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FURTHER COMMENTS ON THE COMPARISON OF SURFACE GRAVITY DATA WITH SATELLITE DATA


We are quite aware of the type of problems and questions raised by Rapp and we gave due consideration to them. Although I do not agree with his points when applied to the specific solution in question, they have some validity in a general way and do deserve some attention.

Our final choice of solution as discussed in [2] and [3] depended on experiences gained in an elaborate sequence of tests and iterations. We tried many possibilities to search for an answer that best satisfied all tests. The combination of surface gravity data with the satellite inputs has formed an integral part of this iterative procedure and this is why it is the first time that such a global solution represents, within the observational data accuracies, both the earth's gravity field at the surface and the potential parameters describing the satellite motion at high altitudes. This can not be said for earlier combination solutions by, for example, Kaula [6], Kohnlein [7] and Rapp [8].

In the solution we seek two characteristics. We require the earth's gravity field at the surface for geophysical interpretations; and we require the potential at satellite heights for computing precise orbits which in turn are required for studying such phenomena as polar motion or earth tides. In principle they are of course compatible and there are no contradictions, but in practice, due to imperfect data sets, this is not always so. We therefore have to find a solution that best satisfies these two constraints so to speak.

Before replying to Rapp's specific points we have to make two further general comments.

We are talking here largely about the gravity set we used. That is the one compiled by Kaula [6]. It probably contains all data collected up to about 1963 or 1964. We have unfortunately no knowledge of the distribution or density of the data except for the number of observed 60 x 60 nm. squares in each 300 x 300 nm. square. Wheter each one such smaller square contains one observation or 500, we
do not know. This is unfortunate indeed, as we have no means of assessing the accuracy of the data. It further emphasizes the need for a new global compilation using all openly available data. Nevertheless, Kaula’s compilation was considered the best available at the time of our work mainly because of the statistical and homogeneous treatment of the data.

Secondly, in our discussion we consider the statistical behaviour of the combination solution. This behaviour is not necessarily the same as that of surface gravity data. For example, with the present methods of satellite geodesy, the accuracy of the \( C_{\ell m}, S_{\ell m} \) tends to decrease with increasing degree and order and some point we can no longer detect the effects of them. This is discussed in detail by Gaposchkin [9]. With surface gravity data we would expect a different, perhaps even an inverse, tendency and the combination solution’s results would be different again. Therefore, the comparison of the statistical behaviour of surface gravity data with that of our new solution is not always valid.

Response to Point 1

Rapp questions the value of the constant in our simple formulas for assessing the accuracy of the mean 300 nm. free air gravity anomalies. That is

\[
a_{\Delta g} = \frac{33}{(n + 1)^2} \text{ mgal}
\]

where \( n \) is the number of observed 60 x 60 nm. squares used to evaluate the mean value of the larger square. In comparison Kaula [6] used a constant of about 17 mgal, equal to the root mean square free air gravity anomaly of the 300 nm. blocks and based on the surface data. This value is probably too small because the linear regression methods used to predict the 60 x 60 nm. mean anomalies tend to give values that are too low but the difference is probably not very significant. The linear regression methods used are discussed in detail by Kaula [10].

A more serious objection is that the above simple rule does not reflect the accuracy of each smaller mean anomaly and does not reflect the distribution of the \( n \) squares within each larger block. Thus the estimates do not, in all probability, give a reliable estimate of relative accuracies of the different blocks. The consequence is that some of the well surveyed areas have received standard deviations that are too high, Our tests have shown that this is not important and that, in fact, it avoids another problem.

Of the 1654 possible 300 nm. squares only 935 contained some observations and only 136 contained more than 20 surveyed 60 x 60 nm. squares. A further breakdown gives, for \( n \gg 20 \), 38 squares in North America, 40 squares in Europe and 4 in Australia. I give these statistics to show the poor distribution of the data available up to about 1964. The complete data set is given in [5]. When one makes a global harmonic analysis with data that is not only poorly distributed but also of very variable accuracy there is always the risk of distorting the global
representation in areas where we do not have good data coverage or where we have
low accuracy data. This is particularly so as the better data contains shorter wave-
length information than we seek in the actual analysis.

