Author's comments in response to referees' comments and notes on manuscript “Correcting for Static Shift of Magnetotelluric Data with Airborne Electromagnetic Measurements: A Case Study from Rathlin Basin, Northern Ireland”, by R Delhaye, V Rath, et al.

Comments from the referees are italicised, and labelled with the corresponding Referee Comment or line number within the manuscript.

Response to comments from Referee 1:

RC1 - "*It is not clear to me to what extent the choice of the grid strike (cf line 269) would influence the modeling results, thus yourassessment might be "biased“ by the selection of the strike direction and thus it would be helpful and interesting to compare your results to those of another strike direction.*"
We are running such comparative inversions and will add figures of the same profiles and depth slices in an appendix or supplementary material to the paper.

RC1 - "*... the comparison of your results with those of Sternberg et al.. As the distribution of the static shift values strongly depends on very different "input“ variables such as reference data base for the near surface resistivity distribution, spatial distribution of the sites and geological situation, the comparison is rather user defined than yielding an objective causal relationship.*"
Both referees comment on the validity of comparison of our static shift corrections to those from a wholly different MT survey. We agree that precisely the same distribution of static shifts could not be expected dude to differences in geology and sampling. The inclusion was to verify that the static shift corrections obtained in our method were of realistic magnitudes; as both referees felt this comparison was invalid we shall remove the paragraph and associated figure.

Manuscript comments from Referee 1:
L63 - "*missing!*" - in reference to "Figure 2, alongside a plot of the borehole resistivity data."
Our sincerest apologies, an incorrect figure was included. The correct figure includes the resistivity data also shown in Figure 18, and is included at the end of this comment, labelled Figure 2, Lithology.

L264 - "*How was the mean calculated? Looking at Fig.4, many sites have phase differences <5° - where they included? This would bias the result, as the uncertainty of the strike should increase with decreasing phase difference. Furthermore, the strikes for the depth range 1410-2520 vary by 45 degrees between SW and NE sites - averaging conceals this observation. The description of the angles by one decimal seems to be exaggerated, and in view of the systematic variation of the strikes the arguments for deciding on the direction of 53.4°E are weak.*"
In order to compute a mean strike direction for the survey area, the strike analysis tool was first applied on a per-profile (i.e., across basin) basis, over depth bands from approx. 400 to 4000 m depth. The arithmetic mean of the resulting seven profile mean azimuths is the reported 53°. As the mean angle of 53 is within 10 degrees of the approximate strike angle of the Tow Valley Fault, this was not further examined. The referee's comments are of course correct in that considering strike angles for data with small phase differences leads to large bias, and that a large deflection in geoelectric strike azimuth is observed near the north-eastern coastline. The approach used does not take either effect into consideration.
We have also realised that Fig. 4 as presented may not be the most appropriate, due to the discrete depth intervals shown. An updated figure showing depth ranges from 300-550, 550-1000, 100-1780 and 1780-3000 m is proposed in order to show the full information. As with the erroneous Fig. 4, the deepest depth band (in this case from 1780-3000 m) shows a region of relatively consistent strike azimuths in the south-west, whereas azimuths in the north-east show a counter-clockwise rotation.
With the referee's observations in mind, taking an arithmetic mean of the strike estimates displayed in the south-west of the region (i.e., at sufficient distance so as to be less affected by the coastline) over the deepest depth band of the new Fig. 4 of 1780 - 3000 m gives a mean strike azimuth of 43 degrees, with a standard deviation of 10 degrees. The less biased regional strike azimuth of 43 degrees supports the rotation to 45 degrees of both the data and inversion meshes presented in the manuscript. The text, significant figures and Figure 4 will be updated to reflect this discussion. The updated Figure 4 is attached at the end of this comment, labelled Figure 4, Strike.

