the model fit conditional on one draw of the simulation. As stated before, we are unable to properly assess how model fits vary from draw to draw. This unaccounted for source of variation is likely large [Christiansen, Schmith and Thejll (2009)] and would be a useful subject for additional research.

1.4. *Real data versus simulated data.* Berliner calls for an assessment of whether large-scale climate models like those studied in Section 1.3 can serve as a surrogate for controlled experiments. In this section, we make a modest advance on this front (see SI for all plots).

Climate scientists, when evaluating these simulations, have focused on several technical issues. Smerdon, Gonzalez-Rouco and Zorita (2008) shows that Mann et al. (2007) employed an inappropriate interpolation of GKSS temperatures and that verification statistics “are weakened when an appropriate interpolation scheme is adopted.” More recently, Smerdon, Kaplan and Amrhein (2010) “identified problems with publicly available versions of model fields used in Mann et al. (2005) and Mann et al. (2007)” thereby showing that “the quantitative results of all pseudoproxy experiments based on these fields are either invalidated or require reinterpretation.” Hence, climate scientists have questioned the value of results derived from the CSM and GKSS simulations due to technical issues internal to the simulation procedure.

As statisticians, we approach the evaluation of simulated data from a somewhat different perspective. When statisticians design simulations, we tend to follow one very important general rubric: if one wants insights gleaned from the simulation to carry over to real data, then key features of the simulated data should match key features of the real data. Thus, we augment climate scientists’ “internal” evaluation of the simulated data with an “external” evaluation comparing it to real data.

We have already seen one way in which the real data and simulated data appear to differ: RegEM gives fits and predictions that are nearly identical to those of OLS PC4 and OLS G5 PC5 on the real proxy data (see SMR Figure 1a and our Figure 1) but the fits and predictions on the simulated data are quite different (see SMR Figure 2 and our corrected versions in Figure 3 and the SI; see also SMR Table S1 and our corrected version given in the SI). More broadly, we observe that the simulations appear to have smoother NH temperatures than the real data. This

is confirmed by the much smoother decay in the autocorrelation function. Furthermore, the partial autocorrelations appear to die out by lag two for the simulations whereas for the real data they extend to lag four. Moreover, it seems the simulated time series have only one or at most two distinct segments, unlike the “three or possibly four segments” in CRU discerned by DL.

In addition to examining the NH temperatures series, we subject the local grid temperature and (pseudo-)proxy series to a number of rigorous tests. We examine QQ plots of several summary statistics of the various local temperature and proxy series with null distributions provided by the bootstrap [Efron and Tibshirani (1994)]. The first statistic we consider is the lag one correlation coefficient (see SI as well as Figure 7 of the paper). We also consider the correlation of each series with the relevant Northern Hemisphere temperature, calculated over the instrumental period. Finally, we standardized each series and looked at the sample standard deviation of the first difference of the standardized series. In each case, QQ plots reveal that the real data distributions are strikingly different than those of the simulated data. In particular, the result about the lag one correlation coefficient confirms Smerdon and Kaplan’s (2007) observation that the “colored noise models used in pseudoproxy experiments may not fully mimic the nonlinear, multivariate, nonstationary characteristics of noise in many proxy series.”

Important and obvious features of the real data are not replicated in the simulated data. This therefore puts the results of Section 1.3 (as well as other studies using these simulations for these purposes) into perspective. How applicable are these results to the real data when such prominent features fail to match? We can think of few more fertile areas for future investigation.

## 2. Section 3 revisited.

2.1. *The elusiveness of statistical significance.* Section 3 of our paper deals with the statistical significance of proxy-based reconstructions and how every assessment of statistical significance depends on the formulation of both the null and alternative hypotheses. The question is whether proxies can predict annual temperature in the instrumental period, with significant accuracy, over relatively short holdout blocks (e.g., 30 years). There are two main variables: (i) the method used for fitting the data and (ii) the set of comparison “null” benchmarks. In Figures 9 and 10 of the paper, we chose a single fitting method (the Lasso) and provided evidence that the choice of benchmark dramatically alters conclusions about statistical significance. The proxies seem to have some statistical significance when compared to white noise and weak AR1 null benchmarks (particularly on front and back holdout blocks) but not against more sophisticated AR1 (Empirical) and Brownian motion null benchmarks. McIntyre and McKittrick (MM) seem to most clearly understand the purpose of this section, and we again recognize their contribution for first pointing out these facts [McIntyre and McKittrick (2005a, 2005b)].

