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Abstract

Extracting Insight from Predictions of the Irrelevant:

Can the Diversity in Our Models Inform Our Uncertainty of the Future?

The open question of whether or not “physically reasonable” solutions of the Navier-Stokes equations even **exist** is one of great mathematical interest and nontrivial monetary reward. For a mathematician such a question is of interest for its own sake. For the geophysicist (or policy maker) interested in the Earth System, however, the interesting questions focus on our ability to adequately simulate partial differential equations thought to describe the climate system. Perfection is a non-starter here; even before we compile our models, we know they are not “valid” representations of the Earth System, as we have built blatant untruths into each and every one of them. So the question is not whether they are perfect but whether they are useful. Can they predict only the irrelevant? Given that we know our models are empirically inadequate, precise probability forecasts based on model-output should not be interpreted naively as decision-relevant probabilities; how then might our simulations provide insight?

Section 11 of Tukey’s 1962 paper, “The Future of Data Analysis,” titled **Facing Uncertainty,** was often quoted by Albert Tarantola. Although written for 1960’s data analysts, it is of equal value to geophysical modellers of this century: “The most important maxim for data analysis to heed, and one which many statisticians seem to have shunned, is this: "Far better an approximate answer to the right question, which is often vague, than an exact answer to the wrong question, which can always be made precise."” Nonlinearity exposes the limitations of least squares methods, as Tarantola stressed in “Inverse Problem Theory.” State uncertainty in nonlinear systems undermines the use of least squares methods in a manner not unlike the way structural model inadequacy undermines the use of model-based probability distributions. What might it mean to put a prior on an empirically vacuous model-parameter? Could it be rational to base policy on the posterior probability distribution of a model-variable, knowing that the model (class) considered was empirically inadequate? Is the theory of probability, as such, irrelevant for decision-support in extrapolation problems like climate change?

“Kitchen sink” models aim to provide the “best available” answer to an intractable problem, by including eve5een a priortiocontce ivichwhb(m)-2.45995.74(m)-2.45995(s)-1.22997( )-0.2(m)]TJ0.223(m)

Questioning the policy relevance of model-based “implied-probabilities” leads to the maintenance of parallel pure and applied research programs over the timescales on which climate policy will demand scientific support (five years and fifty). This also raises the possibility of moving away from probabilities, perhaps towards non-probabilistic odds. How wide is the gulf between programs advancing scientific understanding and those constructing a basis for evidence-based policy making? If applying cost-benefit analysis directs us to the wrong questions, might designing climate experiments to inform a risk management framework answer the right question, approximately?

One cannot take today’s climate models literally given the range of systematic errors even in global mean temperature, a range far exceeding the observed centennial increase. In terms of insight, however, todar46()3.74(r46