Review #6

PROPOSAL NO.: 0417929
INSTITUTION: U of Colorado Boulder
NSF PROGRAM: GEOPHYSICS
PRINCIPAL INVESTIGATOR: Ritzwoller, Michael H
TITLE: Collaborative Research: CMG: Uncertainty and Physical Constraints in Seismic Inferences
RATING: Very Good

REVIEW:
What is the intellectual merit of the proposed activity?

The proposal represents a rare union of a highly competent 'get your hands dirty with the data' seismologist and a renowned statistician interested in geophysical problems. Both are high calibre scientists who have previously made telling contributions to their fields. As I see it the proposal mainly falls into two parts; firstly they propose to develop rigorous ways to quantify uncertainty for certain seismic inverse problems, namely those involving surface wave datasets; and secondly they propose to develop ways of imposing constraints from a variety of geophysical information (e.g. heat flow, seismic etc.) into a single framework, using hypothesis testing. Both of these are important goals in which little real progress has been made to date.

The issue of quantification of uncertainty in inverse problems, is in my view paramount to rigorous inference. Without it we end up in the situation we are at present, where the preoccupation of many seismologists is with generating more and more models (the model construction problem) rather than with inference problem, i.e. we have many seismic models and little way of making informed judgment about what parts of them to believe, or how to compare them.

The issue of unifying disparate data types to make inferences on data classes is a difficult but important one which has never really been solved. Certainly there are no 'handy' methods around. A real success in this area would be very difficult in my view, but an extremely important one.

One area that may be a potential difficulty is whether the two proponents really speak enough of the same language to make major advances. No one can really guarantee this, but the track record of the two and especially of Prof. Stark, make me think that its possible here. I certainly think they are in an ideal position to do so.

An aspect which is only lightly touched on in the proposal is that of quantifying and combining errors in the disparate data types. This is, of course, closely linked to the quantification of uncertainty in the output seismic models, or seismic 'corridor' of acceptable models, since data errors feed into model errors. It wasn't entirely clear to me whether they were assuming that error characteristics for individual data types would be known or whether they plan to devise techniques to rigorously estimate these as well. The latter would be preferable.

I’m also more than curious as to how one rigorously assesses fit to a disparate set of data types, and would think that anything they produce in this area would be very interesting form a theoretical viewpoint and potentially far reaching in its significance to seismologists pre-occupied with model construction.

What are the broader impacts of the proposed activity?

Although I think the questions being addressed in this proposal are important and potentially widely useful in the geophysical community, I believe that two factors are crucial if the research is to achieve a significant broader impact.

The first is that the resulting methodologies for the surface wave tomography be properly communicated to the broader seismic community. Here I don’t mean just published in leading journals, but rather explained to the non-specialist to the point where others might be able to implement such methodologies. What good is it if just a single source of tomographic models comes with ’rigorous uncertainties’ surely the real benefit is when everyone is able to do so, and hence comparisons between independently generated models can be placed on a proper quantitative basis. This is especially important if the uncertainties turn out to be more pessimistic than the community expects.

The second factor which, is somewhat beyond the scope of the present proposal, but nevertheless worth mentioning, is whether the ideas developed here could be extended beyond the particular problem of surface wave tomography. The need to combine independent information from multiple data types is a self consistent manner and also understanding the associated model uncertainties is an important issue for many geophysical inverse problems, for example combining body wave, surface wave tomography, free-oscillation and normal data in seismology; combining seismic, mineral physics and geochemical data for mantle structure; of getting more ambitious, geochronology and mantle dynamical modelling with all of the above. The list goes on. I think the big picture needs to be kept in mind with the present study, although the authors are correct that the surface wave and heat flow data is a good place to concentrate on.

I feel that graduate students who can truly bridge the gap between areas of statistics and geophysics have a lot to offer, and would also be a valuable outcome of the project. Another would be an important bridge between scientists in the geophysical and statistical communities.

Summary Statement

This is an exciting proposal which is very timely, given the current lack of rigorous ways of quantifying uncertainty in all but the simplest problems, and the exponential growth in disparate data-sets becoming available. I think a success in the area of usefully combining information from disparate data is probably the most exciting feature. I rate the proposal highly and will keenly await the results.

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