St. John's College
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Dear Fisher,

Thank you for the papers. In the one on the Behrens-Fisher formula you seem to come very near the attitude I adopt myself. One of the others suggests to me that you may have something that would help me in my gravity problem, which is that of analysing the results of the worst-designed experiment ever produced. I have as far as possible reduced bias due to concentration of observations in particular parts of a 10° square, by taking only one observation for each 1° square (the first on whatever list I was using) and fitting a formula a + bh and using this to reduce to the mean height of the square. This should clear out the most prominent systematic error and leave standard errors with some prospect of being genuine, the results being representative of the 10° squares. The standard errors are of course very variable, ranging from 4 to 100 mgal (1 mgal = 0·001 cm/sec^2) while about 2/3 of the possible squares are totally unoccupied. The problem is to estimate the low spherical harmonics. It is plain on inspection that the variation even between adjacent squares is too big to be random - it is more obvious than any systematic difference between remote ones. Now there are two possible lines of attack. It might be possible to make a least squares solution to harmonics of degree 4, 20 of which are theoretically relevent, but I shrink from this and also doubt the validity, since if those of degree 4 are needed those of degree 5 are presumably present and would give a positive correlation between neighbouring deviations. On the other hand if we had data for all the 10° squares, evenly weighted, spherical harmonic analysis would be valid in any case and the standard errors of the coefficients would be correctly found from those of the 10° squares. The trouble here is that the unoccupied squares will presumably vary as much as the occupied ones : from the latter we can say something about the range of this variation, but nothing about just where the + and - deviations from a formula will be. I could get an estimate of the range by the method in 5·75(p.272) of my book(roughly it will be about 30 mgal) but still don't see what to do with it when I have got it. In a first shot (now abandoned for other reasons) I simply combined this with the standard error for each 10° square and did a harmonic analysis around each parallel by least squares, then comparing parallels to get the P_n^m coeffs. separately. But this seems to be overdoing it, because the s.e.'s for the occupied 10° squares are probably valid.

Thus I seem to be in a dilemma. If I try to fit the data by least squares as they stand the well-determined differences between neighbouring squares will give extrapolated departures from the simple ellipticity formula which will not be supported by the amounts of the total deviations from it within the squares used. If I try spherical harmonic analysis I don't know how to allow for the fact that though I shall have a general idea of the magnitudes of the deviations for the unoccupied squares I don't know them individually and have no way of allowing for this fact. Can you see any daylight ?

Harold Jeffreys.