# An estimate of the errors of the IGRF/DGRF fields 1945–2000

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The IGRF coefficients inevitably differ from the true values. Estimates are made of the their uncertainties by comparing IGRF and DGRF models with ones produced later. For simplicity, the uncertainties are summarized in terms of the corresponding root-mean-square vector uncertainty of the field at the Earth's surface; these rms uncertainties vary from a few hundred to a few nanotesla. (It is assumed that the IGRF is meant to model the long-wavelength long-period field of internal origin, with no attempt to separate the long-wavelength fields of core and crustal origin; the models are meant for users interested in the field near and outside the Earth's surface, not for core-field theoreticians.) So far we have rounded the main-field coefficients to 1 nT; this contributes an rms vector error of about 10 nT. If we do in fact get a succession of vector magnetic field satellites then we should reconsider this rounding level. Similarly, for future DGRF models we would probably be justified in extending the truncation from $n = 10$ to $n = 12$. On the other hand, the rounding of the secular variation coefficients to 0.1 nT could give a false impression of accuracy.

## 1. Introduction

The IGRF/DGRF is a series of numerical models which approximates the long-wavelength part of the main geomagnetic field $\boldsymbol{B}$ of internal origin as it varies with time. It uses a truncated spherical harmonic series to represent the corresponding scalar potential $V$:

$$\boldsymbol{B} = -\operatorname{grad} V$$

with
$$V(r, \theta, \phi) = \sum_{n=1}^{10} \sum_{m=0}^{n} (a/r)^{n+1} P_n^m(\cos\theta)$$
$$\cdot \left( g_n^m \cos\phi + h_n^m \sin\phi \right); \quad (1)$$

here $(r, \theta, \phi)$ are the usual geocentric spherical polar coordinates, $a$ is a reference radius (taken to be the mean radius of the Earth), the $P_n^m(\cos\theta)$ are Schmidt semi-normalized associated Legendre polynomials, and there are 120 numerical Gauss coefficients $g_n^m$ and $h_n^m$. (Strictly, it is the magnetic field $\boldsymbol{H}$, unit A/m, which in a source-free region can be represented as the gradient of a scalar potential. However, because all our measurements are in air and water, which have permeability $\mu$ very close to $\mu_0$, it is conventional to apply (1) to the magnetic flux density $\boldsymbol{B}$, unit T; in practice we give the $g_n^m$ and $h_n^m$ in units of nT.)

Because the geomagnetic field changes with time—the secular variation—a set of coefficients is specified every 5 years, with linear interpolation used for intermediate times. Because of this secular variation, to derive accurate coefficients we need global data coverage for all the relevant epochs. In the absence of such ideal data, the coefficients we estimate are inevitably in error, with the errors being larger at epochs for which the data coverage is poorer.

This paper discusses how well these numerical coefficients estimate the true values corresponding to the total field of internal origin. It is not concerned with the question of how much of this $n \leq 10$ internal field comes from crustal magnetization as opposed to electric currents in the core. Note that the uncertainties quoted by Langel *et al.* (1989) *did* include an estimate of the crustal contribution (Sabaka, personal communication, 1997); in effect they treated the IGRF as an estimate of the core field, which the present author thinks was wrong.

What we are really interested in is not the errors in the 120 individual coefficients, but the error $\delta\boldsymbol{B}$ in the corresponding magnetic field $\boldsymbol{B}$, where $\delta\boldsymbol{B}$ is given by the vector subtraction $\delta\boldsymbol{B} = \boldsymbol{B}_{\text{Earth}} - \boldsymbol{B}_{\text{IGRF}}$. We will never know $\delta\boldsymbol{B}$; the best we can hope for is to estimate its magnitude. For a scalar it is usual to express an uncertainty as the root-mean-square error (the standard deviation) to be expected if the analysis were repeated many times using data sets having independent random errors; this concept is easily extended to a vector.

