On the Relevance of Climate Model Output for Decision Support

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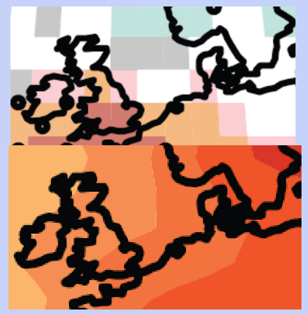
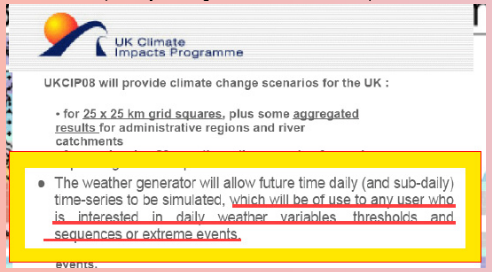
ABSTRACT: How exactly can decision-support and policy making benefit from the use of multiple climate model experiments in terms of coping with the uncertainties on climate change projections? Climate modelling faces challenges beyond those of weather forecasting or even seasonal forecasting, as with climate we are now (and will probably always be) required to extrapolate to regimes in which we have no relevant forecast-verification archive. This suggests a very different approach from traditional methods of mixing models and skill based weighting to gain profitable probabilistic information when a large forecast-verification archive is in hand. In the case of climate, it may prove more rational to search for agreement between our models (in distribution), the aim being to determine the space and timescales on which, given our current understanding, the details of the simulation models are unimportant. This suggestion and others from Smith [4] are interpreted in the light of recent advances. Climate models are large nonlinear dynamical systems which insightfully but imperfectly reflect the evolving weather patterns of the Earth. Their use in policy making and decision support assumes both that they contain sufficient information regarding reality to inform the decision, and that this information can be effectively communicated to the decision makers. There is nothing unique about climate modelling and these constraints, they apply in all cases where scientific modelling is applied to real-word actions (other than, perhaps, the action of improving our models). Starting with the issue of communication, figures from the 2007 IPCC Summary for Policy Makers will be constructively criticized from the perspective of decision makers, specifically those of the energy sector and the insurance/reinsurance sector. More information on basic questions of reliability and robustness would be of significant value when determining how heavily to weight climate model output in the decision process; one obvious example is the question of over what spatial and time averages modellers expect information in current climate distributions to be robust. The IPCC itself suggests continental/seasonal, while distributions at ***10 of kilometres/hourly*** are on offer. Our aim here is not to resolve this discrepancy, but to develop methods with which it can be addressed. This is illustrated in the context of using another physically based, imperfect model setting: using Newton's laws in an actual case of NASA hazard evaluation. Our aim is to develop transparent standards of good practice managing expectations, which will allow model improvements over the next decades to be seen as progress by the users of climate science.



Should climate scientists tell users what we know, or what they want to hear?

Numerate users of climate information (*eg* the energy, insurance and financial sectors) are both numerous and influential. While they would love high space-time resolution insights of 2080, as offered by the BBC, UKCIP and others; they question whether this(say 5km-hourly) information is any more quantitatively relevant than economic forecasts at that lead time. The credibility of science and science-based policy hangs on how we respond.

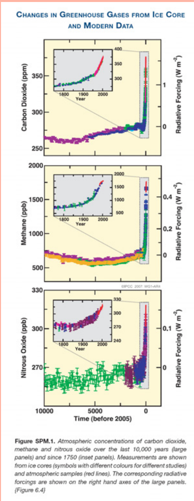
Only ~5% of UK actuaries hold climate change is a myth, and ~60% believe it will impact their work.


UK Climate Impacts Programme

UKCIP08 will provide climate change scenarios for the UK :

- for 25 x 25 km grid squares, plus some aggregated results for administrative regions and river catchments
- The weather generator will allow future time daily (and sub-daily) time-series to be simulated, which will be of use to any user who is interested in daily weather variables, thresholds and sequences or extreme events.

Observational uncertainty: noise and sampling issues

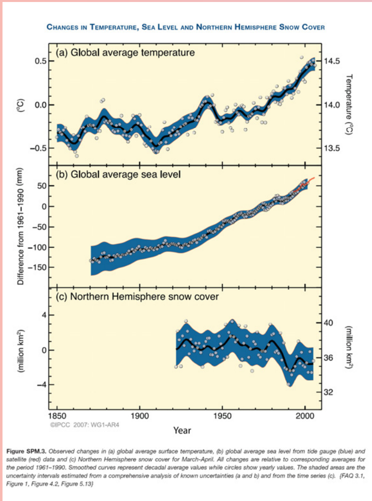


The first and third figures of the IPCC SPM [1], treat a well understood form of uncertainty, and while statistical scrutiny may bring details into question (a la Nychka), the physical picture is clear:

