



Commentary on the debate between James Hansen and Patrick Michaels, November 1998

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1. *Introduction*

I am a biologist turned social scientist conducting research on climate change from a science studies perspective and more recently with respect to climate change impacts assessment. In the early to mid-1990s I spent several months working at the Hadley Centre of the UK Meteorological Office, as well as visiting a number of other major climate modelling groups in the USA and Germany. In this brief commentary, I will discuss the contrast between Michaels' more empirical style and Hansen's more theory-driven approach to climate change science. I will argue that Michaels' appeal to a comparison of 'data' and 'models' is misleading because of the inadequacy of the datasets he attempts to use and the interpretation he provides. Without a critique of the construction of climate models *per se* his case is much weakened.

2. *The critical role of climate models*

What struck me most when reading the transcript was the very different notions contained within of what *is* proper climate change science. Patrick Michaels (PM) seemed to adopt a strongly empirically-oriented view of climate science, in which hypotheses are tested against good data sets. Yet PM's approach downplays the vital role of climate models, without which the question under debate cannot really be tackled. (After all, observations of the past and their correspondence or not with climate models' simulations of the past, tell us little *per se* about the future.) From PM's perspective, it is appropriate to put a great deal of onus on the match between observations of temperature and other climate variables and that predicted from the simulations of climate models. If there is a large discrepancy, the case for past and future anthropogenic climate change as suggested by climate models is significantly undermined according to this viewpoint. PM does not appear to ascribe any capacity to climate models for simulating the physical (and chemical and biological) processes

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which are responsible for generating climate change. If one ascribes some predictive capacity to climate models because of the physical and empirical understanding and knowledge which lies behind them, then PM's criticisms are less convincing. They are less convincing because PM does not provide us with an alternative method or approach for thinking about future climate change due to human-induced emissions of greenhouse gases. The critique is also less convincing because the observations upon which PM draws his case are not good enough to bear the weight of the argument he wishes to make.

As James Hansen (JH) notes at several points, it is no good comparing climate model simulations against short observational records (10–20 years). Longer time-series of observations would be required for PM's case against the models to be convincing on this basis. It is interesting that JH does not specify what sort of observational records (how long, with what coverage, including which climate variables, etc) would be needed before the case for or against the ability of climate model's to simulate past climate would become more convincing. Perhaps the modellers do need to be more up front about what empirical and observational statements would be required for that community to really question their models and the plethora of theories, approximations, data and so forth which goes into making a climate model.

I am inclined to trust JH's position more than PM's because JH is dealing with both climate model simulations *and* data as a joint and iterative system by which more robust knowledge is produced. PM, on the other hand, is appealing to common sense notions of whether the 'data supports the theory', without recognizing the limitations of that position. The data itself is not a straightforward representation of 'reality', but a highly selective and usually spatially- and time-limited portrayal (as well as being frequently a human construct, such as 'global mean temperature' or 'sea-surface temperature'). The climate models, meanwhile, are not merely 'theory', but incorporate data through validation and as input data during model construction and operation, often using the model itself to interpolate missing data points (e.g. as for values of sea-surface and land-surface temperatures) where that data is required as an input. Once we blur the distinction between 'theory' and 'data', PM's position of comparing one against the other becomes less tenable, but so also do the climate modellers have a greater role in analysing the implications of the interrelationship between data and theory.

Unfortunately, JH does not explain clearly the role of models and observations in the construction of knowledge on climate change and human influences, though other climate scientists have done so succinctly (for example, Mahlman 2000). JH is also rather too quiet about the sources and significance of uncertainties: a clear discussion of uncertainty would have strengthened his case by showing that he was being self-critical, not over-stating his position and would have helped members of the audience to use their own judgement in evaluating knowledge-claims. Yet, it is also striking just how quiet PM is on the subject of scientific uncertainties. One is immediately suspicious of accounts of highly complex open environmental systems which leave out a proper discussion of uncertainty!