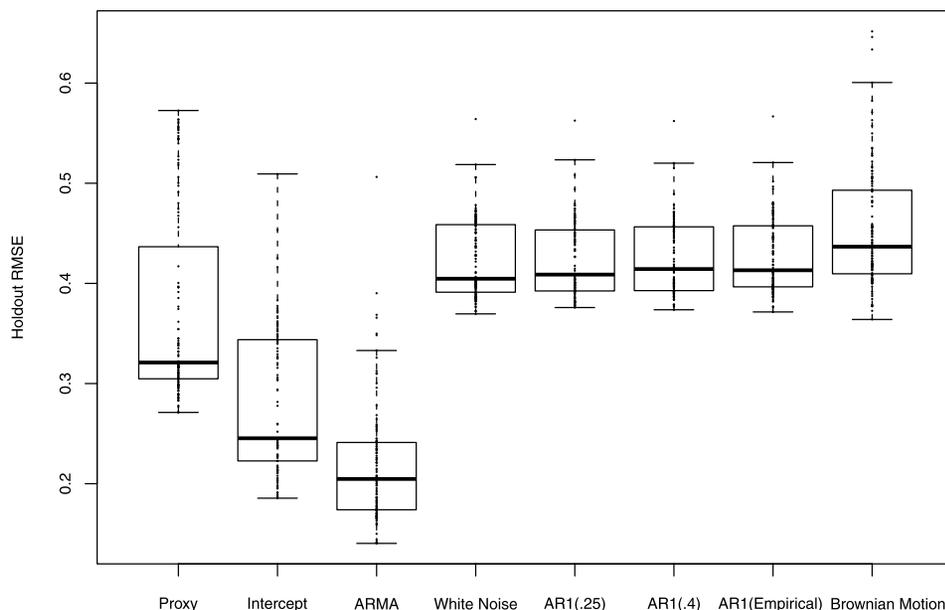


FIG. 5. *Cross-validated RMSE on 30-year holdout blocks for various models fit to proxies and pseudo-proxies. The procedures used to generate Intercept and ARMA boxplots are discussed in Section 3.2 of the paper. The procedures used to generate the White Noise, AR1, and Brownian motion pseudo-proxies are discussed in Section 3.3 of the paper. The CPS fitting procedure used for the Proxy, White Noise, AR1, and Brownian motion boxplots is described in Section 2 of the long form rejoinder.*

Before responding to specific objections raised against the choices we made to generate Figures 9 and 10 of the paper, we respond to a deep and statistically savvy point raised by Smerdon: our Lasso-based test could be “subject to Type II errors and is unsuitable for measuring the degree to which the proxies predict temperature.” He suggests that composite-plus-scale (CPS; see SI for a description) methods might yield a different result.

The holdout RMSEs obtained by using CPS on the proxies and pseudo-proxies (with weights equal to cosine of latitude for Northern Hemisphere proxies and zero for Southern Hemisphere ones) appear in Figure 5. This boxplot has a number of striking features. Averaged across all holdout blocks, CPS predictions based on proxies outperform those based on pseudoproxies.<sup>7</sup> However, CPS performs substantially worse than ARMA models and certainly no better than an intercept-

<sup>7</sup>SMR and others have chided us for calling our various noise series “pseudoproxies.” We note, with Tingley, that such series represent the limiting case of pseudoproxies with zero signal-to-noise ratio and thus can lay some claim to the name. It is regardless an unimportant distinction and, for this rejoinder, we stick with the nomenclature of the paper.

only model. Finally, the various pseudoproxy RMSEs are strikingly similar to one another.

In juxtaposition to Figure 9 of the paper which gave the holdout RMSEs from the Lasso, the CPS holdout RMSEs of Figure 5 are quite provocative and deserve more attention. Using this implementation of the CPS method, one might indeed conclude that the proxies are statistically significant against all pseudoproxies. However, CPS appears to be a “weak” method in that its holdout RMSEs are larger than the corresponding ones from the Lasso as can be seen by comparing Figure 5 here to Figure 9 of the paper. If, in turn, one uses a more powerful method like the Lasso—one that performs better at the ultimate goal of predicting temperature using proxies—the results, as indicated in Figure 9 of the paper, are mixed: the Lasso appears somewhat significant against weak null benchmarks like AR1(0.25) and AR1(0.4) but not against strong ones like AR1(Empirical) and Brownian motion.

Actually, the conclusions are decidedly more unclear than the boxplots of Figure 5 suggest. In the SI, we plot the RMSE by year for the CPS method and provide null bands based on the sampling distribution of the pseudo-proxies (as we did for the Lasso in Figure 10 of the paper). We see that the proxies have consistently lower RMSEs than those of the pseudo-proxies for a majority of the holdout blocks. Against weak pseudo-proxies, the real proxy predictions are highly statistically significant on the first few and last few blocks. However, against more sophisticated pseudo-proxies, CPS forecasts do not appear to be statistically significant. Furthermore, though much worse than ARMA models on the interpolation blocks, CPS predictions are not necessarily worse on the first and last blocks.

Like Aeneas’ description of Dido, statistical significance is *varium et mutabile semper* (fickle and always changing) [Maro (29BC)]. Conditional on a choice of method, pseudoproxy, and holdout block (or aggregation of blocks), the proxies may appear statistical significant. Yet, ever so slight variations in those choices might lead to statistical insignificance. Consequently, it is easy to misinterpret statistical significance tests and their results (as also discussed by DL, Kaplan, Smerdon, Tingley, and WA). We fault many of our predecessors for assiduously collecting and presenting all the facts that *confirm* their theories while failing to seek facts that *contradict* them. For science to work properly, it is vital to stress one’s model to its fullest capacity [Feynman (1974)]. The results presented in Figures 9 and 10 of the paper, in Figure 5 here, and in various figures in the SI, suggest that maybe our models are in fact not strong enough (or that proxies are too weak). Furthermore, in contexts where the response series has substantial ability to locally self-predict, it is vital to recognize this and make sure the model and covariates provide incremental value over that [otherwise, “a particular covariate that is independent of the response, but is able to mimic the dependence structure of the response can lead to spurious results” (DL)]. Methods like the Lasso coupled with pseudoproxies like AR1(Empirical) and Brownian motion will naturally account for this self-prediction (see also Kaplan), whereas naive CPS with latitude-based weighting will not (CPS using univariate correlation weights does, however; see SI).

