Journal of Marine Research, 42, 259–262, 1984

Comments on "On the determination of absolute velocities in the ocean" by M. E. Fiadeiro and G. Veronis

by Lee-Lueng Fu[1]

Fiadeiro and Veronis (1982, FV hereafter) proposed a procedure that they claimed would lead to the best reference level for computing the absolute velocities in the ocean by the inverse method as described in Wunsch (1978). According to FV, the reference level obtained by the procedure is best because the correction required of the absolute velocities at this level to satisfy a set of imposed constraints is minimum among all the possible levels. However, one likes to ask whether the velocity field resulting from this choice of reference level is necessarily the best in the sense of being closest to the true velocity field. This question was not addressed in FV, but its answer could clarify, to some extent, the perspective of their procedure in the long-pursued, controversial search for the absolute velocities in the ocean.

Across a given hydrographic section, the difference between the true total velocity field (reference plus relative) and its estimate by the inverse method is simply $\bar{\mathbf{b}} - \mathbf{b}$, where $\bar{\mathbf{b}}$ and $\mathbf{b}$ are column-vectors whose elements are the true and estimated absolute velocities at a reference level, respectively. Therefore, the best reference level should be the one that minimizes $(\bar{\mathbf{b}} - \mathbf{b})^T\ (\bar{\mathbf{b}} - \mathbf{b})$. In general, $\bar{\mathbf{b}}$ can be expressed by the following equation

$$\bar{\mathbf{b}} = \mathbf{b} + \mathbf{N}\alpha,$$

where $\mathbf{N}$ is a matrix whose columns are the null-space vectors and α is a column-vector whose elements are the null-space projections of $\bar{\mathbf{b}}$. The requirement of minimum $(\bar{\mathbf{b}} - \mathbf{b})^T\ (\bar{\mathbf{b}} - \mathbf{b})$ is thus equivalent to that of minimum $\alpha^T\alpha$. Now the question to be asked is the following: Does the reference level that minimizes $\mathbf{b}^T\mathbf{b}$ as the one obtained by FV necessarily also minimize $\alpha^T\alpha$? The answer is obviously a negative one: the velocity field with minimum projection in the "activated space" does not necessarily have minimum projection in the "null space"; otherwise, minimum projection in the activated space would imply minimum total energy (sum of the null-space and activated-space projections). In general, the dimension of the activated space is much smaller than that of the total space (activated plus null), so the energy in the activated

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space is not necessarily proportional to or even monotonically varying with the total energy. For instance, in the example of the Tasman-Coral Sea section given in FV, the dimension of the activated space was only four, but that of the total space was twenty-three. One might argue, however, that certain statistical properties of the velocity field could make the projection in the activated space proportional to that in the null space. For instance, if the horizontal wavenumber spectra of the velocity field at different levels have the same shape with different energies, then the two projections are proportional to each other. But one has to explicitly state and justify assumptions like this.

The fundamental assumption underlying FV's approach, however, is the existence of a level of no motion in the true absolute velocity field. What they did was to try to find this level and used it as their reference level. This assumption has not been made in most of other inverse studies of the problem (e.g., Wunsch, 1978; Roemmich, 1981; Fu, 1981). The reason for minimizing $\mathbf{b}^T\mathbf{b}$ in those studies is not that the reference level is assumed to be a level of no motion, but that one does not know how to add the null-space components that inevitably exist. The solutions presented in those studies have been meant to be only particular examples of an infinite number of acceptable solutions.

Given that the assumption of the existence of a level of no motion is true, then FV's solution indeed represents the true velocity field because $\alpha^T\alpha$ is identically zero on a level of no motion. However, as demonstrated in Fu (1981) and Wunsch and Grant (1982), the existence of a level of no motion can be in violation of mass conservation in some parts of the ocean. Moreover, even though one can find a level of no motion that is consistent with mass conservation as FV did in the Tasman-Coral Sea, one still cannot prove its actual existence in the ocean because the projection in the activated space does not delineate the whole picture. Therefore FV's solution should be considered as a "particular solution" just like other inverse solutions, except that FV's solution is optimal if there exists a level of no motion in the ocean. However, because there is no fundamental reason why there should exist a level of no motion in the ocean, FV's solution is not necessarily better than any other solutions based on a reasonable reference level, say, at 1600 db.