We carried out some tests in which we gave a higher weight to the well
surveyed areas. In the combination solution we found, however, that this introduced
some anomalies into areas where there was little data. These anomalies were not
introduced by the satellite solution and are therefore spurious. This is hardly a
desirable feature but it is a general characteristic of harmonic analyses through
incomplete and inhomogeneous data. In other tests we compared the satellite solu-
tion against the surface data and found for several 300 km squares, differences
between the two that were greater than the formal statistics would lead us to
predict. This occurred even for some of the well surveyed areas and is probably due
to the higher wavelength information contained in these areas. Furthermore, we
are combining two quite different types of data and we require some type of relative
weights for the two. We have tried to find absolute weights, for, as Rapp rightly
points out, only the use of these will give reliable accuracy estimates for the end
results.

In searching for suitable weights we were guided by the results of the
comparisons with the geometric solution results, with surface gravity data, with
orbits, and with other independent data sets. These comparisons are fully described
in [3], [4], [5]. The geometric solution particularly played an important role. We
concluded that the covariance matrix of the satellite solution was generally
underestimated by a factor of about 4 or 5 and we scaled it accordingly. In each
iteration we used a factor found appropriate for that particular solution.

In combining the satellite results with the surface gravity data we used in
the building up of the normal equations, the standard deviation for each area mean
of

\[ \sigma_{A_g} = \frac{17}{(n + 1)^{\frac{1}{2}}} \text{mgal} \]

This is the value suggested by Kaula [6]. We found
however, that the orbits computed from the subsequent combination solution began
to deteriorate. By multiplying the covariance matrix of the surface data by a
factor we found a combination solution that yielded somewhat better orbits than
the satellite solution alone. At the same time, the gravity anomalies at the earth’s
surface and the comparisons with independent data (gravity data and with astro-
geoid data) changed very little; at most by a few milligals and quite insignificant
compared to the accuracies of the solution. The optimum factor found was usually
around 4. In each successive iteration we repeated these tests but the factor remai-
ned about the same. We have in this manner established for the combination
solution of each iteration the relative weighting of the two data sets. But we also
made comparisons for station coordinates (and the station coordinate determina-
tion is an integral part of a solution of this kind), with independent surface gravity data
and with astageodetic data. We found [3], [4], [5] a very good agreement between
the accuracy estimates obtained from these tests and the formal accuracies estimated
from the solution. Thus the formal accuracy estimates predict a global geoid
accuracy of about \( \pm 3 \) meters (see figure 8, page 4870, of [6]) whereas some
astro-geoid profile comparisons for four major datums yielded a mean accuracy of 3.2 m. (table 13, page 4880 of [3]). Some subsequent comparisons of the entire Australian Astrogeoid against this combination solution gave an accuracy estimate of 3.5 m. for the global solution. For a mean latitude of $-30^\circ$ this is almost identical to the estimate given by the formal statistics of the solution (figure 8, page 4870 of [3]).

Thus, we have not only established reliable relative weights but we have also found reliable accuracy estimates of the results. This implies, with the factor 4 discussed above, that $\sigma_{\Delta g} \approx 33 / (n + 1)^{1/2}$ mgal. I agree with Rapp that this value seems to be excessive, for the root mean square of all the 300 nm. surface gravity data in the order of 17 or 18 mgal. The difference, in view of the above discussion suggests one of several things: (i) The rms of the surface data is too small, due perhaps to the interpolation methods used, (ii) There are significant biases in the data, (iii) There is some remaining higher wavelength information in the data that we have not extracted, (iv) The simplistic variance estimates are inadequate. All factors probably play a role in increasing the variance factor and we will have to investigate them in more detail in the future. However, for the solution here, the adopted value is quite appropriate. Future analysis may require different rules but this would depend on the analysis of that particular solution.