2.2. *Specific objections.* Specific objections to the results of Section 3 came in two flavors: (i) criticism of the specific choices we made to get the results presented in Figures 9 and 10 of the paper, and (ii) questioning the legitimacy of the entire exercise. The specific criticisms of our choices were as follows:

(1) The use of the Lasso [Craigmile and Rajaratnam (CR), Haran and Urban (HU) Rougier, SMR, Tingley, WA]. This is a particularly interesting criticism since some of our critics (e.g., Kaplan, Rougier) seem to think the Lasso is a strong method for this context whereas others (e.g., Tingley, WA) think it weak.

(2) The use of 30-year holdout blocks (SMR, Smerdon, Tingley).

(3) The use of interpolated holdout blocks versus extrapolated holdout blocks (Rougier, Tingley).

(4) Calibrating our models directly to NH temperature rather than using local temperatures (Berliner, HU, NL, Tingley).

We are able to show, by brute force computation, that our results are invariant to these choices. Furthermore, as stated in our paper, we implemented many of these proposals prior to submission (for discussion of variations originally considered and justification of our choices, see Section 3.7 for the Lasso; footnote 8 for 30-year blocks; Section 3.4 for interpolation; and Section 3.6 for calibration to local temperatures). In contrast, we credit MM for pointing out the robustness of these results and Kaplan for actually demonstrating it by using Ridge regression in place of the Lasso (see Kaplan Figures 1 and 2). We direct the reader to our SI where we perform the same tests (1) for a plethora of methods (including the elastic net called for by HU and the Noncentral Lasso called for by Tingley), (2) using 30- and 60-year holdout blocks, (3) using both interpolated and extrapolated blocks, and (4) fitting to the local temperature grid as well as CRU when feasible. Once again, the results demonstrated by Figures 9 and 10 of our paper are robust to all of these variations.

The second criticism is more philosophical. WA allege AR1(Empirical) and Brownian motion pseudoproxies are “overly conservative” (a theme echoed to some extent by HU and SMR) and that “there is an extensive literature contradicting McShane and Wyner’s (2011a) assertions about low or poor relationships between proxies and climate.” We respond by noting our pseudoproxies come much closer to mimicking “the nonlinear, multivariate, nonstationary characteristics of noise in many proxy series” [Smerdon and Kaplan (2007)] and by again reflecting on the scope of our observations. Our paper demonstrates that the relationship between proxies and temperatures is too weak to detect a rapid rise in temperatures over short epochs and to accurately reconstruct over a 1000-year period. While there is literature that disagrees with our conclusions, our explanation is broadly analogous to the statistical significance results for CPS presented in Figure 5: the relationship between proxies and temperature looks good only for a weak method and when the self-predictive power of the short NH temperature sequence (DL) is not properly accounted for.

When it is properly accounted for, statistical insignificance ensues as demonstrated ably by Kaplan. We therefore endorse Kaplan's assertion that proxies (whether coupled with ARMA-like models or alone) must demonstrate statistical significance *above and beyond* ARMA-only models (Kaplan's "ability to correct") and agree with his suggestion for further research on the matter.

**3. Two points on Section 4.** Only two of the discussants (MM and SMR) commented on Section 4 of our paper. In it, we showed that 27 methods have very similar instrumental period holdout RMSEs yet provide extremely different temperature reconstructions [see also Burger and Cubasch (2005)]. This remains true whether one uses the dataset of 93 proxies from the paper or the dataset of 55 proxies favored by SMR, or whether one uses 30- or 60-year holdout blocks (see SI; it also appears to broadly hold for data simulated from climate models as well [Lee, Zwiers and Tsao (2008), Christiansen, Schmith and Thejll (2009), Smerdon et al. (2010)]). Thus, based on predictive ability, one has no reason to prefer "hockey sticks" to "inverted check marks" or other shapes and MM are correct to label this "a phenomenon that very much complicates the uncertainty analysis."

Also unremarked upon was our point that the proxies seem unable to capture the high levels of and sharp run-up in temperatures experienced in the 1990s, even *in-sample* or in contiguous holdout blocks. It is thus *highly improbable* that they would be able to detect such high levels and sharp run-ups if they indeed occurred in the more distant past. That is, we lack statistical evidence that the recently observed rapid rise in temperature is historically anomalous.

**4. Section 5 revisited: Bayesian reconstruction.** We have received a great deal of criticism for our Bayesian reconstruction of Section 5: for not fully modeling the spatio-temporal relationships in the data (Berliner, HU, NL, Tingley, SMR), for using a direct approach rather than an indirect or inverse approach (HU, MM, NL, Rougier), for linearity (Berliner), and for other features.