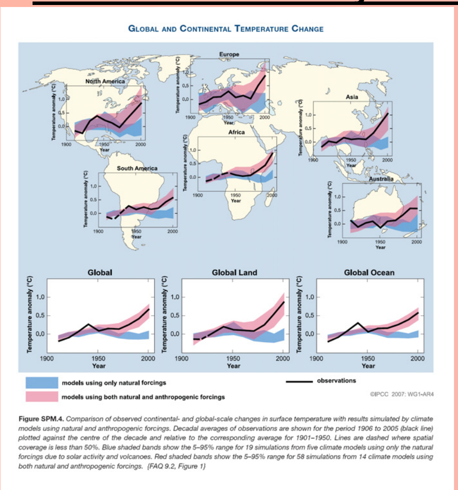
The uncertainty "bars" and "bands" reflect limits imposed by deficiencies in the instruments and sampling, and where unknown independent estimates, if discovered, are expected to fall.

Yet the SPM gives bands and bars not only for these well understood uncertainties, but also for "model range" in the future, where the method to apply this information in decision support is, at best, less clear.

Can we quantify the value-added by running large ensembles of GCM simulations? And improve the presentation to optimise communication with the users of climate science leading to better decisions and policy in practice?

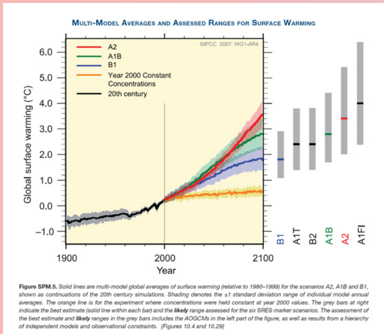


Multi-model variability is not a traditional form of uncertainty

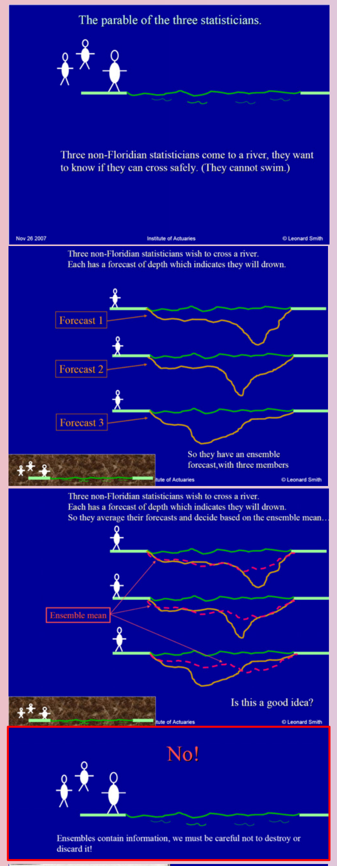


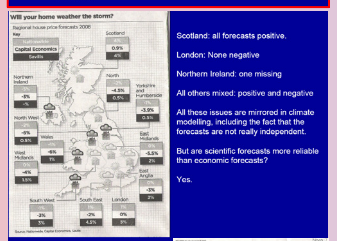
The graph on the left, SPM4, again shows observation based curves, but this time with model based "bands." It is far from clear how one sure interpret these bands, when the models are known ***not*** to relate to any simple sampling interpretation.

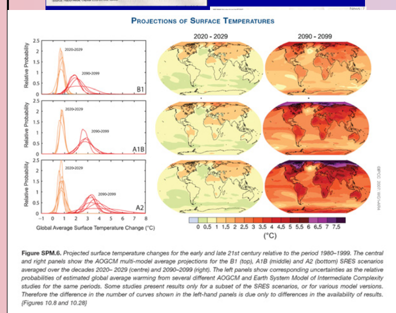
SPM5 (right) segues into the future, with uncertainty bands in the past *and* model ranges in the future. What do these mean?



Can you correct for counting stats?
What information do the bands aim to convey to policy makers here in the SPM?
Why is the stronger forcing (red band) more narrow than the green? (17, 21, 21, 16)
[Thanks to Reto Knutti for numerous discussions on this.]



