

Challenges in Combining Projections from Multiple Climate Models

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ABSTRACT

Recent coordinated efforts, in which numerous general circulation climate models have been run for a common set of experiments, have produced large datasets of projections of future climate for various scenarios. Those multimodel ensembles sample initial conditions, parameters, and structural uncertainties in the model design, and they have prompted a variety of approaches to quantifying uncertainty in future climate change. International climate change assessments also rely heavily on these models. These assessments often provide equal-weighted averages as best-guess results, assuming that individual model biases will at least partly cancel and that a model average prediction is more likely to be correct than a prediction from a single model based on the result that a multimodel average of present-day climate generally outperforms any individual model. This study outlines the motivation for using multimodel ensembles and discusses various challenges in interpreting them. Among these challenges are that the number of models in these ensembles is usually small, their distribution in the model or parameter space is unclear, and that extreme behavior is often not sampled. Model skill in simulating present-day climate conditions is shown to relate only weakly to the magnitude of predicted change. It is thus unclear by how much the confidence in future projections should increase based on improvements in simulating present-day conditions, a reduction of intermodel spread, or a larger number of models. Averaging model output may further lead to a loss of signal—for example, for precipitation change where the predicted changes are spatially heterogeneous, such that the true expected change is very likely to be larger than suggested by a model average. Last, there is little agreement on metrics to separate “good” and “bad” models, and there is concern that model development, evaluation, and posterior weighting or ranking are all using the same datasets. While the multimodel average appears to still be useful in some situations, these results show that more quantitative methods to evaluate model performance are critical to maximize the value of climate change projections from global models.

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1. Introduction

With climate change over the past 50 years or so now firmly established to be mostly due to human influence via the burning of fossil fuels (Solomon et al. 2007), concerns about future climate change of much larger magnitude than observed are increasing, and attention is increasingly directed toward projections from climate models in the hope for policy-relevant information about expected changes and guidance for appropriate mitigation and adaptation measures. The degree of confidence we place on model results, however, essentially depends on whether we can quantify the uncertainty of the prediction, and demonstrate that the results do not depend strongly on modeling assumptions. Since there is no direct verification of future changes' forecasts, model performance and uncertainties need to be assessed indirectly through process understanding and model evaluation on past and present climate.

Uncertainties in future projections stem from different sources and are introduced at various stages in the modeling process. Forcing uncertainties (reflected by different economic and societal developments and political decisions) are often circumvented by focusing on (specific) projections—that is, predictions conditional on an assumed scenario (e.g., Nakicenovic and Swart 2000). Initial and boundary conditions are mostly of minor importance for long-term climate projections. By far the largest contribution to uncertainty stems from the fact that climate models are imperfect and therefore their projections uncertain. This contribution can be further separated into model uncertainty because of limited theoretical understanding (inability to understand a process in the first place—for example, how aerosols affect cloud formation), uncertainty in model parameters, and structural model uncertainty (inability to describe a known process accurately in the model). Parametric uncertainty is introduced by the fact that for many small-scale processes in models, their large-scale effects need to be empirically described rather than resolved, and that the values in these parameterizations are not always well constrained and are not directly observable in the real world. Structural uncertainty (sometimes also termed model inadequacy) means that no set of parameters will make the model agree perfectly with observations (e.g., Sanderson et al. 2008), because certain processes are missing or are only approximated in the model [see, e.g., Stainforth et al. (2007) and Knutti (2008a) for a more detailed discussion].

One way to study uncertainty is to consider results from multiple models. The “multimodel” approach provides a sensitivity test to the models' structural choices. Additionally, an implicit assumption exists that multiple

models provide additional and more reliable information than a single model (see section 2), and higher confidence is placed on results that are common to an ensemble, although in principle all models could suffer from similar deficiencies. But for the nonexpert, a collection of results is often most useful when combined and synthesized. The motivating question behind this study is how model trustworthiness can be increased by combining results of multiple models.

2. Model diversity: Potentials and challenges

Different scientific questions require different models in terms of resolution, components and processes, and spatial domain. However, there are also families of models of the same type—that is, multiple models incorporating the same set of processes at similar resolutions. They partly sample the structural model uncertainty and can be seen as multiple credible approximations of the truth, given some constraints in complexity and computational cost. These are often seen as coexisting rather than competing models (Parker 2006). While two models may make assumptions on smaller scales that could be seen as inconsistent, both models would agree with observations within some uncertainty (typically a sum of observational uncertainty and the structural model error) and would therefore be considered plausible. These model families are usually either variants of a single base model with perturbed parameters [so-called perturbed physics ensembles (PPEs); e.g., Forest et al. 2002; Knutti et al. 2002; Murphy et al. 2004; Stainforth et al. 2005] or multimodel ensembles (MMEs)—that is, a somewhat arbitrary collection of different models of similar structure and complexity (e.g., Eyring et al. 2007; Plattner et al. 2008). The ensemble used here is from the recent World Climate Research Programme (WCRP) Coupled Model Intercomparison Project phase 3 (CMIP3; Meehl et al. 2007b) and consists of 23 state-of-the-art atmosphere–ocean general circulation models (AOGCMs) from 16 institutions and 11 countries. One ensemble member for each model is used. The CMIP3 MME provided the basis for the projections of the latest Intergovernmental Panel on Climate Change (IPCC) Fourth Assessment Report (AR4; Solomon et al. 2007). An extensive discussion and evaluation of these models and an overview of the projections are given in the relevant IPCC chapters (Christensen et al. 2007; Hegerl et al. 2007; Meehl et al. 2007a; Randall et al. 2007). An overview of CMIP3 is given by Meehl et al. (2007b), and a list of models and institutions is also provided by Gleckler et al. (2008). The data are available from the Program for Climate Model Diagnosis and Intercomparison (PCMDI) Web

site (available online at http://www-pcmdi.llnl.gov/ipcc/about_ipcc.php). The detailed structure of these models, the resolution, and the exact number of models, however, are not relevant for the issues discussed here. Issues will be similar for future multimodel ensembles, and some of the conclusions are likely to apply to other research fields where predictive models need to be combined. Most of the discussion similarly applies to PPEs, but since structural errors are likely to be even more persistent when all models share the same core, PPEs may, in fact, offer even harder challenges.

a. Prior distribution

Synthesizing MMEs or PPEs is a problem that can be cast in a Bayesian framework, where a prior distribution determines the initial weight distribution of the sample of models or sample of predictions available for intercomparison, and data (observations) may serve to redistribute the weight among them on the basis of their performance (the likelihood used in the Bayesian analysis). The questions most relevant to this problem then are as follows: What is the prior distribution of these models? Is the sample randomly selected, or systematic, or neither? Are the data significantly constraining the final result or is the prior? We can fairly confidently answer the last question: for both PPEs and MMEs, data constraints are weak at best and do not change the shape and width of the prior distribution robustly (Frame et al. 2005). It seems crucial then to consider the prior distribution, and in this respect a fundamental difference exists between PPEs and MMEs. Because of computational costs, and the large number of model parameters (typically a few dozen), a comprehensive sampling in AOGCM space is impossible. Large PPEs such as climateprediction.net (Stainforth et al. 2005) use tens of thousands of members and can explore a wide range of solutions, but they are always structurally constrained to a single model. Most other AOGCM ensembles are small (i.e., a few tens of models). In either case a critical issue remains, that of the definition of a uniform prior in model space, which ideally would let the data have the greater impact on the final result. There is no absolute distance metric in model space, and uniformity can only be defined with respect to a given input or output quantity. Whatever distance metric is chosen, though, in a systematic or random sampling like PPEs (e.g., Forest et al. 2002; Knutti et al. 2002; Murphy et al. 2004; Stainforth et al. 2005), it is at least clear how the models are distributed; however, for MMEs like CMIP3, the models are sampled neither randomly nor systematically; the ensemble is determined by whichever modeling center had interest and resources to contribute. Most groups provide only their “best” model, so the

ensemble should be seen as a collection of carefully configured “best estimates” rather than an attempt to sample the range of all possible models. In an ensemble of opportunity where the number of models is small, the problem of not sampling the full uncertainty range may thus be severe. The ensemble sampling also changes from one intercomparison to the next, so projections in future ensembles may change just because of the prior sampling (i.e., model selection in the intercomparison), even if the understanding of the system does not change.

