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Reply to the comments of J. C. Dooley on "Analysis of a magnetic sphere using the Fourier transform: a new approach",

by N. L. Mohan, N. B. R. Prasad, S. V. Seshagiri Rao and V. L. S. Bhimasankaram: Bull. Aust. Soc. Explor. Geophys., 13(2), 35–40 (1982) — Reply to N. L. Mohan

We appreciate J. C. Dooley's keen interest in our paper.

1. We would like to make it clear that the Fourier transform of the vertical magnetic effect due to a sphere, V(x), exists but essentially it reduces to the Hankel transform and we mentioned in that sense "it defies". It has been explicitly dealt with by Sneddon (1974: 79–83). Also, Arfken (1970) stated as "The Hankel transform, a Fourier transform for a Bessel function expansion, represents a limiting case of a Fourier-Bessel series. It occurs in potential problems in cylindrical coordinates". In fact, Dooley himself stated in his discussion" . . . for a sphere the 1–D Hankel transform can replace the 2–D Fourier transform."

2. Dooley's statement " . . . the problem is essentially two dimensional and the two dimensional Fourier transform should be used.", though it has been elaborated in the later part of his comments, creates some confusion in the reader's mind.

Here we would like to mention that the magnetic sphere problem as such is a 3–D problem but in our paper the interpretation procedure has been developed for a principal magnetic profile (i.e., the profile runs through the centre of the contours usually perpendicular to the strike direction) and hence is reduced to a 2–D problem. Accordingly we have applied 1–D Fourier transform.

3. In Dooley's equation (3), which he states " . . . a legitimate Fourier transform of V, provided $Z \neq 0$", is associated with modified Bessel function or Hankel function (on the right side of the equation). He has not touched on the equation (3) for parametric evaluation for the obvious reason that it is an extremely difficult task and conveniently ignored to explain why the legitimate Fourier transform is not useful for parametric evaluation.

4. Dooley's later part of discussion (i.e., after eq (3)) is pertaining to another approach. Here we would like to make it explicitly clear that for the same reason that the legitimate Fourier transform does not help us in estimating parameters we adopted the procedure first by squaring the eq (1) (Mohan *et al.*, 1982) and then by converting into the Fourier domain.

5. Dooley's equations (5), (18) and (19) have been obtained only by adopting another transform from Magnus and Oberhettinger (1949, p. 118) and converting Dooley's eq (1) in cylindrical polar coordinates ($_\rho\theta$, z) respectively, obviously are different approaches and Dooley has not explained the computational problems.

It is true that Dooley's equations (5), (18) and (19) are simpler than derived by Mohan *et al.* (eq (6), 1982) but unless the accuracy of the parameters is compared it may not be possible to conclude that Dooley's method is superior to Mohan et al (1982).

Nevertheless, our experience says that the method adopted in our paper (Mohan et al, 1982) is substantially robust, leaving aside the general problems with the discrete Fourier transform.

References

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Sneddon, I. N., (1974). The Use of Integral Transforms, McGraw Hill Book Co. Ltd.

Comments on Reply by Mohan *et al.*

J. C. Dooley

I am glad to see that Mohan *et al.* now concede that there *is* a Fourier transform of the vertical magnetic effect of a sphere, despite their clear statements to the contrary in their original paper. However they still seem to be confused about the difference between Hankel transforms and Hankel functions, and the matter of dimensionality of the problem needs clearing up.

The purpose of the first part of my discussion was to correct a mathematically erroneous statement, not to provide an alternative means of deriving the parameters of the model. I showed that the 1–D Fourier transform of Mohan *et al.'s* V(x) was expressible in terms of Hankel *functions,* not a Hankel transform.

The physical problem of locating the anomalous body is of course three-dimensional, as stated by Mohan *et al.;* it could be regarded as a six-dimensional problem in parameter space, incorporating properties of the body such as direction and strength of magnetisation. My reference to two dimensions refers to the observed anomalous magnetic field, the data available to solve the problem. If only the field along the axis of symmetry is used in the interpretation, much of the available data is not used, and there is no guarantee that the spherical model is appropriate.

As stated by Mohan *et al.,* I have not attempted to compare computational methods in my Discussion. In general, discussions are intended to be limited in length; it would be interesting to compare computational efficiency of the various approaches, but this would warrant a full-scale paper. However the evaluation of Mohan *et al.'s* parameters using their equations (9) to (14), not only at one but at several frequencies, does not seem to be a particularly simple operation. There are computer programs available for evaluating Hankel functions, but I do not propose that one should use my equation (3) for interpretation as it would be subject to the same objections as Mohan *et al.'s* method, i.e. ignoring the 2–D nature of the data. In making comparisons of computational efficiency, the feasibility of obtaining the model parameters from the anomaly map by using such parameters as anomaly amplitude, half-width, distance between maximum and minimum, locations of zero crossings etc., should also be studied; this may provide a better approach than calculation of transforms.

Calculation of a two-dimensional transform, even using the fast Fourier transform, necessarily involves much more computation than for one dimension. In the latter part of my discussion, from equation (16) onwards, I showed that, along the axis of symmetry, the 2–D Fourier transform can be obtained simply from 1–D Hankel transforms; it is probably quicker to compute in this way. By itself, this axial function would still not address the problem of appropriateness of the spherical model, but calculation of the zero-order Hankel transform in the orthogonal direction would give at least a partial check on this.