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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Hilbert problems for the geosciences in the 21st century – 20 years later" *by* Michael Ghil

Anonymous Referee #2

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This paper is essentially an essay, based on an earlier paper written by the same author at the turn of the millennium which posed a set of ten pivotal "big picture" problems facing the geosciences at the time, particularly in relation to climate dynamics, prediction and the possibility of the eventual "enlightened" control of our planetary environment. The vision presented to the reader is on an immensely broad canvas and presents what is clearly an authoritative though personal view of progress (and none the worse for this!), from the perspective of a scientist/theoretician who has been a prolific and influential proponent of the dynamical systems approach towards understanding and modelling the behaviour of complex systems.

The author wisely restricts his "20 years later" update to reviewing just a subset of his Hilbert problems, focusing on both some recent advances (a) in modeling aspects of

the physical climate system (notably on low frequency variability on timescales ranging from the inter-annual to the multi-decadal) in the atmosphere and oceans, and also (b) on the formulation of approaches to modelling the dynamics of global and regional economies and their possible interactions with and responses to variations in the climate. In (a), it is evident that advances in both analysis methods (such as Ghil's own approach to multivariate singular systems analysis) and modelling approaches based on non-autonomous and random dynamical systems have led to new insights into systems as complex as the coupled atmosphere-ocean climate system. In particular, that they can develop much more complicated cyclic or chaotic behaviour than simple, steady equilibria with timescales that are much longer than those of imposed forcings, through bifurcations between pullback attractors - including the intriguing posslibities of more than one such attractor coexisting at the same point in parameter space. Such possibilities have important lessons and implications for scientists modelling these systems and using them to predict future climate responses to changes in forcing.

Particularly intriguing (certainly for this physical scientist!) are the author's recent forays, reviewed in (b), into applying his dynamical systems approaches (alongside others) to similar kinds of problem facing economists in trying to capture the complex dynamics of large scale economies and understand how they may respond to external changes in the environment, especially involving extreme, high impact events. From his perspective as an expert in dynamical systems, Prof. Ghil takes a fascinatingly critical view of how economists have approached the problem of formulating macroeconomic models over time. In particular, he notes some parallels in how ideas have evolved in economics with the world of climate dynamics, in which early ideas focused on predicting and understanding evolution towards simple equilibria but more recent work has moved on to explore non-equilibrium (cyclic and/or chaotic) behaviours, both exogenous and endogenous, which appear to show greater promise in emulating how real economies actually behave.

The overall result is a fascinating, concise (though in places perhaps a little too con-

cise?) and quite readable synthesis which indicates some real progress in pulling together these diverse and important topics in both geosciences and the social sciences. The article is well referenced, and would certainly be expected to stimulate many readers to follow up some of these ideas in the cited literature. My only gripes with the paper in its present form are summarised in the short list of questions and clarifications below. But hopefully the author will take these on board in revising his paper for final publication.

Specific points:

P.8 lines 1-2. The reference here to “an important step in achieving greater rigor in this field is a greater reliance on the counterfactual theory for necessary and sufficient causation (Pearl, 2009) in the attribution of such extreme events” reads somewhat strangely. How can a “counterfactual theory” be rigorous (let alone correct?)? Perhaps “counter-intuitive” is intended here, or I may have completely misunderstood what is intended? Otherwise some additional explanation on what is meant (and what the “theory for necessary and sufficient causation “ actually is) would be welcome - especially since the book cited is not likely to be widely accessible to many readers.

Figure 3 and associated text. Some further explanation of both the context of this figure (perhaps specifying in more detail the DDE being solved and the relevant parameters?) and what the (unlabelled) z axis represents would be very welcome here.

P.3 introduction: The end of this section seems to suggest that we will learn about progress on Ghil’s problem (10) “Can we achieve enlightened climate control of our planet by the end of the century?” in later sections. But the actual discussion in sections 4 and 5 don’t really address this question, at least not directly. Are we anywhere near being able to answer it - perhaps with emphasis not only on the technical feasibility but also on the “enlightened” nature of the policies underpinning such control? Some more discussion in Section 6 might bring this to a more satisfying conclusion?

P. 10 line 22 “heterogeities” [typo]

P.19 line 19. “Atmosphetic” [typo]

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