A simple case where the prior distribution was found to matter even within the set of CMIP3 models was the distribution of simulations across scenarios. Figure 1a shows the mean and one standard deviation global surface temperature ranges for the Special Report on Emissions Scenarios (SRES) A2, A1B, and B1 (Nakicenovic and Swart 2000) and the historical run from all CMIP3 models. The two lower emissions scenarios (B1 and A1B) appear to have a wider uncertainty range in 2100 than the high emission A2 case; although, on the basis of often-used pattern scaling arguments, one would expect the opposite, as the uncertainty scales approximately linearly with warming (Knutti et al. 2008). Figure 1b shows the standard deviations, minimum to maximum ranges in 2100 for all models, and the subset of models that have run all scenarios. The apparent contradiction in uncertainty ranges in Fig. 1a occurs simply because fewer models had run the higher A2 scenario. If the subset of models with all scenarios available is considered, then the uncertainty is strongly reduced for the lower emissions scenarios. Further details are given by Knutti et al. (2008). The uncertainty estimate based on the empirical distribution of the projections of these models can only be wider than the prior distribution of the ensemble if the variance is artificially inflated. This is a challenge, since it requires assessing the likelihood of outcomes that are not simulated by any model. Here we note that the consequences of underestimating the uncertainty from an ensemble of simulations have important repercussions when the simulations are used as input to impact models. That is, “best case” and “worst case” outcomes that could directly affect the severity of impacts may be missed in favor of more centrally distributed and less drastic outcomes.

b. Model averages, independence, and structural error

There is empirical evidence from various areas of numerical modeling that a multimodel average yields better prediction or compares more favorably to observations than a single model. Examples include health (Thomson et al. 2006), agriculture (Cantelaube and Terres 2005), predictions of the El Niño–Southern Oscillation (ENSO;

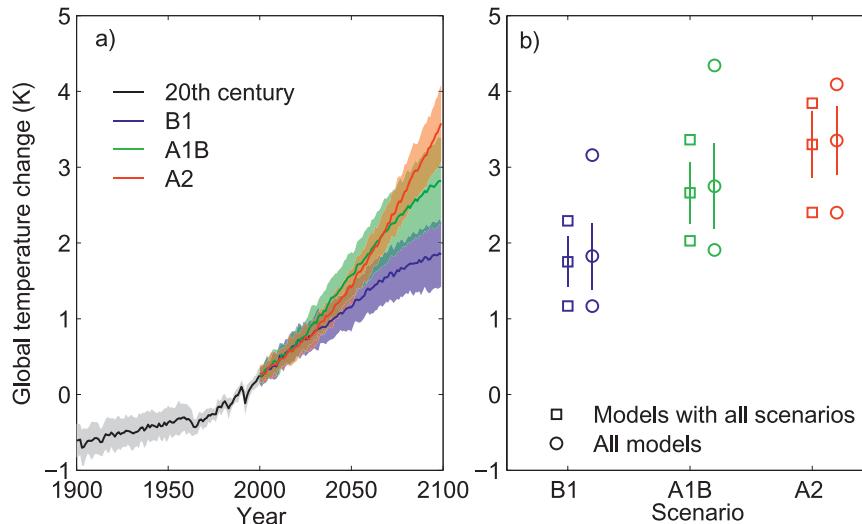


FIG. 1. (a) Multimodel mean and one std dev uncertainty ranges for global temperature (relative to the 1980–99 average for each model) for the historic simulation and projections for three IPCC SRES scenarios. (b) Mean and one std dev ranges (lines) plus minimum maximum ranges (symbols) for the subset of models that have run all three scenarios (squares) and for all models (circles). The model spread for the scenarios B1 and A1B depends strongly on what prior distribution of models is assumed.

Palmer et al. 2005), and detection and attribution (Gillett et al. 2005). Weather and seasonal forecasts show improved skill, higher reliability, and consistency when multiple models are combined (Krishnamurti et al. 1999; Doblas-Reyes et al. 2003; Yun et al. 2003). For a single variable, the multimodel combination might not be significantly better than the single best model, but a large benefit is seen when the aggregated performance on all aspects of the forecast is considered (Hagedorn et al. 2005). Models can simply be averaged (“one model, one vote”) or can be weighted—for example, using Bayesian methods, where weights are based on past relationships between forecasts and verifications. Weighted averages are found to perform better in many cases (Robertson et al. 2004; Min and Hense 2006; Peña and Van den Dool 2008; Weigel et al. 2008), provided that sufficient information is available to determine the weight (see section 2c).

For several generations of climate models, it has been shown that the multimodel average for a variety of variables mostly agrees better with observations of present-day climate than any single model, and that the average also consistently scores high in almost all diagnostics (Lambert and Boer 2001; Phillips and Gleckler 2006; Randall et al. 2007; Gleckler et al. 2008; Pincus et al. 2008; Reichler and Kim 2008; Pierce et al. 2009). While the improvement was sometimes quantified in these studies, it was rarely discussed whether the improvement was as large as expected and how it should relate to improvements in projections.

Near-surface temperature is used in the following section for illustration, because models can simulate temperature reasonably well and because good observations and reanalysis datasets are available, but similar results are expected for other variables. Figure 2a shows the mean bias of local temperature for a collection of single models for boreal winter and summer (the absolute bias at every grid point averaged across all models), whereas Fig. 2b shows the absolute value of the bias for the multimodel average. The multimodel average performs better in simulating the climatological temperature field. There is, indeed, improvement in some areas; however, other locations are almost unaffected by averaging, indicating that errors are similar in many models. The largest errors also tend to be in the same locations where model spread is large. These are often caused by known deficiencies in the models not resolving processes accurately because of resolution (e.g., convection or coastal upwelling in the ocean, topography of mountains), not representing processes well because of inappropriate parameterizations or poor parameter choice (e.g., tropical variability related to ENSO), or not representing them at all (forcings not considered, lack of vegetation model, among others). Note that observations (whether reanalysis or station data) are not always accurate and also exhibit biases. This point is discussed at the end of this section.

A plausible explanation why the biases are not reduced is if the errors are not random but correlated across models. A histogram of all pairwise correlation

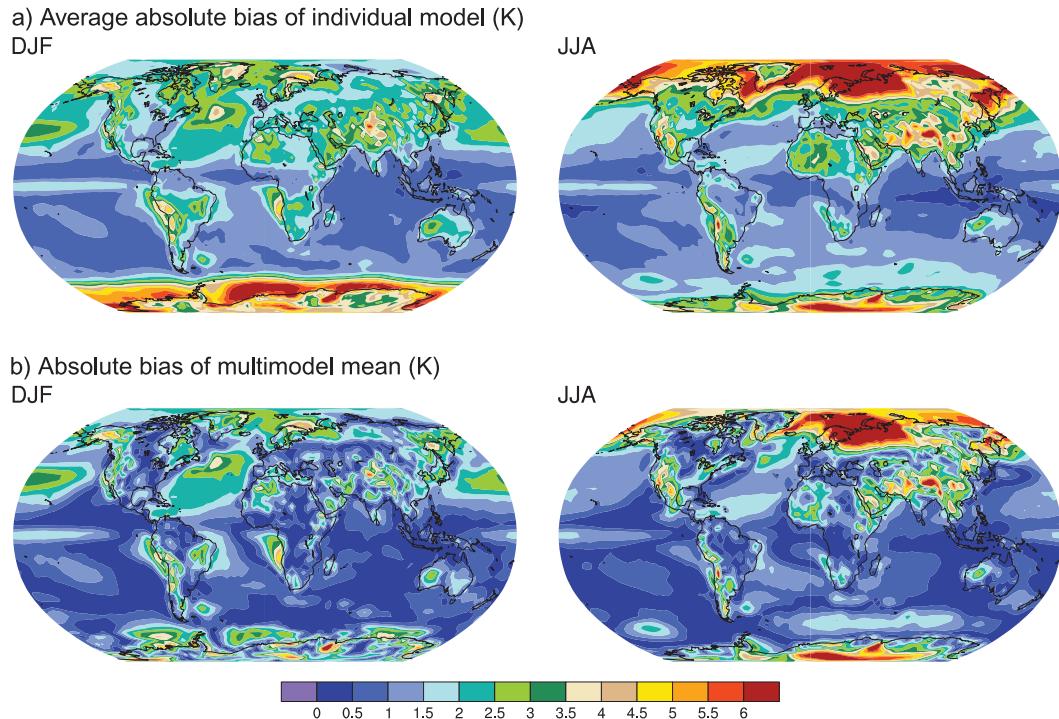


FIG. 2. (a) Absolute bias in 1970–99 average surface temperature from ERA-40, averaged across all CMIP3 models for DJF and JJA. (b) Same as in (a), but bias shown for the multimodel average. In some locations, biases from observations are reduced but the improvement by averaging is very heterogeneous.

values of two-model bias patterns (Figs. 3a and 3b) supports that. Correlations are largely positive and reach values up to 0.9 in cases where the two models from the same institution [e.g., the two Geophysical Fluid Dynamics Laboratory (GFDL) models] or where two versions of the same model but different resolution [e.g., Canadian Centre for Climate Modelling and Analysis (CCCma)] are compared. The result is that simple averaging is not very effective. It is instructive to study how the bias is reduced as the size of the ensemble used in the average is increased. Figures 3c and 3d show the root-mean-square (RMS) bias of the model average as a function of the number of models (i.e., averaging the models first, calculating the bias, and then averaging over space to estimate a typical local bias of the model average). The solid red curve shows the average, resulting from taking many different subsets of models; the red dashed lines indicate the range covered by different random subsets. If all model errors were random in a large sample of models, then the error of the mean should decrease with the square root of the number of models (black dotted lines). Indeed, it does, but not to zero but to a residual that is more than half the initial value. If we assume a correlation structure between the grid points of a pattern—for example, as illustrated in Figs. 3a and 3b—then it is possible to calculate the theoretical RMS

(Figs. 3a and 3b, black dashed) and to show that it converges to $\sigma\sqrt{\rho}$, where σ is the variance of the pattern and ρ is the average correlation (see Fig. 3 caption for the full equation).