The purpose of our model in Section 5 was not to provide a novel reconstruction method. Indeed, when considering a controversial question which uses controversial data, new methodologies are likely to only provoke additional controversy since their properties will be comparably unknown relative to more tried methods. Rather, we sought a straightforward model which produces genuine, properly calibrated posterior intervals and has reasonable out of sample predictive ability. As Sections 4 and 5 of our paper as well as Section 1.3 show, our model achieves this, providing reconstructive accuracy as good if not better than the RegEM method as well as intervals which are properly calibrated. Thus, we take Rougier's characterization of our model ("perfectly reasonable ad-hockery") as high praise. We believe this simple approach is apt, especially for such a noisy setting. A further virtue of simplicity is that the model's assumptions are transparent and therefore easy to test and diagnose. Finally, we believe our model is still among the more

sophisticated models used to produce reconstructions from real proxy data. Thus far, other more sophisticated approaches have only been applied to simulated data.

We now turn to the putatively more sophisticated approaches advocated by our critics [see NL for a very clear exposition; see also [Tingley and Huybers \(2010\)](#) and [Li, Nychka and Amman \(2010\)](#)]. While these models have potential advantages, such as a richer spatio-temporal structure, our experience with real temperature and proxy data causes us to be a bit more circumspect. These models make a large number of assumptions about the relationships among global temperature, local temperatures, proxies, and external forcings. We would like to see a more thorough investigation of these assumptions because they do not seem to apply to real data (e.g., how does DL's finding that proxies appear to lead temperature by 14 years square with such models?). Furthermore, there are even deeper assumptions embedded in these models which are difficult to tease out and test on real data.

Hence, we strongly believe that these models need to be rigorously examined in light of real data. How do they perform in terms of holdout RMSE and calibration of their posterior intervals? How about when they are fit to various noise pseudoproxies as in our Section 3? When replicated data is drawn from the model conditional on the observed data, does the replicated data “look” like the observed data, especially in terms of prominent features? In sum, while we believe these models have much to recommend for themselves and applaud those working on them, we also strongly believe that tests like those employed in Section 3 of our paper, Section 1.4 of this rejoinder, and various other posterior predictive checks [[Gelman et al. \(2003\)](#), [Gelman and Hill \(2006\)](#)] are absolutely vital in the context of such assumption-laden models.

As for the indirect “multivariate calibration” approach suggested by some of the discussants, we point out that it was designed for highly-controlled almost laboratory-like settings (e.g., chemistry) with very tight causal relationships. The relationships between temperature and proxies is considerably dissimilar. Furthermore, we believe the two approaches, direct and indirect, ought not differ much in terms of  $\hat{y}$ , suggesting that “both types of procedures should be able to yield similar results, else we have reason for skepticism” [[Sundberg \(1999\)](#)]. While one approach or the other might give better predictions or confidence intervals in this application or that [a fact that has been observed even in climate settings; [ter Braak \(1995\)](#)], we believe Sections 4 and 5 of the paper and Section 1 of the Rejoinder suggest our model is adept at both prediction and interval estimation.

We return to Kaplan's subtle point that the proxies do not necessarily need to out-predict ARMA-like models, rather that they must simply provide additional benefits when added to such models. This is a trenchant point and the dangers of not evaluating proxy reconstructions in light of ARMA models is illustrated in Figure 6. The Bayesian AR2 + PC10 model in the upper left and the Bayesian PC10 model in the upper right provide essentially identical reconstructions. While the PC10 model has a somewhat smaller total posterior interval, the more striking feature is the disparity in the decomposition. In the AR2 + PC10 model, most of the