3. *Where does the warming occur?*

In the transcript, PM states that 95% of the observed warming since 1945 had occurred in air masses that were below freezing. JH questioned this high percentage, stating that: 'I don't frankly agree with the 95% number...'. During the debate PM stuck to the

95% figure, though subsequently an error was identified during peer-review of PM's paper and the correct value is apparently 78%. JH's intuition that 95% was too high a value is perhaps reassuring. In a recent paper by Jones *et al.* (1999), a global analysis of warming is presented for 1925–1944 and 1978–1987. Areas where statistically significant warming has occurred is indicated (see figure 1, Jones *et al.* 1999).

Whilst the scope of this data is not quite the same as could be the longer time-series examined by PM, it does illustrate that the areas with significant warming locally are not just confined to Siberia and NW North America, though warming is indeed more pronounced in these regions. To identify what is statistically significant, it is necessary to take account of local variability: temperatures in higher latitudes vary more than those in equatorial regions, yet PM does not appear to include local variability in his analysis (Jones 2000). It is this statistical warming which is likely to be important in terms of climate change impacts, since societies and ecosystems which are used to a relatively stable climate regime will be more vulnerable to a smaller change than societies adapted to a more variable climate. In that sense, 0.5 °C could be much more important in the tropics than say 2 °C over Siberia (Jones 2000). Clearly, however, there are unanswered scientific questions on the spatial and temporal patterns of warming. The value of the work of PM and other sceptics is that it pushes the mainstream climate scientists to address some questions they might not otherwise concentrate upon.

4. *Arguing styles*

The audience for the debate was a professional body of scholars and journalists who have an interest in science from a humanistic perspective. It is somewhat difficult to know how this audience influenced the way that the speakers presented their respective cases. In any case, I found it surprising that JH did not call upon the combined forces of the scientific community as represented in the reports of the IPCC in making his case. His commentary relies too much on his own work which, whilst excellent, is not as persuasive to such an audience as a body of work that has been thoroughly reviewed for accuracy and robustness by leading climate change scientists. That international process of extended peer review involving hundreds of specialists—supporters and critics alike—does stand for something of immense significance in terms of the reliability of the knowledge base. It is a far more rigorous process than conventional academic peer review, where three or four specialists only are involved.

PM, meanwhile, seemed at times to confuse and mystify rather than clarify the issues for the audience. For instance, his claim that the IPCC has reduced its estimate of warming due to human emissions since the first assessment report of 1990 is not quite accurate. A range of values is provided in that and subsequent reports. The range—from 1.5 to 4.5 °C global mean surface warming for a doubling of carbon dioxide concentrations in the atmosphere relative to pre-industrial—has not changed for 20 years. The more significant point, perhaps, is that the precise meaning and interpretation of the climate sensitivity has shifted over that time to accommodate new knowledge, new model runs and to maintain consistency in the range in the absence of any better estimate (e.g. van der Sluijs *et al.* 1998).

Both JH and PM miss a trick by not responding more directly to each other's criticisms—the impression given to the reader is that perhaps they do not have a response. For example, PM does not respond to JH's explanation of why you would not expect the temperature at the surface to change by exactly the same amount as it does

in the troposphere, meanwhile, over a period of 20 (as opposed to say 50) years. JH had to be persuaded at one point by a member of the audience to counter PM's points directly.

5. *Socio-economic and political argumentation*

Both JH and PM seemed to falter on analysis of the socio-economic and political ramifications of climate change. The question being debated did require of them that they address the implications of climate change, as it referred explicitly to limiting greenhouse gas emissions because of climate change. Perhaps it was asking too much of one person that they could address both the scientific question of whether anthropogenic climate change has or will occur, and infer from such climate changes the socio-economic impacts and appropriate responses *vis-à-vis* emission reductions? Certainly, JH's overall case would have been more convincing if he had had more time to argue for the scientific case that anthropogenic climate change is already, and will continue, to occur.