The interesting conclusions from Figs. 3c and 3d are that for present-day temperature, half of the typical biases would remain even for an average of an infinite number of models of the same quality. The remaining bias for 22 models is 2–3 times larger than if the models were independent and the errors were purely random. Considerable improvement is only seen for up to about 5 models, and after 10 models the biases are almost stable. The blue lines indicate the bias of the subset of the two best models [best in this case meaning the general circulation models (GCMs) whose December–February (DJF) and June–August (JJA) surface temperatures for the period 1980–99 agree most closely with 40-yr European Centre for Medium-Range Weather Forecasts Re-Analysis (ERA-40)], three best models, and so on, and they suggest that a few good models are better than the multimodel average; the average soon gets worse when poorer models are added. It should be noted here that the models that are best for temperature are not necessarily best for other quantities, but there is a tendency for good models to score high on many diagnostics (Gleckler et al. 2008). This reflects, in part, the amount

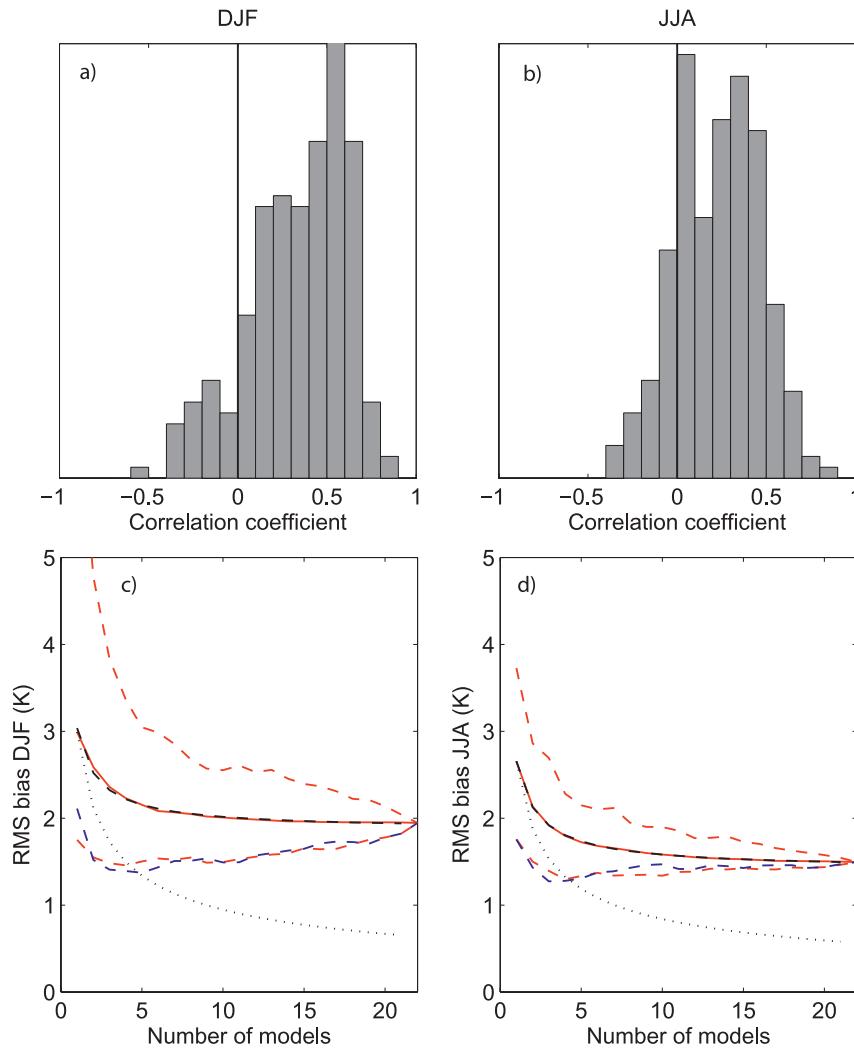


FIG. 3. (a),(b) Histograms of correlation coefficients for all possible pairs of 1980–99 surface temperature bias maps of the CMIP3 models for DJF and JJA. For independent bias patterns, correlations should be distributed around zero. Positive correlations indicate that biases have similar patterns. (c),(d) RMS error of 1980–99 surface temperature (averaged over space, relative to ERA-40) shown as a function of the number of models included in the model average. Red dashed indicates the range covered by randomly sampling the models for the subset; red solid line indicates the average. The RMS error converges to a constant value that is more than half of the initial value for one model. The black dashed line is the theoretical RMS based on the correlation structure similar to (a),(b) and is given by $\sigma[(1 + (N - 1)\rho)/N]^{1/2}$ with σ the variance and ρ is the average correlation. In this case σ and ρ were chosen to fit the red solid line. If the model biases were independent, then the RMS error for a large sample of models should decrease with the square root of the number of models (dotted). The blue line results if the models are sorted by how well they agree with DJF and JJA observations combined, and it indicates that the average of a few good models outperforms an average of more models with poorer performance.

of effort going into the development of a model but also the fact that many variables in the climate are linked, such that biases in one variable will lead to biases in many others.

The reduction of biases by averaging depends not only on the geographical location but also on the magnitude

of the initial bias. Figure 4 shows the distribution of present-day temperature biases in present-day climatology for each model, the multimodel mean, and the average of the five best models (with regard to simulating temperature, as described earlier). The distribution of the model average has more pronounced long

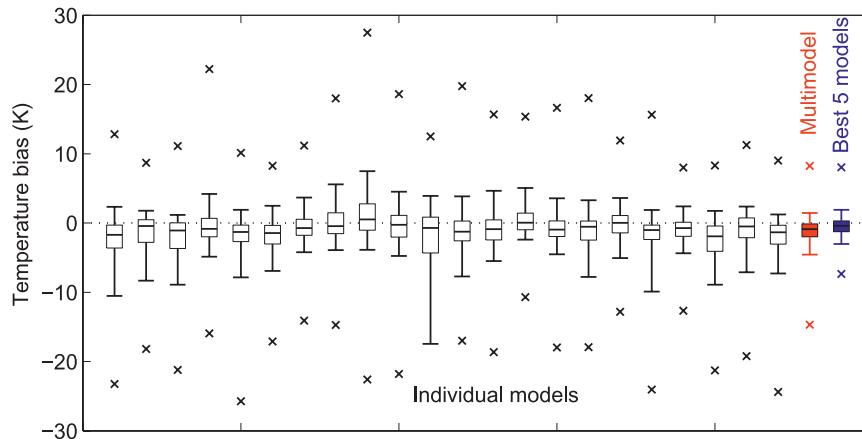


FIG. 4. Box plots of surface temperature biases from ERA-40 for all models and grid boxes, along with the average of all models (red) and the average of the five models that show the smallest RMS temperature biases (blue). The box marks the median and interquartile range, the line marks the 5%–95% range, symbols mark the minimum-to-maximum range. One minimum value is out of range and not shown. While averaging reduces the biases, the tails of the distributions shrink proportionally less than the central part of the distributions.

tails than the single models; that is, the extreme errors are reduced less effectively than smaller amplitude errors, suggesting that there are, indeed, large errors resulting from processes that are similarly misrepresented in many models and therefore are hard to eliminate. As a caveat, observational and reanalysis datasets, of course, also have biases. For temperature these biases are quite small, and the conclusions would be similar for station data or other reanalysis datasets. But for evaluation on other quantities—for example, radiation or precipitation, where little or no data are directly assimilated—this is a major issue. Models used for reanalysis run at much higher resolution than those in CMIP3, but results for some variables are still based on parameterizations similar to those in CMIP3 models. In an analysis such as the one mentioned earlier, any bias in the observations (whether station or reanalysis) would appear as a model error persistent across all climate models, and the conclusions drawn earlier for temperature are only justified because the model errors are generally much larger than the uncertainties in the observations (see, e.g., Knutti et al. 2006, their Figs. 1 and 2).

c. Dependence of projection uncertainty on the number of models

As shown in the previous section, the biases of individual models are correlated. For the present-day state, sophisticated statistical methods also suggest that the equivalent number of independent models in CMIP3 is much smaller than the total number of models (Jun et al. 2008a,b). It is unclear how this model dependency structure maps into the future, and by construction it

is impossible to determine that because of the lack of “observed truth” for the future.

Physical understanding of the assumptions underlying the models suggest that the models are not independent and distributed around the truth, yet many Bayesian methods assume model independency (Giorgi and Mearns 2002, 2003; Tebaldi et al. 2004, 2005; Greene et al. 2006; Furrer et al. 2007b; Tebaldi and Knutti 2007; Smith et al. 2009; Tebaldi and Sansó 2009). The assumption of independence is equivalent to the interpretation that each model approximates the real world with some random error. Such as in a case where a quantity is measured with a random error, multiple measurements will improve the accuracy of the measurement average, and the uncertainty in the mean value will shrink with the square root of the number of measurements N as N increases (Lopez et al. 2006). The implication of the independence assumption is that uncertainties decrease as more models are considered, shown for illustration in Fig. 5 for the method of Furrer et al. (2007a,b) and Smith et al. (2009). Because these methods determine a central tendency common to all models rather than a posterior predictive of a single ideal model projection, it is not surprising that the uncertainty [measured in terms of the width of the probability density function (PDF)] decreases with more models. Thus, it comes down to a somewhat philosophical question of the quantity we are actually trying to estimate.