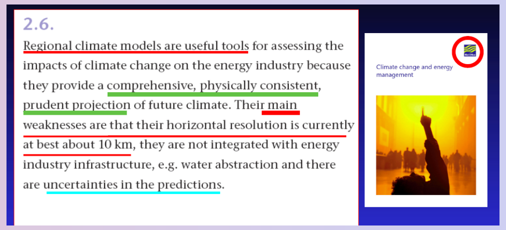


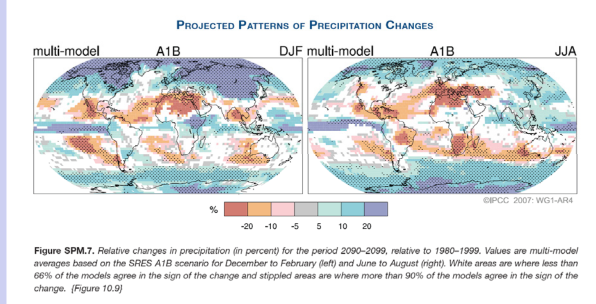


What do we know? What do we know we don't know? What might we learn soon? (where users define "soon")

Do we risk our credibility by selling what we know is not robust?

Scotland in While & Red (SPM6 & 7)


2.6.
Regional climate models are useful tools for assessing the impacts of climate change on the energy industry because they provide a comprehensive, physically consistent, prudent projection of future climate. Their main weaknesses are that their horizontal resolution is currently at best about 10 km, they are not integrated with energy industry infrastructure, e.g. water abstraction and there are uncertainties in the predictions.



If we believe our models are able to give probability forecasts that can be quantitatively used as such, then it is reasonable to expect **Probabilistic Similarity**: future forecasts will refine current forecasts, we do not expect revolutions due to resolving some model inadequacy. Given the currently level of disagreement between models below continental scales, is it reasonable to describe our projections at 10km scales as "comprehensive, physically consistent and prudent" for Scotland (top right SPM7)? And why is the model grid so unclear on the lower blow-up? Where is the justification of treating a PDE in this way?

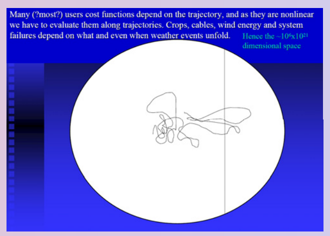
Is the greatest thing climate science has to fear climate science itself?

Negative impacts on science-based policy and decision support can be expected from:
(1)The now diminishing voice of nay-sayer sceptics, who are exhausting their well warn ammunition. (N. Orsekes)
(2)The increasing voice of non-physical scientists (statisticians) making innocent category errors. (S Armstrong)
(3) Climate scientists trying to deliver what is "required" rather than what is known, thereby feeding (1) & (2).

Numerate users are not unfamiliar with ensemble forecasts, and their limitations (see House forecasts in centre), yet they need guidance on whether they should treat climate as they do car accident-risk given gender-age, or as capital risk in the property market. What methodology do we hold they "should" adopt? Is figure SPM2 relevant here?

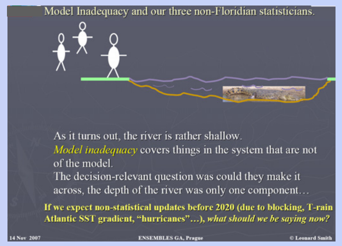
Open Questions for Climate Science in Decision Support:

What space and time scales do we have decision relevant information, probabilities that could be used as such?
When models disagree (in distribution) can we expect to do better than *potential pathways*?
How do we clarify the difference between economic modeling and physical simulation? Why trust simulation more?
Can we argue our simulations are ***Structurally Robust***? Are the distributions of interest robust against model details?
Is there any mathematical foundation for the claim that we will converge toward something (in distribution) where the details (parameterizations, parameter values and structure) do not dominate the outcome at scales of interest?
Can we provide a better method of application than pathways and analogy, if our probabilities are not to be used as such?



New ways Forward

Users often have nonlinear, threshold dependent cost functions; reproducing means and autocorrelations is of little value. Looking for shadowing trajectories [4,5,6] while noting their duration at various resolutions might inform users and model developers.
How can we make our models more relevant?
How can we exploit their empirical targets to speed improvements in model adequacy?



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References: Many available from the publications pages of www.lsecats.org
Discussion welcome at cats.lse.ac.uk/forum

[1] IPCC, 2007: Summary for Policymakers. In: *Climate Change 2007: The Physical Science Basis. Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change* [Solomon, S., D. Qin, M. Manning, Z. Chen, M. Marquis, K.B. Averyt, M.Tignor and H.L. Miller (eds.)]. Cambridge University Press, Cambridge, United Kingdom and NY, NY, USA.
[2] LAS(2007) *A Very Short Introduction to Chaos.* OUP [3] N. Cartwright (1983) *How the Laws of Physics Lie,* OUP
[4] LAS(2002) What Can We Expect From Climate Models? PNAS **99**: 2487-2492
[5] LAS(2003) Predictability Past Predictability Present, now in *Predictability*, T Palmer & R. Hagedorn CUP (2007).
[6] K Judd *et al* (2007) The Geometry of Model Eror, JAS (in press)