However in our case we have the extra complication that $\delta\boldsymbol{B}(r, \theta, \phi)$ varies in space, as does its standard deviation. Here we will consider the variation only over the Earth's surface, and quote a single value which is a global root-mean-square vector error, where the mean is taken over $r = a$. From e.g. Lowes (1966) this is given by taking the square root of

$$\langle \delta\boldsymbol{B} \cdot \delta\boldsymbol{B} \rangle = \langle \delta X^2 + \delta Y^2 + \delta Z^2 \rangle$$
$$= \sum_{n,m} \left[ (\delta g_n^m)^2 + (\delta h_n^m)^2 \right], \quad (2)$$

where $\delta g_n^m$ and $\delta h_n^m$ are the errors in the $g_n^m$ and $h_n^m$, and $\langle x \rangle$ denotes the mean of $x$ taken over the sphere $r = a$. In this paper we will apply (2), using estimates of the standard deviations of the $g_n^m$ and $h_n^m$ to give the expected rms deviation of $\boldsymbol{B}$. But it is important to note that this is an average over

Table 1. Global root-mean-square vector differences between pairs of different models. The second and third columns give the rms differences between the original IGRF and the later DGRF. The fourth and fifth columns give the differences between the DGRF and the continuous spline model of Sabaka *et al.* (1997). The last column gives the values suggested by Lowes in the IGRF Health Warning (see text).

| $n \leq$ | (IGRF–DGRF) | | (DGRF–SABAKA) | | FJL |
|---|---|---|---|---|---|
| | 8 | 10 | 8 | 10 | 10 |
| 1945 | 274 | | 234 | 294 | 300 |
| 1950 | 269 | | 188 | 241 | 230 |
| 1955 | 251 | | 126 | 156 | 160 |
| 1960 | 122 | 145 | 99 | 117 | 100 |
| 1965 | 113 | | 61 | 62 | 50 |
| 1970 | 184 | | 51 | 56 | 50 |
| 1975 | 347 | | 64 | 67 | 50 |
| 1980 | 16 | 19 | 11 | 13 | 10 |
| 1985 | 35 | 36 | 30 | 31 | 50 |
| 1990 | 35 | 37 | 43 | 45 | 50 |
| 1995 | | | | | 100 |
| 2000 | | | | | 50 |

the surface; inevitably, in some regions the actual error will be less than the quoted figure, while in other regions it will be more, sometimes considerably more. Local errors, particularly in regions with poor data coverage, for example the South Pacific, may well be more than three times larger than the quoted rms value.

We will never know the actual errors of any set of coefficients; this would involve knowing the true field! The best we can do is to estimate some typical values. Some modellers do try to estimate the standard deviations of their coefficients, based on the lack of fit of their model to the data used to produce model. Inevitably this involves a variety of assumptions and interpretations, and in nearly all cases where the proposed standard deviations have been tested in retrospect their values have been found to be too small!

In the next two sections the comparisons between three different sets of models are used to estimate the magnitudes of the errors. The following two sections discuss the effect on the DGRF of rounding errors in the coefficients, and of the truncation, and Section 6 discusses the problem of the secular variation.

## 2. Differences Between IGRF and DGRF Models

In most cases the model for a particular epoch is derived in two stages. The production of an IGRF, for an epoch slightly in the future, inevitably involves some forward extrapolation of often inadequate data; we do the best we can, but accept that it may not be very good. Then at a later stage, when we think that we have obtained all the relevant data we are likely to get spanning the epoch, we produce a DGRF model; here the D stands for "definitive", which does not mean that it is perfect, but that we thought we were unlikely to do significantly better in the future.

So a simple indication of the sorts of errors there were in the IGRF models is to compare them with the corresponding (retrospective) DGRF models. This is what is done in columns 2 and 3 of Table 1. There is a complication in that

the earlier IGRFs were produced only up to $n = 8$, so the second column gives the comparison up to $n = 8$, and the third column gives the comparison up to $n = 10$ where this is possible.