The signal of change underlying the truth and model simulations is the abstract concept whose uncertainty is characterized by the posterior distribution, which, in these Bayesian treatments, decreases in width with the

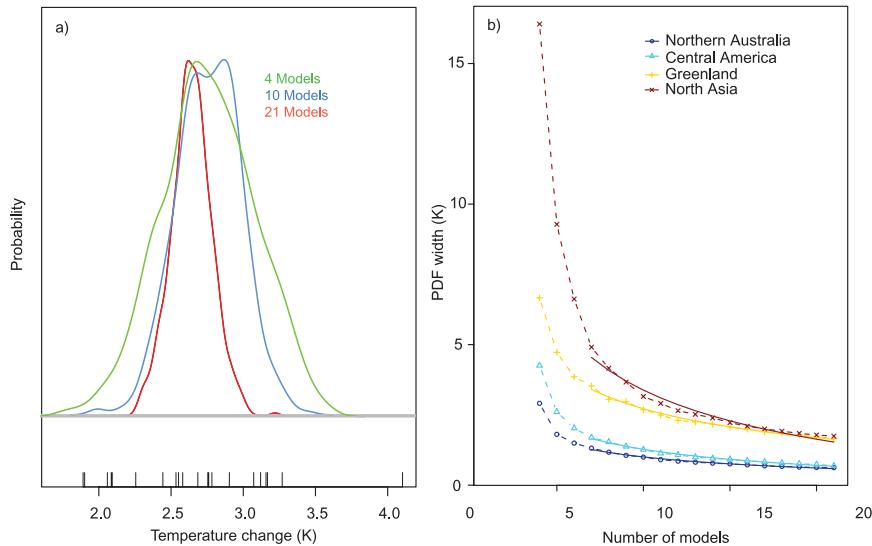


FIG. 5. (a) PDFs for annual global temperature change for the period 2080–99 relative to period 1980–99 from the Bayesian method by Furrer et al. (2007a,b), for the A1B scenario and for 4, 10, and 21 models. (b) Width of PDF (2.5%–97.5%) of temperature change in different regions (DJF, A1B, 2081–2100 vs 1981–2000) as a function of the number of models included, based on the method by Smith et al. (2009), the most recent version of the method originally proposed by Tebaldi et al. (2005). The analysis was repeated many times with different subsets of models and then the results were averaged. A fit (solid) of the form $1/N^{1/2}$ where N is the number of models is given for illustration for $N > 6$. Because the models are assumed to be independent, the uncertainty of the projections is reduced for a larger number of models in all cases shown.

number of models considered. An alternative view of the simulations' relation to the uncertain future climate is to consider each model trajectory as a possible future path for earth's climate and to accordingly represent the uncertainty in future projections by the posterior predictive distribution of a new GCM, whose width is of the same order of magnitude as the range of model projections. This proposal is made explicit in Tebaldi and Sansó (2009) and Annan and Hargreaves (2010).

In contrast, the probabilistic method by Watterson (2008) and studies based on detection and attribution fingerprint scaling (Allen et al. 2000; Stott and Kettleborough 2002; Stott et al. 2006) assume no improvement with additional models, which is equivalent to the assumption that each model is a plausible representation of true world, and one of them is right; however, we do not know which one (of course, strictly, no model is right because we know they are all incomplete, although the prediction of one could be right for a certain quantity). This issue of how much additional information is provided by more models is only now being explored and discussed. While it may be reasonable to assume some improvement in a projection when a few models are available as opposed to one (in particular, when predicting many quantities), a very large number of models should not infinitely improve our confidence, as long as

they are based on the same knowledge, make similar assumptions, or worse (but quite common) if they use parts of the code of existing models.

3. Model evaluation and weighting

Strictly, the calibration and evaluation of climate model predictions is impossible, as projections of climate change relate to a state never before observed. As we cannot evaluate centennial prediction, the evaluation of climate models is, therefore, on the observed present and past climate rather than the prediction. If the model matches the observed data, it only tells us that the data are consistent with the model. One of the difficulties is that the observations often have been used in the modeling process before, to derive parameterizations, or to tune earlier versions of models. Therefore, there is a risk of double-counting information, overconfidence, or circular logic if model evaluation and weighting is done on the same datasets that were used to develop the models. Thus, it is important that models are used to simulate past climates much different from today as part of the process to establish credibility of the model's responses.

If the model and data do not agree, it could be for a variety of reasons: it could depend on the particular metric being evaluated, how various quantities interact

in the model, and which parameter is compared to what observations. For any of these reasons, the interpretation could result in a judgment that the model is in error, the observational data are inadequate, or a combination of both. Agreement between the model and observed data, however, should be seen as a necessary but not sufficient condition (Oreskes et al. 1994). Note here that we should not expect perfect accuracy from models, but we can be satisfied with models that are adequate for a particular purpose. Weather forecast models, for example, do not contain a dynamic ocean component, yet they prove to be useful for the purpose of predicting weather for the next few days (and adding an ocean would not improve the forecast). An energy balance climate model does not even resolve the dynamics of the atmosphere, but it can easily replicate the global temperature evolution over the past century (Knutti et al. 2002; Meinshausen et al. 2008).

a. Why do we trust models?

Confidence in climate models comes from the fact that they are at least partially based on physical principles known to be true (e.g., conservation of mass, energy, and momentum) and that we understand the results in terms of physical processes (Bony et al. 2006) and can track them across hierarchies of models (Held 2005). Climate models reproduce many aspects of the current climate and its forced and unforced variability quite well (Räisänen 2007; Randall et al. 2007; Gleckler et al. 2008). Trends over the instrumental period resulting from anthropogenic forcings are well captured in most models (Barnett et al. 2005; Hegerl et al. 2007; Knutti 2008b). An important independent line of evaluation is provided by paleoclimate evidence, for example, from the last glacial or interglacial period. Since boundary conditions for paleoclimate are quite different from today's climate, a model's ability to simulate past climate is an illuminating test of the model's assumptions. Though uncertainty in proxy data is often large for the distant past, some credibility of the model as well as the actual climate system response to different forcings can be established (e.g., Liu et al. 2009). On the other hand, in some models the response to radiative forcing in a Last Glacial Maximum state, for example, has been shown to be quite different from the response in current climate (Crucifix 2006; Hargreaves et al. 2007). The nature of the forcings for paleoclimate and future projections is often also quite different.

Models continuously improve in simulating the present-day climate (Reichler and Kim 2008), and general aspects of projections from newer models usually agree with older ones. Model agreement is often interpreted as increasing the confidence in the newer model however, there is no obvious way to quantify whether agreement

across models and their ability to simulate the present or the past implies skill for predicting the future. A more in-depth discussion of these topics is given by Smith (2002), Tebaldi and Knutti (2007), and Knutti (2008a).

b. Model evaluation and tuning

In computationally cheap climate models, the calibration of parameters can be done by minimizing some cost function using search algorithms (e.g., Andronova and Schlesinger 2001; Forest et al. 2002; Knutti et al. 2002, 2003; Annan et al. 2005; Beltran et al. 2005; Frame et al. 2006; Hegerl et al. 2006; Meinshausen et al. 2008). Because of the complexity of AOGCMs and the associated computational cost, model tuning (defined as the adjustment of a model parameter within some known observational range) or calibration by automated procedures (e.g., finding optimal parameter values by minimizing some error metric) is usually unfeasible. Model calibration is mostly done in individual parts of the model and involves expert judgment. Formal metrics to quantify agreement with data are complemented with experience from other models to make choices. The number of intermediate versions of a coupled GCM that can be afforded is small, often only a few to a few tens before a final version is selected (CCSP 2008, their Table 4.2). In the few cases where large perturbed parameter ensembles were calculated, the standard model was found to be surprisingly close to the best-performing model (e.g., Sanderson et al. 2008, their Fig. 71), given the enormous degrees of freedom resulting from dozens of uncertain parameters. This suggests that expert judgment is very efficient in finding a good model (relative to the other models in the set) with a small number of trials. The model evaluation process is often not documented and is rarely based on clear procedures and statistical methods. Apart from the computational cost, one reason certainly is the fact that the metric to minimize is not clear. As discussed earlier, climate models serve multiple purposes, so it is not even clear what the best model (given some finite resources) would be. Tuning of model parameters in the sense of blindly minimizing errors without understanding the model's behavior or going outside known observational uncertainty is, therefore, not common in GCMs, and available observations are clearly relied upon for guidance in physically plausible tuning.

