F.A. Dahlen: Does not the technique Prof. Melchior mentioned in his paper depend for its validity upon the complete absence of any oceanic loading effects?

P. Melchior: One evidently should correct all data for oceanic tidal loading. The difficulty comes from the lack of good co-tidal charts (both world and regional) for the diurnal components. However, these diurnal tides are in general small in the oceans near Europe, and do not seem to have a sensible effect.

M.L. Smith: The significance of Prof. Melchior's results is such that we should make sure we understand his data reduction methods. In particular, what steps did Prof. Melchior take to arrive at the determination of ψ_1 from the tiltmeter data?

P. Melchior: Each of three ten-year series was analyzed separately. We do not use the power spectrum analysis because we want to take advantage of the fact that the frequencies are known exactly to eight digits from astronomy. We use least-squares analysis to determine amplitudes and phases for these frequencies. No correction is applied to the readings of the curves.

P.L. Bender: Two sets of measurements from the U.S. support the conclusion of Prof. Melchior that the nearly-diurnal resonance is present. Prof. John Goodkind from the Univ. of Calif. at San Diego has obtained evidence for the expected resonance behavior at diurnal tidal frequencies by analyzing data from a superconducting gravimeter which he has developed. His results are being published in the Geophysical Journal of the Royal Astronomical Society. Dr. Judah Levine from the National Bureau of Standards has recently analyzed data from his laser strainmeter and finds that the results on the ratio of the K_1, P_1 and O_1 tidal amplitudes agrees well with theory if the nearly diurnal resonance is included. However, it differs by of the order of 5 standard deviations from theory if the resonance is not included.

P. Melchior: It is indeed also possible to use extensometers. However, their calibration is more difficult and in many cases is not satisfactory. There is, however, a way to escape the problem of calibration in this case. When one installs such an instrument out of the principal directions (NS, EW), there is in the strain component a sine term (deriving from $\partial^2 W/\partial\theta\partial\lambda$) which can be separated from the normal cosine term (deriving from $\partial^2 W/\partial\theta^2$ and $\partial^2 W/\partial\lambda^2$) if one has a good clock. By taking the ratio of these two terms one directly obtains the ratio ℓ/h, which is sensitive to the liquid core effect.

F.A. Dahlen: I agree completely with the main point made in Dr. Zharkov's paper, namely that physical dispersion is an important effect which must be taken into account. I do however have a minor criticism of the attempt to compare an observed value of δ (hopefully corrected for ocean loading) to the static value of δ calculated for models 1066 A and 1066 B. A more correct procedure would be to compare with the theoretical value of δ at the M_2 frequency. Only then can any discrepancy be attributed to dispersion.

P.L. Bender: Concerning the paper by Soler and Mueller, I think there are good reasons for believing that the motions of the lithospheric plates as given by Solomon et al. do not have a net rotation of more than about 1 cm/yr with respect to the bulk of the mantle. The solution they gave for the plate motions is consistent with the assumption that the "hot spots" in the mantle stay relatively fixed with respect to the rest of the mantle. Also, Kaula has shown that similar solutions within 1 cm/yr are obtained in the following cases: if the sources of material at the mid-ocean ridges and rises are assumed fixed with respect to the bulk of the mantle; if the deep subduction zones are assumed fixed; or if the average position of the plates containing large continental areas is assumed fixed. It is difficult to see how the sources of material for the "hot spots" and the mid-ocean ridges and rises, as well as the sinks of material at the subduction zones, can have a consistent 10 cm/yr motion with respect to the main part of the mantle material.

M.L. Smith: Sticking my neck out, I want to point out that the crust we wish to move about is of order a thousand times greater in horizontal extent than thickness and, further, is surely broken into disjoint, independently rotating pieces. It seems most unlikely to me that such a system would behave rigidly.