As for PM's views on the impacts and responses, the socio-economic arguments he made were interesting, but not a critical or thorough analysis (and clearly could not be given the limited time available). For instance, to argue that the market will simply identify low-carbon energy technologies by itself is to adopt a particular view of how markets work (largely neo-classical) which is not accepted by many professional economists. What about obstacles to the functioning of markets due to subsidies, trade barriers, oligopolies, high transaction costs, national and regional cultures, training and skills shortages, etc? What about evidence that markets left to their own devices never invest sufficiently in longer-term R&D because of the requirements for return on investment in the near term and discounting? PM makes an interesting point about the role of adaptation to climate change in modifying the extent and costs of impacts—a feedback that has tended to be underestimated by many climate impacts specialists. However, selective and anecdotal arguments about the role of incremental adaptation to past changes in parts of the USA are hardly convincing as a thorough or sufficient analysis of the role of adaptation to future climate change impacts at a global scale. None of this is to say that PM's views are not valid and interesting—only that they are highly selective.

PM was quite open about his own political viewpoints, an openness which I admire. I could not help but wonder how much these strong core values influenced his interpretation of uncertain science—science which is open to interpretative flexibility. I think that PM should have addressed that question head on. Likewise, it would have been refreshing if JH had opened-up a bit more about his own core values and how they might have influenced his interpretation of uncertain climate change science. Whilst the notion of the objective scientist appeals to our sense of the heroic individual in search of the truth come what may, I wanted to see some recognition of the scientists' own (multi-faceted) humanity. As PM points out, there are various statements that JH has made in public which do involve a selective interpretation of uncertain model results. Such interpretations cannot be 'proven' according to the conventional notion of scientific proof because climate models cannot themselves be 'proven' or 'validated' but at best 'confirmed', as Oreskes *et al.* (1994) have argued persuasively from a philosophical/logical perspective, and colleagues and I have argued with respect to climate models (Shackley *et al.* 1998, 1999, 2000).

Because climate models are associated with assumptions, beliefs, practises and instruments—what Kuhn calls a paradigm—they cannot be merely ‘objective’. If interpretation of uncertain knowledge based on prior assumptions, beliefs and practises is involved, then we have to ask what informs that interpretation, those assumptions, beliefs and practises. To maintain that social and institutional values and beliefs have nothing to do with it just seems implausible, which isn’t to say that it is never possible. For argument and supporting empirical evidence of how interpretation and judgement in the climate sphere is not simply the translation of ‘objective’ knowledge see Shackley (2000) and Edwards and Miller (2001). Most people wouldn’t spend a lifetime pioneering climate modelling, without that lived experience at personal, institutional and policy levels having some effect upon their interpretation and judgement. The mistake is perhaps to think that there is anything unusual or wrong about this influence and to close ourselves off to the possibility that multiple values are part and parcel of scientific endeavour.

6. *Concluding comments*

In this short commentary I have perhaps revealed my own position as a ‘believer’ but hope that I have also raised some critical questions for both the speakers. Overall, I agree more with JH than with PM, but admire PM’s skills as a debater. The exchange reassures me that a lively critical debate on the science of climate change is occurring in the USA, but I am concerned about the connections that are not as visible as the debate set before us here. How have the ‘contrarians’ influenced political thinking behind closed doors? How have scientific arguments been presented behind-the-scenes to the automobile, energy, agricultural and chemical industries and has this reflected the state of knowledge from both mainstream and contrarian perspectives? If not, then something is seriously wrong with the provision of scientific advice to both the private and public sectors. Ironically, in the UK we may be in danger of believing in the science *too uncritically*. Four government-sponsored climate change scenarios of temperature, rainfall, sea-level rise and other climate variables from one climate model are now being used in numerous public and privately-funded projects on climate change impacts. Whilst they cover a range of possibilities, it could be potentially misleading to believe that the four scenarios encompass the changes suggested by other models or methods.

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