Statistical methods to evaluate and weight models are not routinely used in GCM development, but they have been used to a posteriori combine models or determine parameter ranges and distributions from both PPE (Murphy et al. 2004; Piani et al. 2005; Knutti et al. 2006; Murphy et al. 2007) and in Bayesian methods using MME (Giorgi and Mearns 2002, 2003; Tebaldi et al. 2004, 2005; Greene et al. 2006; Furrer et al. 2007b;

Tebaldi and Knutti 2007; Smith et al. 2009; Tebaldi and Sansó 2009). However, the field is still in its infancy, and no consensus exists on how models should be best evaluated. In the IPCC AR4 (Randall et al. 2007), the evaluation of models was mostly an expert assessment discussing what aspects of climate are well simulated, where models have improved, and what difficulties remain. The models were assessed as a group rather than as individuals, and future projections did not weight individual models or select subsets. No overall performance metrics or rankings were proposed. Results were either presented as multimodel equal-weighted averages or as a collection of individual models to show the model spread, but without any quantitative information of how the model spread should be interpreted or as to which models may be more credible.

The main issue with model performance is that there is virtually an infinite number of metrics that can be defined, and a large number of them may be defensible for certain purposes. Whether a model is “good” or “bad” depends on the question at hand. Models have been evaluated on many different quantities but mostly on the present-day mean climate and variability; however, for the upcoming CMIP5-coordinated model experiments, paleoclimate simulations will be used for the first time as a standard part of the evaluation process (Taylor et al. 2009). Prior to this, the present-day climate was used as a standard reference, at least partly because most of the observations are about the present-day mean state and variability and because we believe that we understand many aspects of the present climate rather well. It may also be a remnant from earlier times when multicentury transient simulations were not yet possible. The question of whether the simulation of the present-day climate matters for future projections is difficult to evaluate. For example, significant efforts go into improving tropical variability in models’ simulations of El Niño and significant improvements have been made, yet models do not agree on the sign of future El Niño change. This is at least partly due to the large multidecadal and centennial time-scale variability of ENSO (seen in observations and in multicentury control runs from climate models), and sampling issues related to this nonstationary base state cause difficulties in evaluating what the future behavior of El Niño may be. For many large-scale changes—for example, temperature projections, which still are uncertain by about a factor of 2 even on the global scale—tropical variability related to ENSO is probably of minor importance, as the effect of ENSO on global temperature is only on the order of 0.1°C. On the other hand, ENSO will have a strong effect on Australian water availability. Therefore, using ENSO as an example where inherent low-frequency variability may make it difficult to ever

provide an accurate projection of future El Niño behavior, model development and evaluation is often done based on processes of interest rather than on an analysis of what quantity would be most important to be well represented in the model to make an accurate prediction. Multimodel ensembles are of value here because they allow for a determination as to why models agree or disagree, thus shedding light on where efforts are best spent to improve a prediction. Indeed, the CMIP3 archive has sparked many attempts to isolate why models differ, for example, by quantifying agreement in different feedbacks (e.g., Bony et al. 2006; Soden and Held 2006).

Good agreement with observations in one metric does not guarantee good performance in other variables, but correlations of performance across variables at least within one component of the climate system are quite high because many variables are influenced by the same processes and parameterizations. Models that represent some basic variables, such as temperature and precipitation, well often also perform well in other variables (e.g., Gleckler et al. 2008).

c. Model weighting

Models can be combined by experts defining certain (sometimes ad hoc) selection criteria to pick subsets of more skillful models. In the absence of formal methods to weight models other than including or excluding them, this may be a useful approach. To give a few examples, van Oldenborgh et al. (2005) quantified the effect of climate change on ENSO using a subset of the CMIP3 models, and several studies predicted changes in Australian rainfall and runoff based on subsets and rankings of models (Perkins et al. 2007; Maxino et al. 2008; Pitman and Perkins 2008). Waugh and Eyring (2008) and Eyring et al. (2007) assessed the performance of stratospheric chemistry–climate models but found only small differences between weighted and unweighted projections. Schmittner et al. (2005) produced projections of future changes in the Atlantic meridional overturning circulation and also found unweighted averages to be similar to the weighted ones, but they report a decrease in the model spread after weighting. Santer et al. (2009) found that detection and attribution of water vapor changes are insensitive to model quality. Temperature and precipitation changes were calculated based on CMIP3 by considering all models (Meehl et al. 2007a), or based on weighting with current climatology (Giorgi and Mearns 2002, 2003; Tebaldi et al. 2004, 2005). Greene et al. (2006) additionally used observed trends for weighting.

In the area of weather and seasonal prediction, the ensemble approach is well established (e.g., Fraedrich

and Leslie 1987; Doblas-Reyes et al. 2003; Hagedorn et al. 2005). Despite the success of combining models in other areas, such attempts are still rare in the climate community, and many people are reluctant to deviate from the interpretation of the family of coexisting models (Parker 2006). Some scientists argue that we cannot attach weights, produce meaningful PDFs, or even define the space of plausible models (Stainforth et al. 2007), because all models have essentially zero weight. This may be strictly true, but from a pragmatic point of view, model selection is already routinely done. Newer models are developed and older ones are phased out in newer intercomparisons and IPCC reports (Meehl et al. 2007a), thus giving them zero weight. Clearly, there are many issues with PDFs derived through statistical analysis of MMEs, but the problem may lie more in how to communicate and interpret them than in whether or not they should be constructed in the first place. PDFs are, of course, always conditional on the model, statistical assumptions, and observational constraints (although that generally is not informative for the decision maker without further discussion of the model's trustworthiness).

One way to test whether some observed quantity is important for a prediction is to consider the correlation between the observed quantity and the prediction across a set of models. If the correlation is weak, then the observation likely has little effect on the prediction and weighting or creating a subset based on that quantity will not impose any constraint on the prediction. It may, however, introduce spurious biases in the projection if the sample size is small (selecting a subset of 5 or so models out of 20 will have the tendency to reduce the model range and variance even if the subset is chosen randomly). If the correlation between an observation and a prediction is strong, then that means that the observation may be a good predictor for the quantity of interest, and a constraint on the observation will constrain the future. The assumption, of course, is that the correlation across several models represents the influence of a process that affects both the observation and the prediction and not just the simplicity of the underlying model—that is, that all models are based on the same parameterizations. In many cases (e.g., where observed greenhouse attributable warming is related to future warming) this assumption is justified; however, particularly in PPEs where all models share the same structural core and many constraints are applied without understanding the processes behind it, the correlation may, indeed, be unphysical. There is also the assumption that an observation does not manifest itself differently in different models because of nonlinear interactions, or that several observations can have different realizations in different models due to such interactions.

The correlations between predictions and observation features of the current climate mean state (which is predominantly used for model evaluation) are predominantly weak if existent at all. Figures 6a and 6b show the correlation of the CMIP3 seasonal temperature biases as compared to ERA-40 near-surface temperature for the period 1980–99 (aggregated as root-mean-square over space) and future global seasonal warming for the period 2080–99 in the A1B scenario. Correlations are vanishingly small, and even if both winter and summer are considered and related to simple quantities such as the transient climate response and climate sensitivity (Figs. 6c and 6d) to exclude the effect of different forcings, the correlations do not improve. Thus, the climate response does not seem to depend in an obvious way on the pattern of twentieth-century temperature, at least in the range of models considered.

This is consistent with the fact that newer climate models reproduce the current climate significantly more accurately than older ones (Randall et al. 2007; Reichler and Kim 2008), yet the spread of projections on both global and local scales is not decreasing very much (Knutti et al. 2008). For example, the range of climate sensitivity has only decreased from 1.5°–4.5° to 2.0°–4.5°C over the last two decades. In the most recent CMIP3 intercomparisons, the standard deviation of all model climate sensitivities was 0.69, which is reduced but not significantly so from the earlier CMIP1 and CMIP2 intercomparisons (0.78 and 0.92, respectively; one standard deviation), after several years of model development, more observational data for model evaluation, and an increase in computational cost of probably at least two orders of magnitude. These results are also consistent with recent studies that found only a weak statistical relation between observations of the present-day climate and climate sensitivity (Murphy et al. 2004; Piani et al. 2005; Knutti et al. 2006; Sanderson et al. 2008). It is also in agreement with the results of Jun et al. (2008b), who noted that there was very little correlation between the ability of the climate models to simulate the observed patterns of the mean temperature and the observed patterns of the temperature trend.