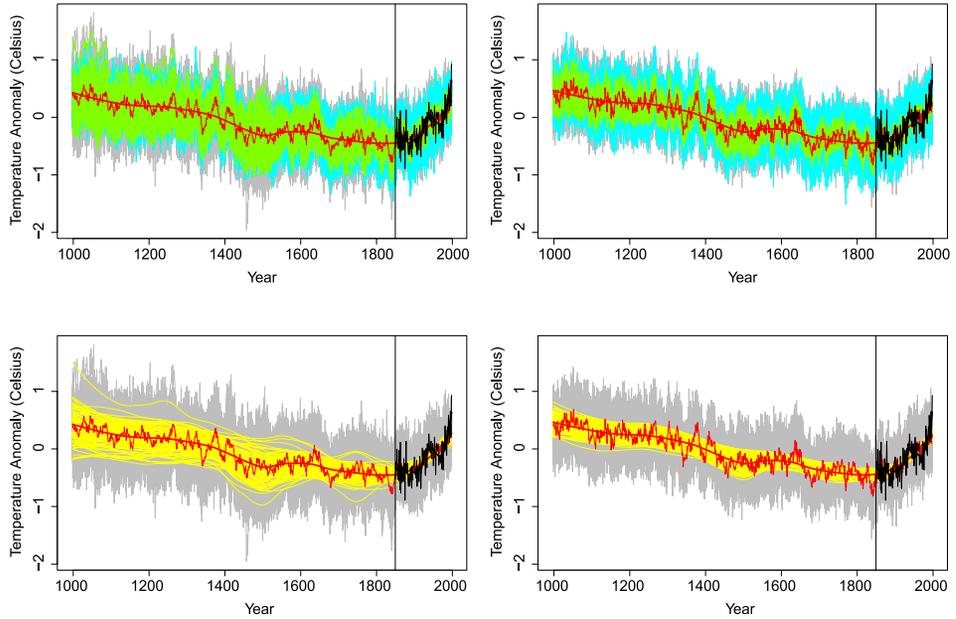


FIG. 6. *Bayesian backcasts and uncertainty decompositions.* In the upper left panel, we re-plot the Bayesian AR2 + PC10 model from Figure 16 of the paper: CRU Northern Hemisphere annual mean land temperature is given by the thin black line and a smoothed version is given by the thick black line. The forecast is given by the thin red line and a smoothed version is given by the thick red line. The model is fit on 1850–1998 AD and backcasts 998–1849 AD. The cyan region indicates uncertainty due to  $\varepsilon_t$ , the green region indicates uncertainty due to  $\vec{\beta}$ , and the gray region indicates total uncertainty. In the upper right panel, we give the same plot for a Bayesian PC10 model with no AR coefficients. In the bottom panels, we re-plot each model's backcast and total uncertainty. We also provide smooths of each posterior reconstruction path in yellow.

uncertainty is due to  $\vec{\beta}$  as indicated in green; for the PC10 model, the uncertainty due to  $\varepsilon_t$  and  $\vec{\beta}$  are more equal in their contribution to total uncertainty.

This has dramatic implications for, among other things, smoothed reconstructions as shown in the bottom two panels. Smoothing has the effect of essentially eliminating all uncertainty due to  $\varepsilon_t$ . Thus, the yellow region in the bottom right plot is extremely narrow (which explains why confidence intervals in the climate science literature are typically so narrow; see Figure 17 of the paper). On the other hand, when an AR2 structure is added, even smoothed confidence bands are quite wide. This is a profoundly important point which highlights the necessity of modeling the temporal dependence of the NH temperature series and we thank Kaplan for raising it.

**5. Statistical power.** Our results of Section 3 do not depend on the Lasso and are robust to changes in the null distribution (i.e., the pseudoproxies), the fitting algorithm, the holdout period length and location, and the calibration target series.

Nonetheless, there was substantial criticism of the Lasso by a number of discussants (CR, HU, Rougier, SMR, Tingley, WA) and worries that our tests lacked statistical power (Smerdon). In this section, we discuss two of those criticisms (Tingley and Smerdon) and show that lack of power may be intrinsic to the data at hand.

5.1. *Tingley.* Tingley asserts that the Lasso “is simply not an appropriate tool for reconstructing paleoclimate” and purports to show this via a simulation study which has two components. The second of the two components (featured in the right-hand plots of Tingley Figure 1 and in Tingley Figure 2) does an exemplary job of showing how the Lasso can use autocorrelated predictors in order to provide excellent fits of an autocorrelated response series—even when the response and predictors are generated independently.

The first component of the simulation (featured in the left-hand plots of Tingley Figure 1) is problematic for a number of reasons. First, it is not clear to us how this simulation relates to proxy-based reconstruction of temperature. If one compares and contrasts plots of Tingley’s simulated data (see SI) to Figures 5 and 6 of the paper, one sees that his target “temperature” series fails to look like the real temperature series and his pseudo-proxies fail to look like the real proxies.

Second, Tingley implements the Lasso in a completely nonstandard way: “The Lasso penalization parameter [ $\lambda$  on page 13 of McShane and Wyner (2011a)] is set to be 0.05 times the smallest value of  $\lambda$  for which all coefficients are zero.” There is no apparent statistical justification for this choice, and, when  $\lambda$  is selected through ten repetitions of five-fold cross-validation (as is done throughout our paper), the Lasso RMSE is twice as good as in Tingley’s Figure 1 (see SI).

Third, we must consider the two methods under consideration. This simulation is *exactly* the kind of situation where the Lasso is known to perform poorly. When one has identical predictors each with the same coefficient, “the Lasso problem breaks down” and methods like ridge regression are superior [Zou and Hastie (2005), Friedman, Hastie and Tibshirani (2010)]. Furthermore, Tingley’s benchmark of composite regression is both unrealistically good for this simulation and utterly nonrobust (furthermore, it also fails to reject the null tests of Section 3; see SI).

Composite regression performs unrealistically well because it is a univariate linear regression of  $y_t$  on a series which roughly equals  $y_t + v_t$  where  $v_t \stackrel{\text{i.i.d.}}{\sim} N(0, \sigma_\omega/\sqrt{1138})$  (where  $\sigma_\omega$  is set to various levels by Tingley). It is impossible for any method to perform comparably well against such an ideal procedure (one that has asymptotic zero RMSE as the number of pseudoproxies goes to infinity). Ridge regression, known to perform well in settings like this simulation, does 1–6 times worse than composite regression (see SI) and is therefore not much better than the Lasso (in fact, it is worse for the high noise settings). Even the true data-generating model for  $y_t$  with the true parameters—another unrealistically strong