L299 - "I *don't understand the reasoning: If the diagonal elements are small, the relative error of the datum is naturally high and will decrease its influence. The higher errorfloor will downweight diagonal data with high significance (small relative error). Introducing different error floors generally favors certain components among those of similar significance (= similar relative errors) - what is the motivation to do that?*"

As we understand the comment, the referee has two concerns regarding the errors used: a) what is the motivation of applying an error floor, and b) why use different error floors for the diagonal and off-diagonal impedance elements. With regards to a), we believe that the experimental errors recorded for some of the data are understated, and using a modest error floor avoids potentially over-fitting of the data in the correponding frequency ranges. With regards to b), we concede that using a higher error floor on the diagonal impedances will bias the inversion towards fitting the off-diagonal impedances, however, this is primarily of concern when the diagonal impedances are of similar magnitude (and importance for structural resolution) as the corresponding off-diagonal elements. As evident in Figure 4, strike analysis shows that the majority of our data are two-dimensional in form (i.e., insignificant diagonal impedance elements - from inspection of the data typically an order of magnitude smaller than corresponding off-diagonals), with a limited amount of three-dimensional data. If a greater portion of data exhibited significant diagonal impedance components, any error floor used should be the same for both diagonal and off-diagonal components, for the reasons described by the referee. However, as the data inverted here are predominantly two-dimensional in form, and inverted in an approximately strike-aligned co-ordinate system, the structural bias introduced by a higher error floor on diagonal components is expected to be minimal.

L370 - "*How were the frequencies selected? The behaviour of delta Ex,y with frequency is an important feature for the assessment of the static shift, as it should be frequency independant, especially towards the high frequency end.*"

When comparing the observed MT data to the synthetic high frequency responses at the six frequencies (between 10000 and 1000 Hz), only points at which the two curves were parallel were considered. A comment clarifying this will be inserted in a revised manuscript.

L399 - "*The Kolmogorov-Smirnov Test assumes independent data. Fig. 7 shows a regionally smooth pattern which indicates spatial correlation and thus contradicts independency. How do you justify the test?*"

The intent of using the KS test was to provide some rigor in examination of the distribution of static shifts. As this is purely for analysis and not used further, we will reduce the comments to stating that the distribution appears close to log-normal, with a longer tail towards conductive static shift corrections.

L411 - "*If comparing distributions, their similarity does not imply causality - I find the topic too speculative for such a long paragraph and I suggest to skip Fig.10, as the estimation of the static shift values presumably strongly depend on the reference model which is not described for the US data; thus the assessment is rather vague.*"

Responded to previously. Paragraph to be removed.

L512 - "*In case of source field influence, the magnitudes are biased and their fit would lead to model bias which I regard as even worse as it will not be discovered as bias. Thus the phase misfit might present valuable information about the source field assumptions!*"

We thank the referee for an insightful comment. Given that the survey area is predominantly agricultural, with local source fields from electric fences, such bias cannot be ruled out. However, examination of the phase of input data do not reveal significant source field interference (as represented by phases that tend towards 0).

L552 - "*It would be interesting to compare the conductances of the conductive layers.*"

It would – taking a straightforward conductance (i.e., thickness/resistivity) gives the following conductances for Mc c.f. Mo (corrected c.f. uncorrected):
Lower Lias mudstones: 31 c.f. 13 S.
Dolerite sill: 34 c.f. 23 S.
Mercia Mudstone Group: 103 c.f. 100 S.

From this, the corrected model Mc has significantly increased conductances for both the Lower Lias and dolerite sills, whereas the MMG is recovered with a similar conductance in each model. If the section of borehole resistivity measured in the Lower Lias interval is representative of the entire layer, the integrated conductance would be significantly greater than the values computed above. Even if Mc does not fully attain such a high conductance, the closer-to-reality conductance adds confidence to the resolution of the underlying layers. We can include this comparison in an appendix or supplementary if appropriate.