Rather than relating global performance in simulating surface temperature to the prediction of future warming, one may argue that model performance on local-to-regional scales should be considered and used for regional projections. That is, if there is a high correlation between a good performing model in a certain location and a preferred value of future climate change, then perhaps that agreement would provide information on what the future climate change may be. But there is still no guarantee that good present performance is a predictor of future climate change, since future climate change is

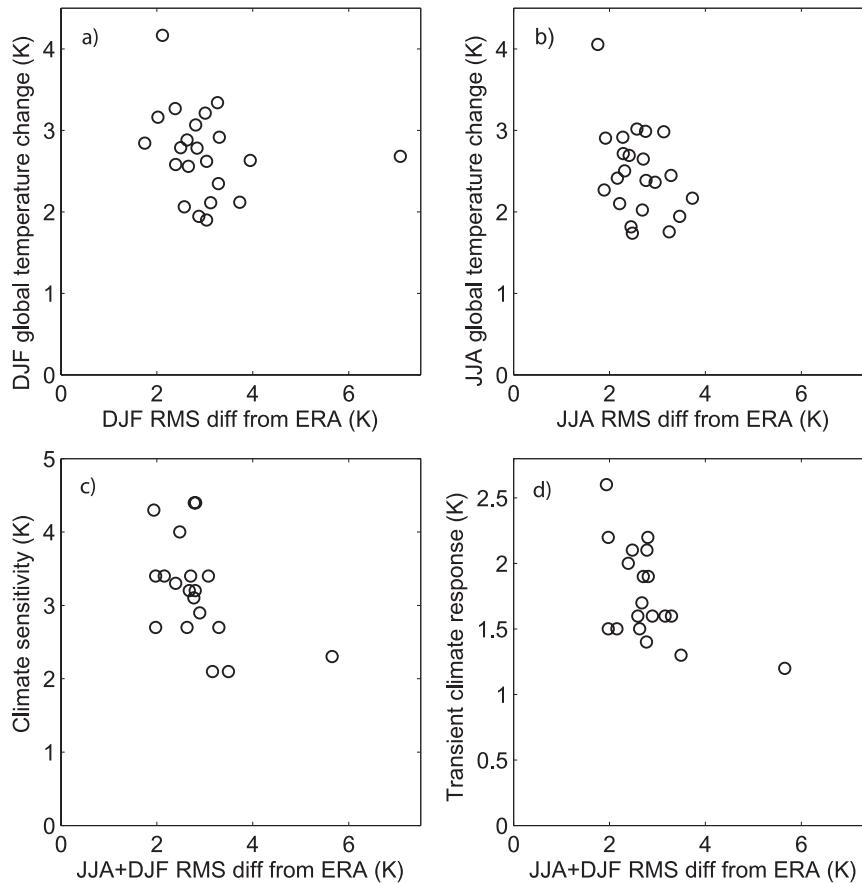


FIG. 6. Scatterplots of rms error from ERA-40 surface temperature for different seasons (DJF, JJA) vs predicted (a) DJF warming, (b) JJA warming difference of 2080–99 from 1980–99 in the A1B scenario, (c) climate sensitivity equilibrium global surface warming for $2 \times \text{CO}_2$, and (d) transient climate response global surface warming at the time of CO_2 doubling in a $1\% \text{ yr}^{-1}$ CO_2 increase scenario. Each circle marks one model. Correlations are near zero in all cases, indicating that the climatology of surface temperature is weakly related to the predicted warming.

unknown. Figure 7 shows the correlation between the performance in simulating current surface temperature and predicted warming by the end of the century in the A1B scenario at each grid point. While there is correlation between the present surface temperature and future warming in some locations, the correlation is weak. On land its absolute magnitude exceeds 0.4 only in a few locations, indicating that the current temperature explains less than 20% of the model spread in most locations. But because the number of models is small, many correlations occur by chance, and the distribution of correlations for all grid points does not strongly differ from a null hypothesis where all points were purely random (Figs. 7b and 7d). Most correlations should, therefore, be seen as essentially random. On the basis of a similar analysis but using regional patterns and multiple variables, Whetton et al. (2007) concluded that applying

weights based on present-day climate was useful, but correlations [in their case, based on the regions defined by Giorgi and Francisco (2001)] also rarely exceeded 0.4 and therefore provide a very weak constraint. Cases have been reported where correlations between observations and predictions are strong—for example, between the seasonal cycle and climate sensitivity (Knutti et al. 2006), the albedo feedback on seasonal and decadal time scales (Hall and Qu 2006), past and future sea ice reduction (Boe et al. 2009), or for the amplification of tropical surface temperature variability on short and long time scales (Santer et al. 2005). In some cases weighting or selecting a subset of models leads to smaller model spread for predictions—for example, for Australian rainfall (Perkins et al. 2009; Smith and Chandler 2010)—but in most cases these are carefully selected quantities based on process understanding rather

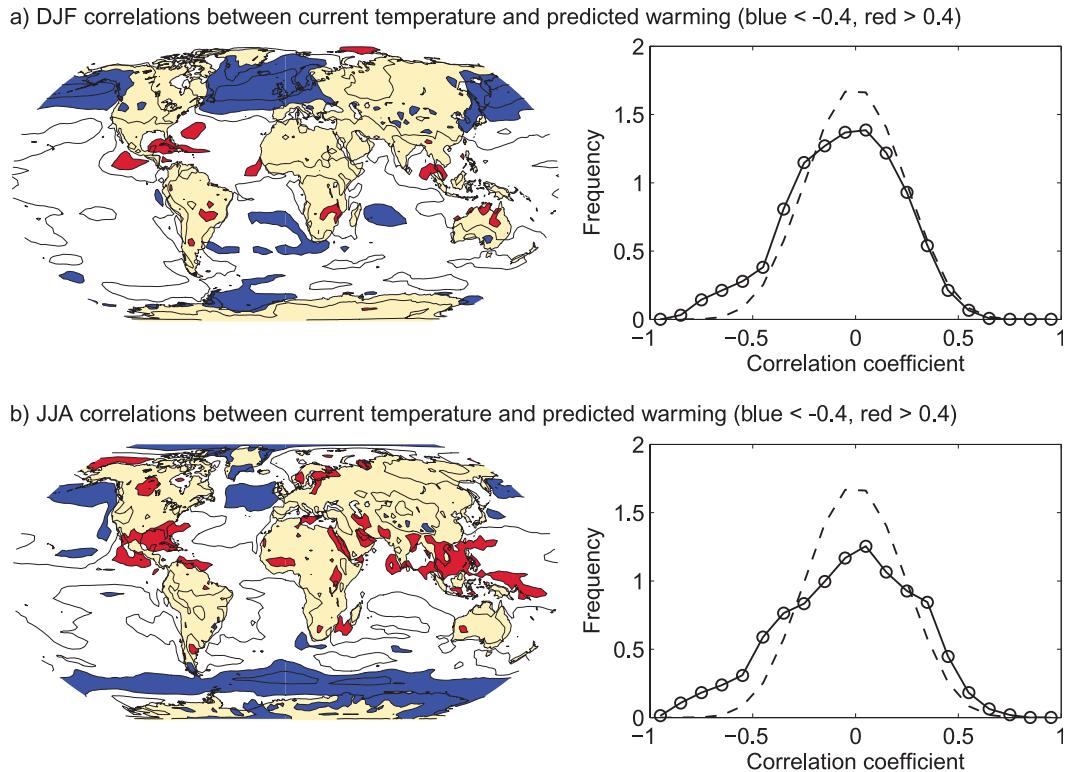


FIG. 7. Correlations between mean temperature for the period 1980–99 and predicted warming for the period 2080–99 in the A1B scenario at each grid point, for (a) DJF and (b) JJA. Contour intervals are 0.2, correlations smaller than -0.4 and larger than $+0.4$ are shown in blue and red, respectively. (right) The distribution of correlation values for the CMIP3 models (circles) and the distribution that would result from normally distributed random numbers. Most correlations are insignificant and are expected to appear by chance because the number of models is small.

than on a broad aggregation of model biases across space, time, and variables.

However, one may also argue that evaluating models only on a single variable is an approach that takes a too narrow focus. Reichler and Kim (2008) evaluated the CMIP3 models on a large set of variables of the present-day mean climate state and combined all errors into a single number. Here we use an updated version of their results based on four seasonal mean results from 37 different observed climate quantities (T. Reichler 2008, personal communication) but only use the ranking of the models rather than the overall performance index to generate different subsets. The hypothesis is that the spread of projections from a subset of N good models should be smaller than the spread of all models. Figure 8 shows the ratio R of the standard deviation of N good models to the standard deviation of all models for the precipitation trend (percent change in local precipitation per Kelvin change in global temperature calculated for each grid point and each model for the period 1900–2100). The ratio R is determined at every

grid point, and the results are shown as box plots, for the two best models (leftmost), three best models (second from left), and so on. Ratios R of less than unity indicate that the spread of the N best models is smaller than the spread of all models. Note that the precipitation response is spatially heterogeneous, so one would not expect the box plot widths to decrease with a subset of better models, but one would expect R to be lower *on average* if the subset of models was in closer agreement than the set of all models. The median (box center) and mean (red solid line) of R at all locations is, indeed, smaller than unity, but a close inspection shows that most of that effect is an artifact of the standard deviation being a biased estimator of the spread for small samples. For normally distributed numbers and sample size $N = 2$, the standard deviation underestimates the real spread by roughly 20%, for $N = 5$ by 6%, and for $N = 10$ by 3% (red dashed line). The maps show R for a subset of the 11 best versus all 22 models and reveal a picture without any obvious structure. In fact, the same figures for a random subset of models are almost undistinguishable.