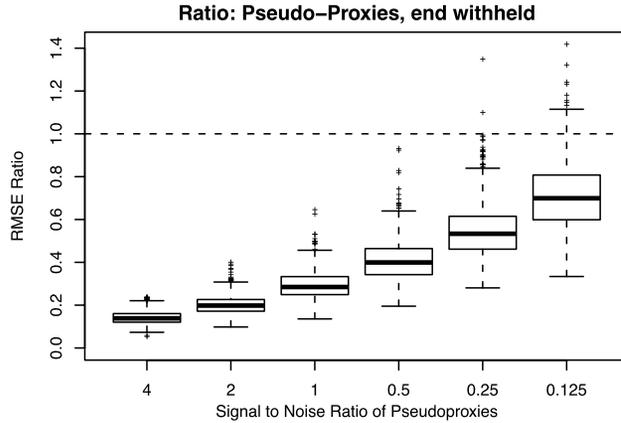


FIG. 7. *Tingley simulation perturbed with  $\sigma_\beta = 3$ . We plot the ratio of the RMSE of the Lasso to that of composite regression. Compare to the lefthand plots of Tingley Figure 1. In the SI, we give the raw RMSEs of two methods as well as their ratio for this value of  $\sigma_\beta$  and others.*

model—performs about 13 times worse than composite regression in some settings (see SI).

Additionally, composite regression lacks robustness to slight but realistic perturbations of the simulation. For instance, consider setting  $x_{t,i} = \beta_i y_t + \omega_{t,i}$  where  $\beta_i \stackrel{\text{i.i.d.}}{\sim} N(1, \sigma_\beta = 3)$  (Tingley’s simulation corresponds to  $\sigma_\beta = 0$ ). RMSE box-plots for this simulation appear in Figure 7. In this case, the Lasso dominates composite regression by more than a factor of five in the lowest noise case. In fact, composite regression appears to do arbitrarily badly relative to the Lasso for high values of  $\sigma_\beta$  (see SI for  $\sigma_\beta = 1/3, 1, 9$ , and  $27$  in addition to the  $\sigma_\beta = 3$  presented here).

Thus, it is not the Lasso but the simulation that is broken: the Lasso is used in a setting where it is known to perform poorly, is implemented in a non-standard fashion, and is pitted against an unrealistically good and nonrobust competitor model. Furthermore, it is unclear how this simulation relates to proxy-based temperature reconstruction.

Tingley’s simulation does, however, raise a subtle issue. He shows the Lasso, when fit to strong AR1 and Brownian motion pseudoproxies that contain no signal, can provide better predictions than when fit to some of his pseudoproxies which do contain signal. Taken together, this suggests that we may never find statistical significance when given many weakly informative proxy series and thus we lack power.<sup>8</sup> We argue that if this is the case (as it indeed may be; see our discussion of

<sup>8</sup>Though it is beyond the scope of our work, we note that, by making use of additional information (e.g., the spatial locations of the proxies and local temperatures), it is possible that the proxies might become considerably more predictive/informative than they have so far proven to be.

Smerdon), it is something endemic to all methods, even his composite regression and the Noncentral Lasso (see SI).

As a final point, Tingley claims it “is simply not the case” that dimensionality reduction is necessary for the paleoclimate reconstruction problem, and he suggests Bayesian hierarchical models as “a more scientifically sound approach.” This is an odd comment since Bayesian hierarchical models are well known to reduce dimensionality in the parameter space via partial pooling [Gelman et al. (2003), Gelman and Hill (2006)]. We thus reiterate our claim that dimensionality reduction is intrinsic to the endeavor.

5.2. *Smerdon*. Smerdon suggests a highly sophisticated test of whether or not the Lasso has power in this paleoclimate context. His simulation technique is to increasingly corrupt local temperatures and compare how the Lasso performs on these series to the proxy and pseudoproxy series. This is quite clever since, by definition, local temperatures contain signal for NH temperature.

The first thing of note that Smerdon shows is that “even ‘perfect proxies’ are subject to errors” (see Smerdon Figure 1a which is reproduced as the top left panel of Figure 8), a fact we have noticed in our own work (see SI). Local instrumental temperatures have substantial error when predicting CRU NH temperature on holdout blocks.

Smerdon also shows that the proxies perform similarly to local temperatures corrupted with either 86% red noise or 94% white noise (see the top left panel of Figure 8 which reproduces Smerdon Figure 1a). On the other hand, our AR1(Empirical) and Brownian motion pseudoproxies outperform the corrupted temperatures suggesting that our test rejects even “proxies” known to have signal (albeit a highly corrupted one). We agree with Smerdon that these results come with a number of caveats,<sup>9</sup> but we believe they warrant more reflection.

Smerdon says “skillful CPS reconstructions (latitude-based weights) can be derived from such predictors” (i.e., from corrupted temperatures) and presents Smerdon Figure 1b as evidence. This figure is problematic and misleading for a number of reasons. First, it is entirely in-sample. Second, it omits the Lasso’s performance. In fact, the Lasso gives a lower RMSE at the same task (CPS has an RMSE of 0.223 and 0.266 on the 86% red noise and 94% white noise corrupted temperatures respectively whereas the Lasso has 0.070 and 0.108, respectively).