Figure 7 - "*I have objections against the spatial interpolation technique resulting in a rather continous smeared pattern which might give a very misleading impression: E.g. the arrow points at the center of sites 15,16,23,24 - interpolation between #15 and #24 is equally justified compared to the present interpolation and would show a blue connection leaving #16 isolated - this would result in a completely different picture!*"

Figure 7 has been updated to have a slightly larger colour range. The interpolation used is a straightforward spline as implemented in the Generic Mapping Tools suite, rather than the nearest neighbour used previously. The spatial trends have not changed drastically, however, clarity has been added to areas such as the one noted by the referee. Specifically, considering the interpolation between sites 15, 16, 23 and 24, sites 15 and 24 (i.e., with resistive corrections) have multiplicative static shift corrections of 1.48 and 1.57. In contrast, sites 16 and 23 (i.e., with conductive corrections) have corrections of 0.38 and 0.51. Operating in the logarithmic domain, the resistive corrections are 0.17 and 0.2, compared to conductive corrections of -0.42 and -0.29. As a base test, the mean of the four corrections is -0.08, i.e., on the red side of the scale rather than 0 as could be suggested by the referee. We hope that the greater dynamic range of the colour scale clarifies the figure adequately. The updated figure is included at the end of this comment as Figure 7, Interpolation.

In response to comments and notes from Referee 2:

We appreciate the referee's comments regarding the overstating of our results and conclusions; we will adjust the language and phrasing of the article. In our understanding we did never suggest the use of AEM as a non-plus-ultra method for static shift correction, nor that AEM gives better results than the commonly used surface TEM approach.

The referee states that they do not agree that a "*robust, appreciable improvement in the fit to the borehole data*" is observed; we respectfully stand by our assertion that this is observed. Considering Figure 18, it is clear that neither model precisely reproduces the observed lithology. As MT is sensitive to variations in resistivity, and as the borehole resistivity log is incomplete, we cannot be certain of the existence of resistivity contrasts between lithologies. However, what borehole resistivity measurements exist show clearly that Mc has a significant improvement in resolution of the highly conductive (4 Ohm.m) Lower Lias, with a conductive layer at the correct depth range of similar low resistivity. In contrast, Mo manages to recover a significantly more resistive layer of 12 Ohm.m (i.e., almost an order of magnitude more resistive) at a depth 100m beyond that of Mc and the observed lithology. Given the significant shielding effect that such a conductive layer can have for the accurate modelling of deeper structure, an improvement in the accuracy of this layer's resistivity is highly significant – note also that this argument is addressed above in the discussion of integrated conductances at the borehole location. Additionally, as mentioned in the article, the gradient of the resistivity curve of Mc agrees much more with the gradient of the borehole resistivity. As it is well known that MT and borehole resistivities rarely agree in magnitude, the agreement of Mc and borehole resistivity gradients is a stronger result than Mo matching the magnitude but not the gradient of borehole resistivity.

Referee 2's minor comments:

1) "*EM cannot resolve permeability, as decades of experience by petroleum well log analysts and groundwater hydrologists has clearly shown.*"

We agree. The manuscript will be clarified to avoid any confusion.

2) "*The new part of the paper concerns the use of FDEM data, but the MT forward problem and inverse algorithm are described in depth while the FDEM forward and inverse algorithms are hardly described at all.*"

We will improve their descriptions.

3) "*The MT source is energized by atmospheric electricity in general, does not have to be a lightning strike, nor does it need to generate a Schumann resonance.*"

We agree & shall amend the description.

4) " *There are a large number of assertions in the paper that are not properly justified, rather they are simply conjectures*"

Regrettably without clarification from the referee we are unable to ascertain what other assertions he would like addressed. We will improve the discussion with respect to the main assumptions for the methodology used in this manuscript. The mentioned issue of comparison to the Cascades has already been addressed above (we will remove the comparison due to invalidity).

Fig 2, Lithology:

