Reply

CARL WUNSCH

Department of Earth, Atmospheric and Planetary Sciences, Massachusetts Institute of Technology, Cambridge, MA 01239

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Veronis raises a number of issues about my paper (Wunsch, 1985). I will respond only briefly as I believe that the reader interested in understanding whether there is any substantive controversy here is best-advised to decide for himself by reading both Fiadeiro and Veronis (1984) and my own paper, as all the major issues raised by Veronis are discussed in detail there. The proof of this particular "pudding" will be in whether others find the methods and examples described by me in Wunsch (1985) useful or not. If the only recourse to the use of undetermined systems is to over-determine them, then the examples and discussion I give will be ignored and forgotten.

I will confine myself to a few specific issues and not repeat what I have already said in the published paper. I am accused of a variety of sins of omission and commission.

1) The original title of Wunsch (1985) contained the subtitle "Comment on a paper of Fiadeiro and Veronis," but they declined to reply, and the editor decided that the paper should stand on its own. I took the opportunity to dwell on some wider issues rather than simply comment on what they had done—hence the discussion of row and column scaling that would be important considerations in less artificial circumstances.

2) I emphasized the availability of techniques for solving and interpreting underdetermined systems. Because Fiadeiro and Veronis decided the only sensible thing to do with an underdetermined problem was to over-determine it (a very strong message in their paper), my concern was that others might be prematurely discouraged from trying procedures that I regard as powerful and useful, and for which the extensive experience in other fields is directly relevant.

3) Veronis' discussion of a strawman underdetermined system doggedly ignores the fact that it is exceedingly rare to confront an underdetermined problem in a complete vacuum with zero information about likely magnitudes, ranges, space or time scales. For the example he describes, an inverse method would produce the result that no information was available to change an initial estimate of the unknown parameter, and that would be the end of the story. He also misses the point that when he chooses a numerical grid, an underdetermined problem has implicitly and automatically been forced to become a less-underdetermined, or even a fully determined one, by the simple act of disallowing solutions with rapid variation between the grid points—a scale assumption of precisely the same form as the one I used in imposing explicit problem covariances on the larger-than-grid scale.

4) Everyone would agree that the most desirable situation is to have adequate data to over-determine a problem, but there are commonly more things to determine than there are data, and some decisions are necessary. A number of options exist and the results need not be "devoid of physical content." One might reduce the number of model parameters being determined so that the problem becomes over-determined. This procedure is often the best one. In other cases, it requires great care and much experimentation, as the precise nature of the reduction in parameter space may determine the nature of the solution, introducing inadvertent biases. A good example is a box model of the ocean circulation. If one represents the entire ocean by, say, a two- or three-box model, the modes of motion and of response to external perturbations are severely limited by the choice of model. It may be better (as discussed by Wunsch and Minster, 1982) to live with a formally underdetermined model in order to determine which degrees of freedom are determinable, which are indistinguishable, and what new data would most efficiently reduce the indeterminacy. An alternative is to introduce scale, anisotropy, or other kinds of prior information, as several examples in my paper show.

The question of why $\langle \Psi^2 \rangle = 0.4$ was imposed is easily answered. This simple example demonstrates that even extremely crude information, such as an approximate estimate of the solution energy level, can be used and often helps a great deal. It is in this way, for example, that ideas about the total kinetic energy found in the ocean can be used to estimate not only the velocity field, but also the fraction of the flow field that has not been determinable (as stated in my Abstract).

All fluid flows are observationally underdetermined, as even with perfect data and a global, uniform network they always have scales smaller than the smallest
observational or numerical grid spacing. Various smoothing operations or grid choices can hide this ultimate underdeterminacy very effectively, but it is still there, and it occasionally reappears abruptly, as in the long-range weather forecast problem. Underdetermined systems are susceptible to biases, of course, but they are determinable and controllable, through scaling procedures.

5) Veronis claims my discussion of “projection” is nothing but a restatement of the process. The purpose of the discussion was to point out that projection is familiar and useful in many other contexts, including ones in which there is little or no visual resemblance to the original field, as in a band-passed current meter record.

6) The issue of resolution and variance in the rotated space was discussed in my 1978 review paper and in Wunsch et al. (1983), and in both cases, I pointed to the extensive and clear account by Wiggins (1972). A reproduction of the details is out of place in a brief comment.

7) Veronis states that my section 4 does not treat underdetermined systems because “inequalities count.” The true solution magnitude is \( u = 0.625, v = 0 \); with the bounds used, \(|u|, |v| \leq 4\), the problem has an infinity of solutions, many differing radically from the correct one. How such a problem can be labeled “overdetermined” is mystifying.

Rather than attempting to respond to all of the remaining detailed criticisms made by Veronis, I will end as I began, by stating that any real controversy here will be resolved by the extent to which inverse methods are found by the oceanographic community to be useful. If they are found to be unenlightening or confusing at best, they will rightfully end on the scientific trash heap, the main cost having been a waste of time and printers’ ink. I would be the last to claim that my papers are error free or as clear as they might be. They do reflect, as well as I am able to express it, with inevitable mistakes, what I have done and what I think is important.

If inverse methods are interpreted in the widest possible sense as systematic procedures for comparing ideas with data, then their use will surely grow as the oceanographic database grows along with the complexity of our theoretical ideas. I have little doubt that the procedures and numerical methods now in use will be improved and in some cases replaced by better ones (as has happened already), but the fundamental notions of what the methods are about and what they can do will not change.

In the Art of reasoning upon Things by Figures, 'tis some Praise, at first, to give an imperfect and rough Draft Model, which upon more Experience, and better Information, may be corrected.

Charless Davenant (1698; quoted by Edwards, 1972).

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REFERENCES