The large difference for 1975 is because at that time we were feeling our way, and the 1965 IGRF plus secular variation was used for 10 years, rather than for the 5-year period used for all the other epochs. Ignoring that value, we see that the differences vary from about 300 nT rms at 1945 down to about 15 nT at 1980, when at long last we had real global vector data from the MAGSAT satellite; the IGRF 1980 modelling was done in 1981, so it included some MAGSAT data.

Of course not even the DGRF will be perfect; if the IGRF and DGRF errors were independent, then the differences given in the table would somewhat overestimate the error of the IGRF. However, as much of the data is common to both models, the errors of the models might well be correlated to some extent, so the figures are not necessarily over-estimates.

## 3. Comparison with a Cubic Spline Model

One problem, particularly for the IGRF models, but also to some extent for the DGRF models, is that they were mostly produced one at a time, so the sequence of models often shows marked discontinuities from one model to the next. A more recent model by Sabaka *et al.* (1997) is a continuous model, using cubic splines to specify the time variation from 1900 to 1995. The modellers constrained the time variation to be fairly smooth, so, for example, the presence of the MAGSAT data in 1980 will have a significant beneficial effect on the model coefficients for both earlier and later times. So the second comparison I have made is between the DGRF models and this continuous model. The resulting differences are given in the fourth and fifth columns of Table 1. The DGRF–SABAKA rms differences are of the order of half of the IGRF–DGRF differences, becoming relatively smaller for the later years when the DGRF models used MAGSAT data. (Note that DGRF 1990 is an average, in which a weight

of one third was given to the Sabaka *et al.* model.)

Of course this Sabaka *et al.* model has its defects. While the spline constraint allows the good data of 1980 to influence the model at other epochs, there is the possibility that poor data at one epoch will somewhat mar the model elsewhere. (And the covariance matrices for the 5-year epochs will not be independent.) Also the constraint of smoothness in time means that it cannot properly handle geomagnetic jerks. However the Sabaka *et al.* model is likely to be considerably better than most of the DGRF models. The figures of the fifth column of Table 1 are therefore taken as a reasonable estimate of the likely accuracy of the DGRF. This is the basis on which Lowes suggested in 2000 the rounded figures of the last column as working values in the "IGRF Health Warning", published on the IAGA web site at `http://www.ngdc.noaa.gov/IAGA/wg8/igrfhw.html`.

## 4. Effect of Rounding Errors on DGRF Models

When the IGRF was introduced in 1968 it was correctly thought that the accuracy available then did not justify quoting the Gauss coefficients to better than the nearest 1 nT. However this implies a rounding error of up to $\pm 0.5$ nT, which contributes a standard deviation of $1/\sqrt{12} = 0.3$ nT for each coefficient. For our $n \leq 10$ models we see from (2) that this contributes a global rms error of 9 nT. Until now this has been negligible in comparison with the uncertainties of the coefficients, except possibly for 1980 when we had MAGSAT data; at that time the NASA modellers claimed about 1 nT rms error for their $n \leq 10$ model, though the comparison of Table 1 suggests this was too optimistic.

But with the prospect of a succession of vector magnetometer satellites, such as Ørsted and Champ, we need to think again. Once we have good data, it is perhaps feasible that we can consistently achieve a global model error of a few nT, so for future retrospective DGRF models there is a strong argument that we should specify the coefficients to the nearest 0.1 nT, so as to reduce the rounding error to about 1 nT for a $n \leq 10$ model.

## 5. The Effect of Truncation

Another question that will arise when we have consistently good satellite data is whether the present truncation at $n = 10$ is still good enough. Table 2 gives rounded estimates of the contribution of the higher harmonics to the present main field. We see that omitting the $n = 11$ and 12 terms corresponds to omitting about 35 nT rms, so it would be worth including these terms if the resulting increased error of prediction is significantly less than this value. If in fact we can achieve a few nT vector error for an $n \leq 10$ model, then it is almost certain that this condition would be satisfied. There is therefore a strong case for extending future DGRF models to $n = 12$.