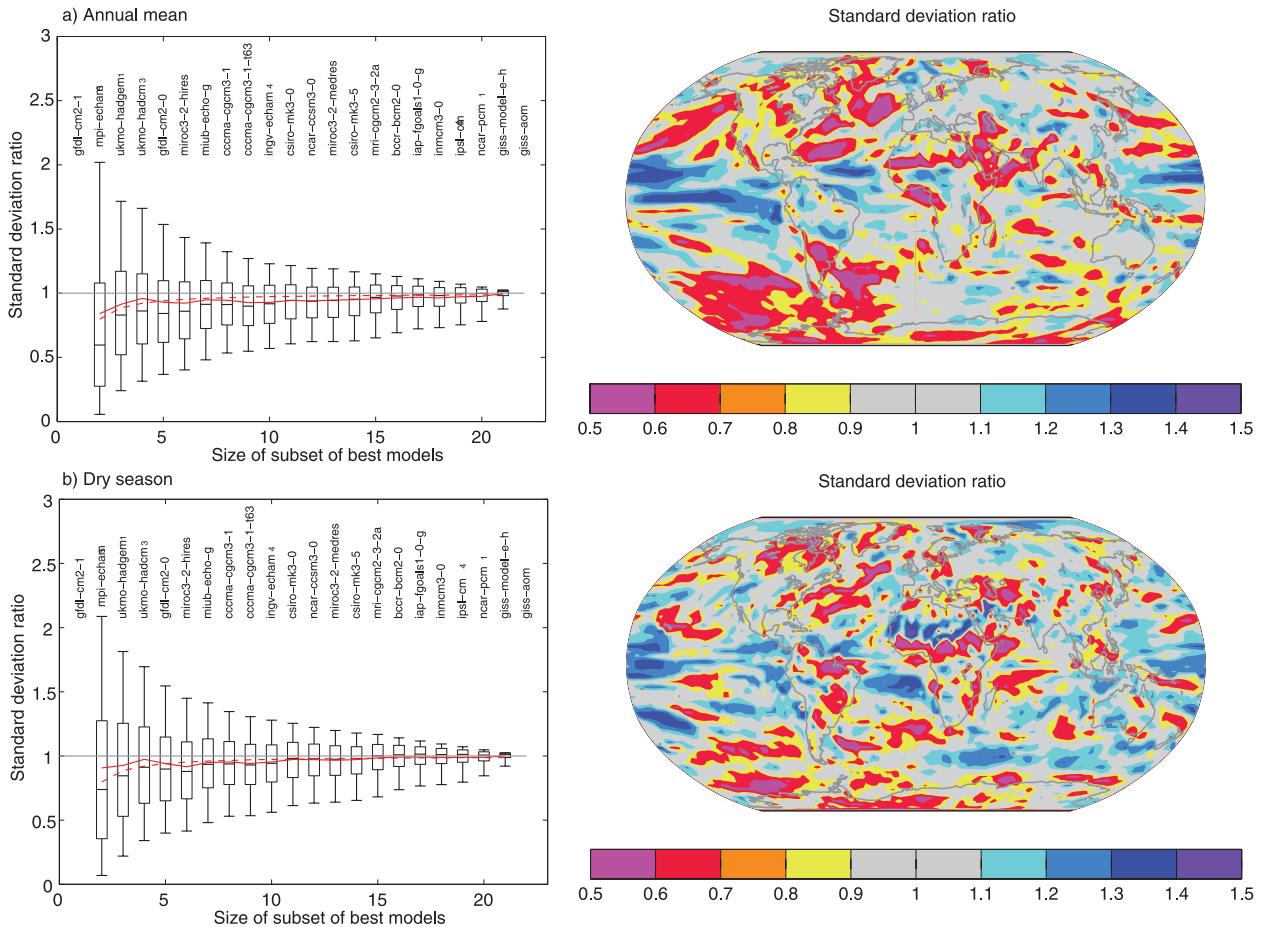


FIG. 8. Ratio R of the std dev of a subset of N good models (for present-day climatology) to the (a) std dev of all models, for different sizes of the subset and annual mean precipitation trend (percent per kelvin global temperature change over the period 1900–2100) and (b) the dry season (three driest consecutive months at each grid point). For example, for $N = 3$, R is the std dev of GFDL Climate Model version 2-1 (CM2-1), Max Planck Institute (MPI)-ECHAM5 and Met Office (UKMO)-Hadley Centre Global Environmental Model version 1 (HadGEM1), divided by the std dev of all models. Here R is calculated at every grid point and summarized in the box plot. Ratios R less than unity indicate that the subset of good models has a smaller spread in the predicted rainfall trend. (left) The box plots of R for all grid points and different subsets of models. The box marks the median and interquartile range, the line marks the 5%–95% range. The model names of the ranking used (updated from Reichler and Kim 2008) are given in the figure. The red solid line indicates the mean of R over all grid points. The red dashed line is what would result from normal random numbers, because the std dev is negatively biased for small samples. (right) The ratio R for the 11 best models vs all models. While the spread for a subset of good models decreases in some places, the effect is small, and there is little benefit of selecting a subset.

The somewhat surprising conclusion from this analysis is that if one would perform a ranking of all models based on a comprehensive set of global present-day diagnostics and select a subset of models that agree well with observations, then the tendency for a better constrained projection (on average in all locations) would be very small in most cases.

Probabilistic projections based on fingerprint scaling (Allen et al. 2000; Stott and Kettleborough 2002; Collins et al. 2006; Harris et al. 2006; Stott et al. 2006) also amount to reweighting, albeit of a single model’s projections; however, in this case the relation between the past and present is clearly quantified and understood in

terms of the overall feedback strength. Models with a stronger greenhouse warming over the past decades show a higher warming in the future. Similar arguments hold for probabilistic studies of global temperature constrained by the observed surface warming, ocean heat uptake, and radiative forcing (e.g., Forest et al. 2002; Knutti et al. 2002), where the performance of a model often relates clearly to the future prediction, at least for the next few decades.

In summary, this section highlights the difficulty with weighting models based on observations. Correlations between observed quantities and predictions are small in many cases, resulting in little, if any, change in a weighted

average and small reductions in the model spread. This does not imply that a weighting of models is impossible in principle, but it indicates that the choice of a meaningful metric is far from trivial. A few recent studies reported a reduction in model spread after evaluating the models on multiple criteria, but whether the prediction is in fact more accurate remains to be seen (and it will take a long time to find out). In most studies the weighted averages and model spread are similar to those of the unweighted ensemble, a result explained by the absence of correlation between the observations used to weight the models and the models' future projections.

4. Model combination and loss of signal

A last issue that deserves attention is the fact that an average of multiple models may show characteristics that do not resemble those of any single model, and some characteristics may be physically implausible. If two variables— x and y —are related in a nonlinear way, then the average of x and the average of y from several models will not follow the original relation between x and y . Therefore, a model average state may not even be physically plausible. In cases where there is a bifurcation between multiple solutions, an average state may not exist. While these issues may not be serious for most large-scale climate projections as long as the perturbations are not large, there is the issue of loss of signal, which is serious and has not been addressed so far. One such case is the predicted change in precipitation, resulting from anthropogenic warming. While models agree on the large-scale drying in the subtropics and the wetting of the high latitudes, the locations of the maximum changes are often a bit shifted. In some areas the models also predict opposite signs in the trends. Figures 9a and 9c show multimodel precipitation changes displayed as maps, similar to those presented in the IPCC AR4 (Alley et al. 2007, their Fig. SPM 7), except that trends (as before) in percent change in local precipitation per kelvin change in global temperature are calculated for each grid point and each model for the period 1900–2100 rather than showing the difference between the end of the twenty-first century and the present as in IPCC. The trends are used to maximize the signal using a 200-yr period and to reduce the effect of different global temperature change in the different models. If anything, the models should agree better among each other, but the maps are very similar to those in the IPCC AR4 (Alley et al. 2007) and the conclusions will not depend on these choices. Changes are shown for the annual mean rainfall (Figs. 9a and 9b) and for the dry season (Figs. 9c and 9d), that is, for the driest three consecutive months in the present at each grid point. Further details

are given by Solomon et al. (2009). A histogram of the land area (restricted to 60°S–60°N, as this is essentially the area relevant for agriculture) that is undergoing a certain change in precipitation shows that almost every model (light blue lines) shows drying of more than 15% K^{-1} for the annual mean and more than 20% K^{-1} in the dry season in some places, but the multimodel average (black line) does not. The distribution of precipitation is much narrower for the model average because changes are of opposite sign or maxima are not collocated in the individual models. If the distributions of the individual models are averaged (dark blue line), then the individual distribution is about 50% wider than the multimodel distribution (black line). If we interpret the CMIP3 models as a collection of predictions of which one may be the truth (of course, none is exactly) but we do not know which one, then the average precipitation change expected is 50% larger than the multimodel mean suggests. Large drying may well occur in some locations even if the multimodel average has lost that signal. This is particularly disturbing because plants have thresholds beyond which they can no longer survive, and the difference in effects between using the individual models or the multimodel may thus be very large. The presentation of a multimodel mean map for precipitation without any further discussion of this problem may, therefore, be misleading, especially if used to inform adaptation decisions. The idea of robust decision making (Lempert and Schlesinger 2000; Dessai et al. 2009) requires sampling of a broad range of outcomes, and precipitation is a good example where such concepts are likely to be more useful than model averages.