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<sup>9</sup>Smerdon samples the temperature grid only once and he samples from the whole globe as opposed to either sampling from the NH or using the locations of the Mann et al. (2008) proxies. He also samples the noise series only once for each setting. Furthermore, he conducts only one repetition for each holdout block. Finally, he compares the Lasso performance on 283 predictors constructed from local temperatures to performance on 1138 proxies and noise pseudoproxies, thus lowering  $p$  substantially. We believe consideration of these factors are unlikely to alter the basic picture presented in the top left panel of Figure 8. However, it would likely increase the variance of the various boxplots thus making the differences less stark. Moreover, it would be interesting to see the RMSE distributions from holdout block to holdout block along with intervals for resampled temperatures and noise pseudoproxies (i.e., in the style of Figure 10 versus Figure 9 of the paper).

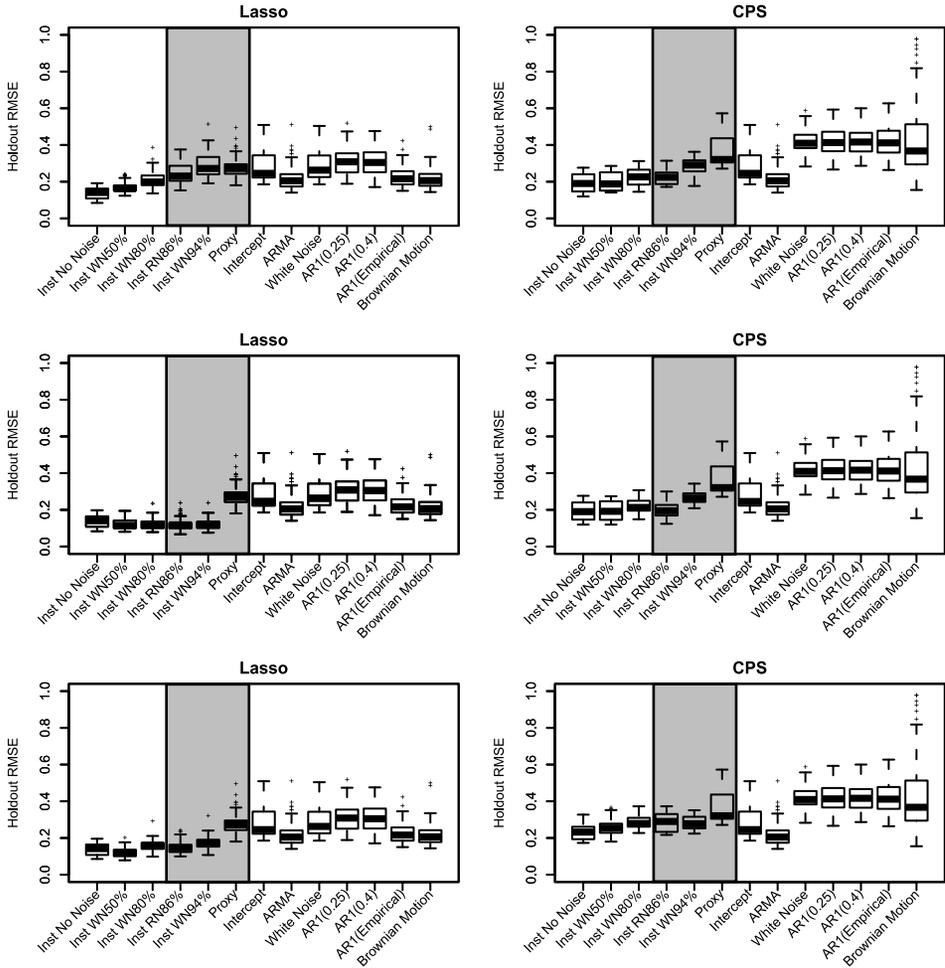


FIG. 8. Comparison of Lasso and CPS on various pseudoproxies. In the top left, we plot give hold-out RMSEs for the Lasso trained using Smerdon’s instrumental period local temperature pseudoproxies (this figure replicates Smerdon Figure 1a). In the top right, we give the same plot for CPS. In the middle row and bottom rows, we consider two variants of Smerdon’s pseudoproxies (see text for more detail). Compare to Smerdon Figure 1a and the SI.

A fairer comparison would be to use the test of Smerdon Figure 1a on CPS, which we present in the top right panel of Figure 8.<sup>10</sup> As can be seen, CPS has lower RMSEs on the corrupted instrumental temperatures than on noise pseudoproxies such as AR1(Empirical) and Brownian motion. The proxies also appear

<sup>10</sup>The eight right-most boxplots of the top panel of Figure 8 should be reminiscent of Figure 5. They are identical except the former is based on one repetition for each holdout block whereas the latter averages over 100 such repetitions thus decreasing variation.

to perform better than these noise pseudoproxies. So it seems that CPS passes Smerdon's test. On the other hand, the Lasso performs equivalently to or better than CPS in all 13 cases. Hence, it does not seem that CPS reconstructions are so skillful after all since they are worse than those of the Lasso.