A referee, Dr. De Santis, has pointed out that for models obtained from a non-uniform data set by least squares, and using only a truncated set of coefficients, changing the truncation level could change the amount of higher-harmonic power aliassed into the lower coefficients. However, in the present context, which assumes good coverage of satellite vector data, the actual least-squares' solution is always taken to a higher degree, usually at least $n = 13$; the truncation to

Table 2. Global rms field produced by harmonics of degree $n$. (Rounded values based on the spectra of Cain *et al.* (1989), Langel and Estes (1982), and Langel *et al.* (1989).)

| $n$ | rms (nT) |
|-----|----------|
| 9   | 120      |
| 10  | 50       |
| 11  | 30       |
| 12  | 15       |

obtain an IGRF occurs only *after* the analysis, so no change of aliassing is involved.

## 6. The Problem of the Secular Variation

This is the perennial problem of geomagnetic modelling. It has been suggested above that the improved accuracy we can expect will justify changes in the specification of future DGRF models, i.e. in retrospective models for which there is by then good data spanning the relevant epoch. However for future, prospective, IGRF models, the situation is more complicated. The Working Group is now committed to producing such models ahead of time, so some forward extrapolation is necessary, and to estimate the accompanying secular variation will involve even more extrapolation.

The secular variation coefficients, which are applicable for up to 5 years, are at present given to 0.1 nT, so as to give consistency with the 1 nT rounding of the main-field coefficients. However, because we recognize the relatively larger uncertainties involved, we truncate the secular variation coefficients at $n = 8$, so the consistency is more apparent than real. In fact it is very doubtful if even the $n = 7$ and 8 coefficients are significant at some epochs. For example, Table 3 gives a comparison of the $n = 8$ coefficients of the three submissions for the IGRF 2000–2005 secular variation. (Note that for ease of presentation, the coefficient values have been multiplied by 10.) It is clear that for all the coefficients the scatter is far larger than 0.1 nT; in fact in the majority of cases even the average of the three values is not significantly different from zero!

But let us put this into context. By looking at past fields we can get some feel for the power spectrum of the secular variation, and typical values are given in Table 4. We see that the $n = 7$ and 8 terms between them contribute about 5 nT/yr to the secular variation. It is likely that by using these, mostly wrong, coefficients we are at present adding more error than we would by making all the coefficients zero, which is what truncating at $n = 6$ would imply. So for prospective secular variation models based essentially only on surface data (as the three models of Table 3 were) it would probably be more logical to truncate the models at $n = 6$.

From Table 4 we also see that making the $n = 9$ and 10 coefficients zero is ignoring a contribution of only about 1 nT/yr, so probably does not matter at present. It would matter only if we were able to very much improve the accuracy of the $n \leq 8$ coefficients.

We have no experience of using successive vector magnetometer satellites, or a long-lived satellite, to help in determining the secular variation. However it is reasonable

Table 3. Comparison of secular variation model coefficients. The third,
fourth, and fifth columns give the $n = 8$ coefficients of the candidate
models for IGRF 2000 SV proposed by Macmillan and Quinn (2000),
Langlais and Mandea (2000), and Golovkov *et al.* (2000), respectively.

| $n$ | $m$ | BGS | IPGP | IZM | |
|---|---|---|---|---|---|
| 8 | 0 | −4 | −4 | 0 | $10^{-1}$ nT/yr |
| 8 | 1 | 5 | −1 | 3 | |
| 8 | 1 | −2 | −6 | 12 | |
| 8 | 2 | −3 | −5 | −2 | |
| 8 | 2 | −2 | 1 | 1 | |
| 8 | 3 | 4 | 0 | 7 | |
| 8 | 3 | −5 | −2 | 6 | |
| 8 | 4 | −5 | −14 | −12 | |
| 8 | 4 | 6 | −1 | 5 | |
| 8 | 5 | 2 | 0 | 6 | |
| 8 | 5 | −2 | 1 | 9 | |
| 8 | 6 | −2 | −9 | −5 | |
| 8 | 6 | −7 | −9 | 3 | |
| 8 | 7 | −6 | −9 | −9 | |
| 8 | 7 | 1 | 6 | 3 | |
| 8 | 8 | 3 | −1 | 2 | |
| 8 | 8 | 4 | −4 | 2 | |