5. Conclusions

In this study we have shown that extracting policy-relevant information and quantifying uncertainties from ensembles of opportunity of climate models is difficult. The prior distribution of the models is important but unclear, except that it is likely too narrow and not capturing the full range of plausible models. An average of models compares better to observations than a single model, but the correlation between biases among CMIP3 GCMs makes the averaging less effective at canceling errors than one would assume. For present-day surface temperature, for example, a large fraction of the biases would remain even for an infinite number of models of the same quality. Extreme biases tend to disappear less quickly than smaller biases. Thus, models are dependent and share biases, and the assumption of independence made in some studies is likely to lead to overconfidence, if the uncertainty is measured by the standard error of the ensemble means (inversely proportional to the square

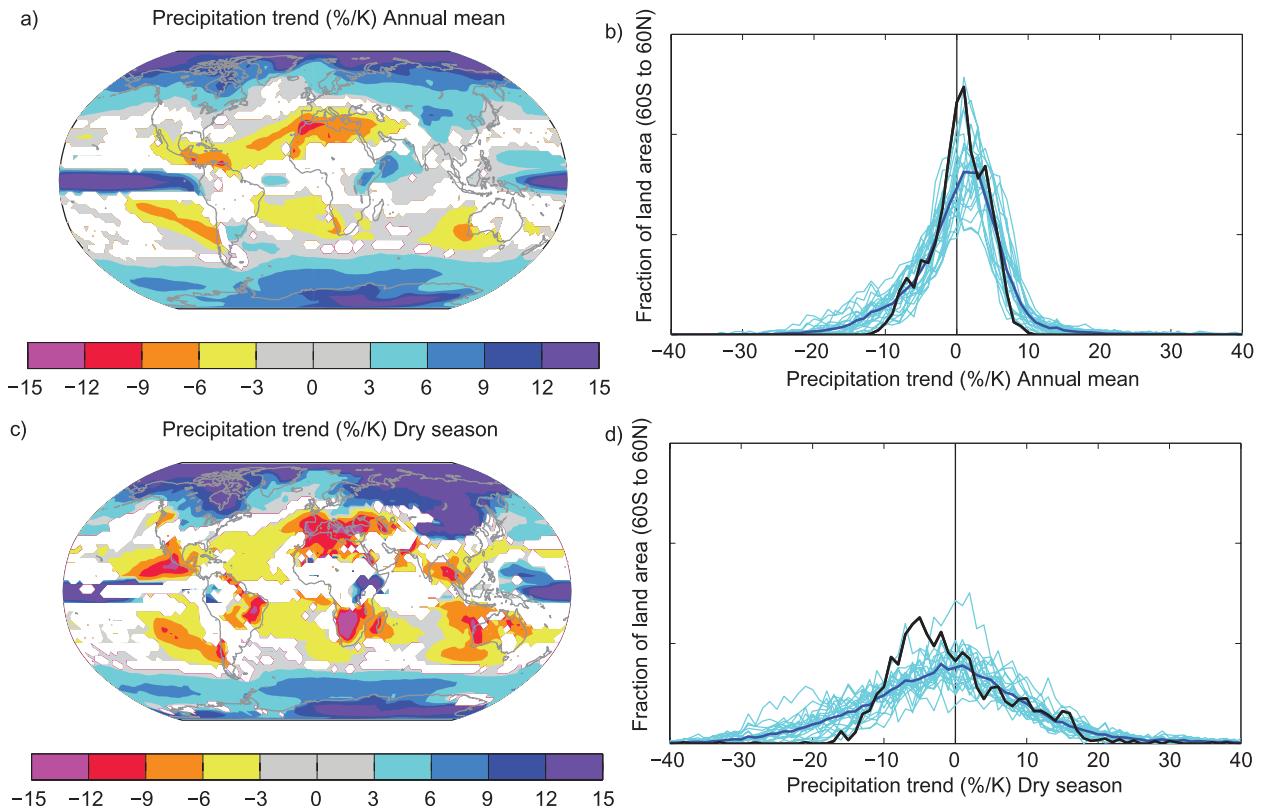


FIG. 9. Precipitation trend (in percent per kelvin global temperature change over the period 1900–2100, relative to the base period 1900–50) for (a) the annual mean and (c) the dry season (three driest consecutive months at each grid point). White is used where fewer than 16 of 22 models agree on the sign of the change (see Solomon et al. 2009 for details). (b),(d) Distribution of the fraction of land area between 60°S and 60°N that shows a certain drying or wetting. Light blue lines indicate each CMIP3 model; the average of the light blue lines is given in dark blue. The black distribution is the result for the multimodel mean shown in the left panels. The expected precipitation change in the multimodel mean is about 30% smaller than in any single model.

root of the ensemble size). Quantitative methods to combine models and to estimate uncertainty are still in their infancy. Some studies have proposed ad hoc methods for weighting or selecting subsets of models but few have demonstrated any improvement in the projections' skill or that the evaluation criterion is even relevant to the forecast. International assessments by IPCC (Randall et al. 2007) or the U.S. Climate Change Science Program (CCSP 2008) evaluate models but provide little information of how model error/bias translates into bias in future projections. They show what models can and cannot simulate in the present, but a discussion whether this should make us confident or not for the predictions made is often missing. The issue of combining models will become more important with the availability of more computing power and more models. Future ensembles may be more heterogeneous as some models include more components (e.g., chemistry, ice sheets, dynamic vegetation, upper atmosphere, carbon cycle, land use), and some groups are starting to produce perturbed physics ensembles with their model.

One would hope that a model that can reproduce many observed features is a better model than one that is unable to do so. However, defining performance metrics that demonstrably relate to prediction skill remains a largely unresolved problem. It is shown here that most straightforward metrics (e.g., root-mean-square errors from climatology) do not correlate with future projections on a large scale. Local biases also correlate weakly with local projections. Selecting subsets of models based on an overall evaluation of how they simulate present-day climatology is shown to have a small effect on the spread of projections. While there may be benefits in selecting subsets of models in certain areas after careful process-based assessments, a general recipe or an overall model ranking for all purposes seems unlikely to exist.

Understanding what makes the projections of two models agree or disagree, evaluating models on key processes, developing metrics that demonstrably relate to projections, and searching for emerging constraints in the system on the basis of observations (Knutti et al. 2002; Stott and Kettleborough 2002; Hall and Qu 2006;

Knutti et al. 2006) may be ways forward. Large perturbed physics ensembles with multiple models may help find constraints valid across structurally different models. Seamless prediction—that is, the initialization with observations and evaluation on weather and seasonal time scales using climate models—could help provide constraints on feedbacks that operate on both short and long time scales. Paleoclimate provides another opportunity to evaluate models, although observational uncertainties tend to be large in the distant past. New methodologies—such as stochastic-dynamic parameterization [Palmer et al. 2009, unpublished manuscript (available online at <http://arxiv.org/ftp/arxiv/papers/0812/0812.1074.pdf>)], where stochastic parameterization schemes are devised to represent model uncertainty to produce the benefits of a multimodel ensemble in a single model—could eventually provide an alternative to the current multimodel ensemble methodology.

The community would benefit from new methods to intelligently combine perturbed physics and multimodel ensembles, as well as statistical methods that can incorporate structural model uncertainty. Thus, taking advantage of the characteristic that the multimodel ensemble average outperforms any individual model, methodologies could be developed to better assess uncertainty by using this information combined with characteristics of the range of model realizations. Such methods, however, require a set of models that have a reasonable spread with which to begin. In fact, it is likely that more could be learned from a model that is on the outer edge of the range than from another model near the center of the range. Model diversity is important for these kinds of multimodel exercises, with the range of model realizations providing information that informs the plausible spread of model realizations. It is also more useful if the data used for development is not the same as that for evaluation and weighting.

Given the demonstrated difficulties in defining model performance and the lack of consensus on selecting and weighting models, methods to combine models should be assessed carefully and compared to multimodel ensemble averages and information derived from model spread. The overconfidence achieved by improper weighting may well be more damaging than the loss of information by equal weighting or no aggregation at all. As long as there is no consensus on how to properly produce probabilistic projections, the published methods should be used to explore the consequences arising from different specifications of uncertainty.

The lack of consensus on combining models also underscores the need for decisions that are robust against alternative future climate outcomes (Lempert and Schlesinger 2000; Dessai et al. 2009). In certain cases,

the simple specification of a few illustrative models as alternative plausible outcomes without probabilities (similar to the illustrative SRES scenarios) may also be a useful and transparent choice to test the sensitivity of adaptation and policy decisions to the uncertainty in future climate change. However, there is some danger of not sampling the extreme ends of the plausible range with a few cases—for example, very high climate sensitivities that are not present in CMIP3 (Knutti and Hegerl 2008)—and the danger that the illustrative models will be interpreted as equally likely, even if no probabilities are specified. In any case we feel that as the amount of data from climate models grows and as the dependency structure across the ensemble gets more complex when perturbed parameter versions of some models become available, metrics to evaluate models and quantitative methods to extract the relevant information and to synthesize it are urgently needed.

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