As mentioned in the response to Tingley, the Lasso is known to perform poorly in situations where all the predictors are the same. This is approximately the situation governing the five leftmost boxplots in the two upper panels of Figure 8. In contrast, we consider two variants of Smerdon's simulation. The first variant, plotted in the second row of Figure 8, defines noise percentage differently. Rather than adding noise to local temperatures such that the variance of the noise accounts for some fixed percentage of the variance of the sum, we add predictors to the matrix of local temperatures such that a fixed percentage of the predictors are pure noise (e.g., for 50% white noise, the matrix of predictors has 566 columns, 283 of local temperatures and 283 of white noise). The Lasso performs spectacularly well in this setting and seems fully powered with the local temperatures always outperforming various noise series. On the other hand, CPS performs similarly in this case as in Smerdon's example: local temperatures outperform noise series but the predictions on the whole are quite poor.

The second variant of Smerdon's simulation replicates what we did with Tingley's simulation. Rather than using local temperature plus noise as predictors, we used a random slope times local temperature plus noise where the slopes were i.i.d.  $N(1, \sigma_\beta = 3)$ . In some sense, this better reflects the relationship between proxies and temperature. In this setting, the Lasso again performs very strongly with the corrupted local temperatures always outperforming various noise series. CPS gives worse results across the board. For results using different values of  $\sigma_\beta$ , see SI.

In sum, when predictors have approximately the same coefficient *and* there is a very high noise level (e.g., the 86% and 94% noise conditions of the top left panel of Figure 8), the Lasso is perhaps underpowered. In variants of the simulation that might be more true to real data (the middle left and bottom left panels), the Lasso performs very well. On the other hand, CPS performs weakly in all settings: it simply does not provide particularly good out of sample predictions compared to methods like the Lasso.

We are left wondering why the Lasso "fails" Smerdon's test, suggesting a lack of power. Low power can be explained by recognizing an unavoidable reality: the NH temperature sequence is short, highly autocorrelated, and "blocky" (DL observe "3 or possibly 4 segments"). Thus, the effective sample size is far smaller than  $n = 149$ . Consequently, as is always the case with small samples, one lacks power. It therefore follows (as shown in Section 2.2 and the figures in the Appendix to the SI) that failure to reject the null against AR1(Empirical) and Brownian motion pseudo-proxies is not specific to the Lasso. Rather, it is endemic to the problem. Unless we can find proxies that strongly predict temperature at an annual level, power will necessarily be low and uncertainty high.

**6. Conclusion.** In conclusion, we agree with Berliner that statisticians should “not continue with questionable assumptions, nor merely offer small fixes to previous approaches, nor participate in uncritical debates” of climate scientists. Nonetheless, we believe that these assumptions (linearity, stationarity, data quality, etc.) were clearly stated in the beginning of our paper and were not endorsed. We believe it was important to start with these questionable, perhaps even indefensible, assumptions to engage the literature to date and to provide a point of departure for future work.

We also reiterate our conclusion that “climate scientists have greatly underestimated the uncertainty of proxy-based reconstructions and hence have been overconfident in their models.” In fact, there is reason to believe the wide confidence intervals given by our model of Section 5 are optimistically narrow. First, while we account for parameter uncertainty, we do not take model uncertainty into account. Second, we take the data as given and do not account for uncertainties, errors, and biases in selection, processing, in-filling, and smoothing of the data as well as the possibility that the data has been “snooped” (subconsciously or otherwise) based on key features of the first and last block. Since these features are so well known, there is absolutely no way to create a dataset in a “blind” fashion. Finally, and perhaps most importantly, the NRC assumptions of linearity and stationarity [NRC (2006)] outlined in our paper are likely untenable and we agree with Berliner in calling them into question. While the “infinite confidence intervals” of the [Brown and Sundberg \(1987\)](#) test reported by MM are unrealistically large due to physical constraints, we agree with their central point that this matter warrants closer examination since it is absolutely critical to assessing statistical significance and predictive accuracy.

As a final point, numerous directions for future research appear in this paper, discussion, and rejoinder. We hope statisticians and climate scientists will heed the call on these problems.

**Acknowledgments.** We thank Jason Smerdon for a number of helpful conversations about data, methods, and code. Those conversations were tremendously fecund and greatly enhanced our rejoinder. We believe such conversations to be paradigmatic of the great value of collaboration between climate scientists and statisticians and hope they can serve as a template for future work between the two camps. We also thank Steve McIntyre for several very helpful discussions about data and code.

#### SUPPLEMENTARY MATERIAL

**Supplement A: Long form rejoinder: A statistical analysis of multiple temperature proxies: Are reconstructions of surface temperatures over the last 1000 years reliable?** (DOI: [10.1214/10-AOAS398REJSUPPA](https://doi.org/10.1214/10-AOAS398REJSUPPA); .zip). This document is the long form of “Rejoinder: A statistical analysis of multiple temperature

proxies: Are reconstructions of surface temperatures over the last 1000 years reliable?” It contains all the text from the short form which appeared in print as well as the supporting details and figures for the claims made in that document.

**Supplement B: Code repository for “Rejoinder: A statistical analysis of multiple temperature proxies: Are reconstructions of surface temperatures over the last 1000 years reliable?”** (DOI: [10.1214/10-AOAS398REJSUPPB](https://doi.org/10.1214/10-AOAS398REJSUPPB); .zip). This repository archives all data and code used for “Rejoinder: A statistical analysis of multiple temperature proxies: Are reconstructions of surface temperatures over the last 1000 years reliable?” In particular, it contains code to make all figures and tables featured in the long form (which is a superset of those in the short form).

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