The observation equation relating the surface data to the satellite data can be written in the form (Analogous to eqn, 16 page 5310 of Kaula [6] and to equations 2a and 2b page 206 of [2]).

$$g_T - e_T = g_s - e_s + \delta_g$$

where the meanings of the various terms are the same as in [2]. The quadratic sum to be minimized in the adjustment therefore is:

$$\sum_i \left( \frac{\bar{e}_{T_i}}{\sigma_{T_i}^2} \bar{e}_{T_i} \right) + \sum_i \left( \frac{1}{\sigma_{s_i}^2} \bar{e}_{s_i} \right) + \sum_{k,m} \left( \delta_{K_{km}} \tilde{K}_{km}^{-1} \tilde{g}_m \right)$$

For the first term we would use $\sigma^2 \approx 17^2 / (n + 1)$. For the second term we would use the equivalent expressions appropriate to the satellite solutions and the corresponding (modified) weights. In the last term the $K_{k,m}$ is the covariance matrix of the surface data and estimated by the linear regression technique [6]. An element of $K_{k,m}$ is $K_{\ell, x+1} (\psi_{k,m})$ for a maximum degree $\ell = \ell_x$ and calculated by equation (18) page 5310 of [6]. Because Kaula's predicted 300 nm. means (that is the one's used in our final analysis) were obtained from a linear regression of the residuals of $g_T$ with respect to a sixth degree field we must use $\ell_x = 6$. Then, neglecting the non-diagonal elements in $K_{k,m}$, the diagonal
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elements are of the order \([q^-^2] - [q^+^2]\) or, about 150 mgal\(^2\). This, together with the first term would give a total variance of something like 450 mgal\(^2\) for the surface data.

Response to Point 2

Rapp is probably correct when he suggests that the accuracy of the anomaly degree variances can be represented by a linear function if one talks about surface gravity data. The result I gave \(v^2 \approx 1.8 \times 10^{-3}\) for the combination solution and is an empirical function describing quite well the behaviour of the accuracy estimates of the anomaly degree variances of this solution. As mentioned above, the statistics of the combination solution do not necessarily behave the same as those of surface data.

Therefore it is not valid to extend this law very much beyond \(\ell = 16\). What I have wished to show in my comparisons (fig. 3, page 217 of [2]) is that with the satellite and surface data our disposal, the orders of magnitude of the accuracy estimates of the anomaly degree variances becomes of the same order as the anomaly degree variances themselves somewhere in the region \(\ell = 16\) to \(\ell = 18\). This is a conclusion of the results and of any empirical rule governing the behaviour of the degree variances.

During one of the iterations we did in fact extend the field up to \(\ell = 18\) but we found that these additional terms did not improve our orbits and did not significantly improve our comparisons with the independent surface gravity data nor with the astro-geoid data. The improvements in the comparison of this solution with the dependent surface data improved of course but as remarked in [2] such tests are of quite limited value. What these tests suggest is that the terms between \(\ell = 16\) and \(\ell = 18\) did not contribute much to the accuracy of the solution. Rather then carry them along for the sake of having an 18, 18 solution we dropped them. This test was admittedly carried out in an early iteration and we eliminated the station coordinate part of the solution, in order to fit the problem into the computer. But this should not change our general conclusion.

This test suggests that we have extracted most of the global information in the surface gravity data set that we used. However, by using some alternate functions to describe the potential we could extract more. Thus when I say that we have extracted “most of the global information” I should add, “that can be extracted by the type of analysis we used”.

Response to Point 3

The independent tests of the combination solution against surface gravity are shown in figure 3, pages 217 – 219 of [2]. In view of Rapp’s remarks some elaboration of the data used would appear to be in order. The surface data was taken from Talwani and Le Pichon’s compilation [11][12]. The first comparison is
for a profile in the North Atlantic running between two tracks of the Hr. Neth. Ms. Snellius. If one takes Strang van Hees' [13] 1° means for these two ship tracks and smoothes out some of the short wavelength features, one finds the agreement with the mean profile used in figure 3 of [2] and that taken from [11] to be better than 5 mgals. This is admittedly a somewhat rough procedure but is shows that the smoothing carried out in Talwani and Le Pichon's compilation does not invalidate the longer wavelength information for at least this part of the North Atlantic.

