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Reply to comments by Lerch *et al.* on 'The Earth's shape and gravity field: a report of progress from 1958 to 1982'

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The paper by Lambeck & Coleman (1983, L/C) to which Lerch, Klosko & Wagner (1986) take exception is a perspective, perhaps a slanted one, of important progress made in the observation of the Earth's global gravity field during the 25 years of the *Geophysical Journal* up to mid-1982. This is stated nowhere more clearly than in the sub-title: 'a report of progress from 1958 to 1982'. Readers of the scientific literature over those years would have been aware of the enormous progress that had been made in this area of geophysics. Yet readers, particularly those interested in the geophysical interpretation of these fields, may have mused on some of the differences that appeared amongst the various published solutions. They may also have asked why it remains necessary to compute new gravity field models each time a new satellite is launched. What we set out to do, *inter alia*, was to highlight some of the differences between published solutions and to suggest why they may have occurred. A major motivation in doing this was that some of these differences could be interpreted as geophysically significant features by unwary users of the published geoid or gravity anomaly maps.

Our essential conclusions were threefold:

(1) Differences between the published results (those discussed in L/C) are greater than would be expected from statements made in those papers about the accuracies of the individual solutions. Particularly significant differences occurred for the higher degree and order Stokes coefficients.

(2) Detailed documentation of the solutions was not always available, clear, or complete.

(3) Considerable improvements can still be made in both methodology and in the data evaluation.

The first conclusion serves as a warning not to take all lumps and bumps in the shape of the Earth too literally, in particular as many of the more substantial discrepancies occur in tectonically interesting areas (see L/C, fig. 8). This conclusion appears to have been lost in the response by Lerch *et al.* in which the emphasis is placed on the very low degree harmonics rather than on the geophysically more interesting higher degree coefficients. That the then current models, or the presently available models, are not wholly adequate is also seen in the need to compute specific gravity field coefficients to describe the orbits of satellites such as *SEASAT* (Lerch *et al.* 1982b; Marsh & Martin 1982) or *STARLETTE* (Marsh, Lerch & Williamson 1985). In making the second point it was hoped that more information on past work could be forthcoming, and that future work would be more carefully documented. The current response by Lerch *et al.* is already beneficial in this respect and a number of the questions we raised have been answered in the recent papers by Lerch *et al.* (1985) and Reigber *et al.* (1985). The third point was made in the belief that such improvements are required before the next generation of gravity research missions is launched.

We examined solutions emanating from three institutions; the Smithsonian Astrophysical Observatory (the SE series of models), the joint French–German effort (the GRIM series) and the Goddard Space Flight Center (the GEM models). The latest models available at the time of writing the paper were respectively SE 1980, GRIM 3 and the GEM 10 series. Rightly or wrongly we did not discuss GEM 9 because GEM 10 is its direct derivative and presumably the better solution. Our discussion was primarily of these models and, for historical reasons, of some of the earlier models published by these three groups. The L/C paper is not intended as an attack on any one solution and criticisms were levelled at all three. One place where we have been remiss, unrelated to the present Lerch *et al.* comment, is that we downplayed the solutions based on surface gravity data, particularly those by Rapp (e.g. 1977, 1984).

Much of the comment by Lerch *et al.* refers to their GEM L2 model. This model was not published until 1982 November and then in a very abbreviated form (up to degree and order 6) (Lerch, Klosko & Patel 1982a). The more detailed technical report, giving the solution to degree and order 20 (Lerch, Klosko & Patel 1983) did not appear until 1983 February and the *Journal of Geophysical Research* paper cited by Lerch *et al.* was not published until 1985 September. Lerch *et al.* state that our estimates of precision or accuracy of the low degree coefficients in their solution are too large by an order of magnitude. From the brief comparison we were able to make of the GEM L2 solution, between the time of submission and page proofs, we concluded that their solution was probably accurate to about 50 cm for harmonics of degree $l \leqslant 5$ (see p. 45). The statistical evaluations of these low degree coefficients suggests a rms error of about 1 m for harmonics of degree 2–5 (see L/C table 4) but we emphasized the unreliability of the estimates that are based on small samples (p. 38) and also suggested that a major part of the differences occurred from the (3, 3) coefficients. We considered that it was the Gaposchkin solution that was likely to be in error for these coefficients because the analytical description of the orbits does not include interactions between the zonal and tesseral harmonic perturbations. We subsequently stated that the precision of the low degree GEM L2 coefficients was probably more like 50 cm. Lerch *et al.* (1982a) do not give the precision estimates of their fifth-degree coefficients but, if we adopt the same precision for the fifth-degree coefficients as for the fourth-degree Stokes coefficients ($\cong$ 3.5 cm in terms of geoid height), their rms geoid precision for $l \leqslant 5$ is $\simeq 14$ cm. The difference is a factor of 3–4, not an order of magnitude. Incidentally, our estimate is consistent with Tai & Wunsch's (1983) ability to determine the sea surface topography from altimeter data and our more pessimistic estimate of accuracy does not invalidate their results as stated by Lerch *et al.* in their comment. We certainly have not stated that the GEM models are inferior to the others. On pp. 38 (lines 24, 25), 44 (lines 11–12 of Discussion) and again on p. 45 (line 19) we state that we consider the GEM 10 solution to be superior to the others. Perhaps we simply have not said it loudly enough.

