

## COMMENT

**Comment on Soon et al. (2001) 'Modeling climatic effects of anthropogenic carbon dioxide emissions: unknowns and uncertainties'****James Risbey\***

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In their contribution 'Modeling climatic effects of anthropogenic carbon dioxide emissions: unknowns and uncertainties' Soon et al. (2001) hang out some of the 'dirty laundry' of General Circulation Models (GCMs). There is nothing wrong with doing that in any healthy research community. A dirty laundry list provides some indications of what's wrong with models and may even indicate something about the general state of the field. The fact that one can generate such a list is not meaningful per se, however, since one can generate such lists in any complex modelling endeavour, healthy or otherwise. Nor does such a list necessarily tell you anything meaningful about what GCMs can and cannot do. Like any model, GCMs have different utilities for different functions/purposes. The assessment of utility makes sense only in relation to particular functions. In each case the function must be specified and the processes that are integral to simulation of that function must be identified. For each relevant process, one would then want to diagnose it with appropriate variables on the spatial and temporal scales intrinsic to the process, e.g. Risbey & Stone (1996). This is the approach foreshadowed by Gates et al. (1990) and Gates (1992) in setting up a hierarchy of validation approaches for GCMs. By contrast, Soon et al. (2001) make a set of general claims on the basis of the laundry list, mostly without relating function to process, variable or scale. Where GCM functions are identified, they are often so general as to defy meaningful assessment. Some examples from the paper serve to illustrate these points.

Soon et al. (2001) cite Oreskes et al. (1994) on the point that it is impossible to validate climate models because natural systems are never closed and model results are always non-unique. From this they claim that 'it follows from Oreskes et al. that the intrinsic value of a climate model is not predictive but heuristic',

and that it is improper to use climate models 'to predict unconstrained scenarios of change by adding CO<sub>2</sub> to the atmosphere'. Just because you cannot 'validate' a model in this way, it does not follow that it cannot be used for making useful predictions. Weather GCMs are subject to the same limitations in this regard as climate models, yet the world's weather services use weather GCMs routinely to assist in making predictions. By and large weather GCMs do a good job for short-term (i.e. a few day) forecasts and clearly do have some predictive skill. In particular, weather models have more skill for some purposes (e.g. 500 hPa height fields on Day 1) than others (e.g. precipitation in a specific region on Day 7). To infer that GCMs have no prima facie predictive skill for CO<sub>2</sub> simulations does not follow from the points made by Oreskes et al. (1994).

In several places Soon et al. (2001) cite flux errors in the energy budget in GCM simulations compared to observations, generally taking the presence of such errors to imply that GCM simulations are unreliable. The presence of such errors may or may not be important depending on what one wishes to simulate and what the critical thresholds for such errors are. In some cases the flux errors may be irrelevant for the process of interest or may simply be subcritical. Again, one must identify what one is trying to simulate and describe the processes and variables principally governing its response. If one can then argue that the key processes are sensitive to the flux errors, then the errors are demonstrably important—see for example the analysis of Marotzke & Stone (1995) on the importance of flux errors in simulation of the thermohaline circulation. Without such analysis a list of flux errors in models is just that.

Where GCM simulations of specific variables are examined in Soon et al. (2001), the models are sometimes impugned on the basis of specious reasoning. In their Fig. 3 they show ensemble mean time series

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results from 30 GCMs of the Atmospheric Model Intercomparison Project (AMIP) forced with observed sea surface temperatures (SSTs) over the period 1979 to 1989. In the figure the model ensemble mean time series of precipitation is fairly flat and does not mirror the large interannual variability of observations shown. Soon et al. (2001) claim on this basis that 'the modeled interannual variabilities of the hydrological cycle are seriously underestimated by a factor of 3 to 4'. This does not follow from the ensemble mean results shown. Precipitation is spatially and temporally heterogeneous and does not follow straightforwardly from SST specification. Any average of precipitation across models in a time series plot will smooth out much of the variability that occurs within individual models. The flat ensemble mean time series presumably reflects this. To obtain estimates of the interannual variability of precipitation in models compared to observations one must examine individual model runs, not ensemble means. The same point can be made in an alternative way by considering an ensemble mean of observations (e.g. taken from different decades overlaid), which would also display greatly reduced variability compared to that assessed for a single realization.

Soon et al. (2001) cite shortcomings in GCM simulations of stratospheric temperatures and note the importance of stratospheric dynamics for simulating interdecadal troposphere-stratosphere variability in winter. They then claim that 'in order to address properly the climatic response of added CO<sub>2</sub> (or for that matter any number of external forcings under consideration), a GCM that resolves the stratosphere appears to be another necessity.' This is a non sequitur. The studies cited indicate that resolving stratospheric dynamics is key for the purpose identified. However, it is another step again to claim that you cannot simulate anything meaningful in regard to CO<sub>2</sub> effects without a fully resolved stratosphere. For that, one would need to show that simulating the correct winter interdecadal variability was critical to simulation of CO<sub>2</sub> effects on all other spatial and temporal scales.

In a similar vein, the Soon et al. (2001) review cites shortcomings of GCM simulations of the El Niño Southern Oscillation (ENSO). By and large, GCM

modellers have been quite forthcoming and candid about these shortcomings (Meehl & Washington 1996, McAvaney et al. 2001). It would be fair to cast doubt on GCM utility for seasonal to interannual climate predictions in the Pacific basin. But just because ENSO and other phenomena appear on a GCM dirty laundry list does not mean that there is no utility in simulating longer time scale responses to enhanced CO<sub>2</sub>. To go on to claim that 'Our review of the literature has shown that GCMs are not sufficiently robust to provide an understanding of the potential effects of CO<sub>2</sub> on climate necessary for public discussion' is a sweeping extrapolation from isolated and unrelated points and may well amount to throwing the baby (GCM) out with the bath water (dirty laundry).

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