Table 4. Power spectrum of the secular variation. The table gives the total
global rms field produced by terms of degree $n$.

| $n$ | rms (nT/yr) |
|---|---|
| 5 | 11 |
| 6 | 9 |
| 7 | 4 |
| 8 | 2 |
| 9 | 1 |
| 10 | 0.3 |

to hope that such satellite data will reduce the errors of the
$n = 7$ and 8 coefficients, so as to make them significant in
future models.

But even if the satellite information greatly improves our
knowledge of the immediately past secular variation, there is
still the problem of extrapolation into the future, as the secular
variation itself changes with time at 2–3 nT/yr$^2$, the secular
acceleration. In the past some modellers have used a Tay-
lor series in time, incorporating acceleration terms; however
these terms are very poorly known, and the series often gives
a very poor forecast. More recently, modellers have tried
other numerical extrapolation techniques which are perhaps
less poor, but it seems likely that the basic problem will re-
main until our understanding of fluid processes in the Earth's
core allows us to apply some physical constraints to the ex-
trapolation.

So what about the prospective IGRF in the golden future
when we have plenty of data? Even if a retrospective model

can be determined to a few nT out to $n = 12$, an IGRF is
inevitably going to involve one to two years' forward extrap-
olation, which involves using a prospective secular variation
model. This will severely degrade the forward-looking IGRF
after a few years.

So although the present paper recommends increasing the
truncation level for (*retrospective*) DGRFs to $n = 12$, there is
a strong case that, at least for the time being, for the *prospec-
tive* IGRF we should keep to the present truncation level of
$n \leq 10$ (with coefficients rounded to 1 nT) for the main field,
and $n \leq 8$ (with rounding to 0.1 nT) for the secular varia-
tion. The hope is that by doing this we avoid giving less
well-informed users too great a confidence in the numbers
they are using.

As noted above, the explicit (prospective) IGRF secular
variation model will not usually be a good model of the
actual time variation then. Similarly, the implicit secular
variation involved in the linear interpolation between the
(retrospective) 5-year main-field models will not necessar-
ily be a good model of the real secular variation then. These
"secular variation" models are there solely to allow extrap-
olation/interpolation of the main-field model. Theoreticians
should at present look elsewhere for models of the true sec-
ular variation.

It is perhaps fitting to end this Section by quoting the fol-
lowing, written by Norman Peddie, and presented to the
equivalent of Working Group V-8 at the IAGA meeting at
Seattle in 1977:

Here lies new IGRF,
Had trouble in every nation.
He died the way his father died,
Of secular variation.

## 7. Conclusions

This paper has attempted to quantify the magnitude of the
errors in the present sequence of IGRF/DGRF models. The
results for the current eighth generation IGRF/DGRF models
are summarized by the last column of Table 4.

With reasonable luck, in the near future there should be
a succession of vector magnetic survey satellites. The au-
thor suggests therefore that the rounding level applied to the
DGRF main-field coefficients should be reduced from 1.0 to
0.1 nT, and that the truncation level for the main field should
be increased from $n = 10$ to $n = 12$.

On the other hand, for the prospective secular variation the
current 0.1 nT/yr rounding, and $n = 8$ truncation, is almost
certainly too optimistic at present. The situation should be
somewhat better in the future, though we do not yet have
proper experience of determining the secular variation from
satellite data. However, because of the difficulty of forward
extrapolation, the author suggests that it is unlikely that the
rounding and truncation levels should be improved beyond
their present values.

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