The second profile in the North Atlantic, the profile for the South Atlantic and the profile in the Indian Ocean along the equator are taken along ship tracks and are given in detail in [11] and [12]. Presumably these tracks have been tied to base stations in the respective ports so that some of the systematic errors have been eliminated. For the second Indian Ocean profile, that along 25° latitude, I have used the 1 x 1° means given in [12] and they appear to largely follow ship tracks. Thus all the comparisons are essentially for ship tracks and not for area means. Figure 3 in [2] is perhaps misleading in this respect as some of the profiles are labelled 5° x 5° means. This is an oversight but the texts in both [2] and [3] are quite explicit. With these comments in mind accuracy estimates of 5 mgals for the mean value over a five degree length of the profile - based on measured values - does not appear as excessive to me as it appears to Rapp. X. Le Pichon (private communication) confirms this. In fact some simple calculations would perhaps indicate that they are quite in order.

For the North Atlantic, for example, there could be, with three profiles passing through the latitude band between 28 and 34° about 15 1° x 1° blocks containing some surface data in a 300 x 300 nm. block. This is ignoring other data available within the region. Using the $17/(n+1)^{1/2}$ rule would give about 5 mgals for the estimated accuracy of this block, and is in agreement with the values given by Rapp based on Obenson's rule. All this goes to show that we should be careful about using empirical rules based on incomplete data sets. In view of my comments in [1] and [2] the estimate of 50 mgal$^2$ for the information not contained in the global field is of the right order of magnitude.

What we do with this value is perhaps an open question. I have simply assumed that it represents the amount of gravity information contained between degree 17 and some unknown maximum value. The result was $\mathcal{L}(\text{max}) = 25$, but it could well be 28 or 30. After all, with 5 mgal data we should be able to find some of the wave numbers that are higher than 25 even though their amplitudes are smaller than 5 mgal. This explains why higher wave numbers are found by Rapp in his point [2]. But I am not entirely convinced by this argument. It would be true only if there is global coverage and if there are no systematic errors in the data. It will be some time before these conditions are satisfied. That we were forced to increase our accuracy estimates indicates that these conditions were not satisfied completely. In any case, to argue whether we can extend the field up to 25 or up to 30 does not make very much sense to me. At 25 we have a half wavelength of about 750 km and at 30 we have 600 km. This is a considerable improvement on the
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1100 km that we have now but what appears to be required for geophysical purposes is information on even shorter wavelengths. These can only be obtained on a global scale and in the near future, by the use of satellite altimetry or by low perigee satellite to satellite tracking. The Geos C altimeter satellite to be launched in 1974 will contribute very much to this goal.

Summary

The suggestion that we have weighted the surface gravity data too low, is not valid. The weights used are different than one would expect from only analysing surface data but all tests indicate the correctness of our choice. This is sufficient justification for their use although we would like to understand better why this is. Our tests have indicated that there is little point in extending the present solution beyond about degree 16 or 18 whereas Rapp suggests that 24 — 28 may be a more reasonable goal. Either we are talking about different data sets or we have different concepts of what is significant.

Assuming the $5^\circ \times 5^\circ$ global coverage of 5 mgal accuracy Rapp suggests that the solution could be realistically carried out to degree 36. My estimates tend to be lower but this again depends on the definition of realistic or significant, and on the reliability of the data.

Throughout this work we have been concerned only in solving for those terms that either significantly affect the satellite orbit or that can be determined with some reliability from the surface data. Our basis for deciding what is or what is not significant formed the raison d'etre of the various tests made and discussed in [2] to [5]. We could have determined many more terms but they would not have improved the reliability of the overall representation.
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