The reason why one would argue that one reference level is better than another is that he has not incorporated all of his knowledge, assumptions, and even prejudices about the velocity field into the formulation of the problem. Ideally one should formulate the problem such that it takes into account all of the prescribed properties of the velocity field so as to make all the reference levels lead to equally possible solutions. Among these solutions, one then has no reason preferring one to another. For instance, if some constraint is made to require that the flow at certain deep levels be "reasonable," then one can even choose the surface as his reference level and still obtain an acceptable solution. In the face of an undetermined problem, the best one can do is to present the range of acceptable solutions in terms of physically interesting variables like mass, heat, and salt transports that can be compared with results from other approaches such as direct observation and numerical modeling. In practice, however, it is usually difficult to carry out such an approach, because either some of the constraints are very difficult to formulate mathematically, or the resulting problem is very difficult to solve. One then inevitably has to face the choice of proper reference levels. Ideally one can make a Monte Carlo selection (e.g., Press, 1968) to avoid any bias arising from preconceived or oversimplified notions of the velocity field, or at least one can explore a few plausible candidates (e.g., Wunsch, 1978) instead of limiting himself to a single choice based on assumptions that are difficult to justify.

As a final remark, the imposition of additional dynamical constraints to the velocity field may be able to make the problem overdetermined and thus have a unique least-squares solution. Welander (1983) has recently proposed along this line an approach that requires the conservation of potential vorticity and Bernoulli function; however, the applicability of his approach to real oceanographic data has not yet been demonstrated.

Acknowledgments. The research described here was performed at the Jet Propulsion Laboratory, California Institute of Technology, under contract with the National Aeronautics and Space Administration. Professor George Veronis has made some valuable suggestions on the presentation of this paper.

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Reply to L.-L. Fu

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The approach that has been adopted by Wunsch (1978), Roemmich (1981), Fu (1981) and others who have applied the inverse method to the large scale circulation

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problem is to start with a reference surface at which the velocities are set equal to zero and then to find the velocity correction with minimum amplitude required to satisfy the conservation constraints. In our article (Fiadeiro and Veronis, 1982) we have proposed a search procedure that examines a large range of starting surfaces (based on depth, potential density and potential vorticity). The minimum correction is found for each of the chosen surfaces and the one that has the minimum of all these minima is selected as the optimum choice for a surface of no motion. That choice is our way of resolving Fu's statement: "One then inevitably has to face the choice of proper reference levels."

We have chosen zero velocities at the reference surface just as Wunsch, Roemmich and Fu have had to do in practice. We have certainly never claimed more than Fu is willing to accept: "Given that the assumption of the existence of a level of no motion is true, then FV's solution indeed represents the true velocity field. . . . " Nor do we disagree with Fu's statement: "Moreover, even though one could find a level of no motion that was consistent with mass conservation as FV did in the Tasman-Coral Sea, one still could not prove its actual existence in the ocean. . . . " Since all of our results are based on an assumed level of no motion, we cannot prove that it exists; we can only show consistency.

The objection raised by Fu is that we did not ask the question of how close our calculated velocities are to the true velocity field. It seems to us that this is an idle question. We start with a limited amount of information, and the velocity field that we obtain is constrained only by that information. In common with the other studies our analysis can say nothing about the nullspace vectors.

As Fu observes, the formulation of the inverse problem to obtain velocity corrections leads to a highly underdetermined system of equations with infinitely many solutions that satisfy the constraints with tolerable error. The issue is not one of finding solutions (that can always be done) but of providing an acceptable rationale for choosing a best, or a class of best, solutions. Our procedure makes maximum use of the physically reliable content of the data (geostrophic shear) and consistency with the constraints (mass conservation). It minimizes the contribution of the inverse correction, which has unavoidable uncertainties. Once one has obtained an optional velocity field in this fashion, one can go on to explore the effects of nonconservative processes. In a subsequent publication (Fiadeiro and Veronis, 1983) we have shown how we extend the procedure to obtain information about the air-sea heat exchange in the western North Atlantic.

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