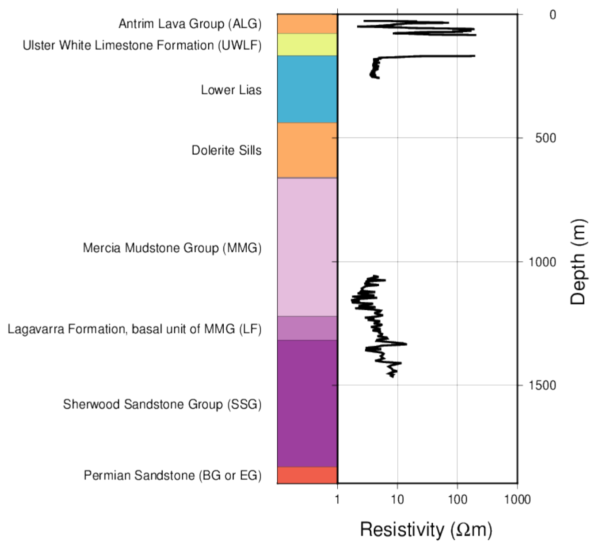
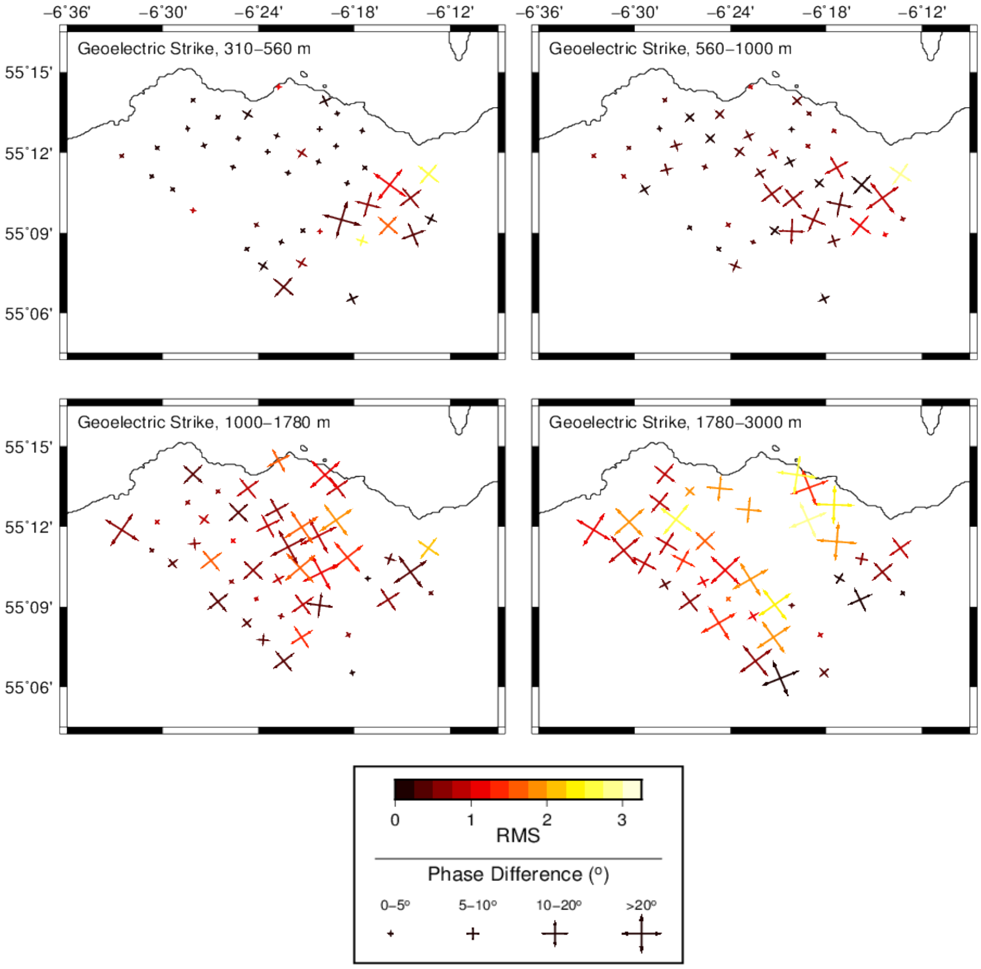


Fig 4, Strike



–6˚36'
–6˚30'
–6˚24'
–6˚18'
–6˚12'
55˚15'
55˚12'
55˚09'
55˚06'
Geoelectric Strike, 310–560 m
Geoelectric Strike, 560–1000 m
Geoelectric Strike, 1000–1780 m
Geoelectric Strike, 1780–3000 m
0
1
2
3
RMS
Phase Difference (º)
0–5º
5–10º
10–20º
>20º

Fig 7, Interpolation