Lerch *et al.* also comment on our method for comparing solutions. We used a statistical approach, one whose limitations for evaluating small subsets of coefficients were emphasized (e.g. pp. 31, 34 and 38). Nevertheless, these tests do reveal significant differences between the various solutions, although they do not indicate where the problems lie unless *a priori* covariance matrices are introduced. These were not available, but some of the more recent models do provide this information. Other tests had already been developed by Klokočník & Pospíšilová (1981) who drew a conclusion similar to ours about the precision of the low degree harmonics.

We did not consider the GRIM 3 model in detail because we were aware of problems with this solution (see L/C, p. 38). A much improved model (GRIM 3 L1) has now been published and comparisons with the GEM L2 model have been made (Reigber *et al.* 1985). Agreement between the two models for the low degrees is indeed improved, as has already been noted by Klokočník (1985), particularly for the low degrees. One would have real cause for concern if they were not, for these solutions use comparable *LAGEOS* data sets (about 16 months of data in the GRIM solution and 30 months in the GEM solution). Hopefully, it means that it will no longer be necessary to adjust gravity fields each time a new satellite is launched as is the current practice. Yet disagreements for the shorter wavelength variations in gravity or geoid height remain disturbingly large. Reigber *et al.* cite differences between GEM 10B and GRIM 3 L1 of nearly 20 m for $5^\circ \times 5^\circ$ area means, and an rms average for this difference of 2.4 m. Differences between GRIM 3 L1 and GEM L2 (presumably for harmonics up to degree 20) have an rms of 2.6 m and a maximum difference of 13.5 m. Maximum differences for $5^\circ \times 5^\circ$ area mean gravity anomalies exceed 30 mgal and the rms difference is 9 mgal.

Much of the remainder of the Lerch *et al.* comment is made up of comparisons of models (GEM L2, GRIM 3B) not available in 1982 when the L/C paper was written and it would not be appropriate for us to comment on these solutions without first repeating our earlier tests. Nevertheless, one is tempted to ask why do they compare SE 1977 with their models, and not the more recent model published in full by Gaposchkin (1980) as did L/C, which contains some *LAGEOS* data (albeit only two arcs)? One is also tempted to ask why it is that in one paragraph the statistical tests are said to be invalid because the different solutions used similar data sets (end of paragraph 1, section 1), but these solutions suddenly become 'reasonably independent' some pages later (paragraph 4 of section 3)? These tests seem to be at least as independent as comparisons of successive quasi-iterations of the GEM models. Do the comparisons in fig. 2 not raise questions of why model GRIM 3B has less power than GEM 9 at high degree when the former contains altimeter data and therefore the oceanographic sea surface topography signal?

If we have been overcautious in estimating the precision of the low degree harmonics in the GEM L2 gravity model, this does not invalidate the general conclusions summarized in the second paragraph of this note. Certainly, differences in geoid heights between the latest series of models remain large and there is room, as well as need, for considerable improvement in modelling the Earth's global gravity field. This need is generally recognized as in, for example, the recent report by the US National Research Council (Anon. 1985, p. 85) and by Smith & Tapley (1985). Lerch *et al.* in their present note comment only in passing on the high-degree coefficients in the gravity field, the part that we find most interesting. Therefore we take it that they agree with us on that aspect of our state of knowledge of the Earth's gravity field and that there is room for improvement (see also Wakker, Ambrosius & Aardoom 1983).

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