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Comment on "Deep resistivity cross section of the intraplate Atlas Mountains (NW Africa): New evidence of anomalous mantle and related Quaternary volcanism"

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1. Introduction

[1] Tectonic interpretation of images and models of the subsurface derived from geophysical data must be based on a firm foundation of the following four precepts; (1) the data must be shown to be internally consistent, (2) the dimensionality assumed for modeling must be shown to be valid, and if 2D then the adopted strike direction must be shown to be consistent with the data, (3) the final model must fit the data to within their statistical limits, and (4) resolution of interpreted features must be demonstrated. Particularly in a highly nonlinear problem, such as electromagnetic geophysics, without examining model resolution it is impossible to know the veracity of the interpretation.

[2] In their paper, modeling magnetotelluric (MT) data across the Atlas Mountains, *Anahnah et al.* [2011] undertake a tectonic interpretation of a derived electrical resistivity model focusing particularly on a region of purported anomalously low resistivity in the uppermost mantle. However, the data and resulting model on which their interpretation is based are highly suspect, particularly at the longer periods pertaining to lithospheric mantle depths, meaning that the corresponding interpretation has not been proven by the extant data and cannot be trusted. We have little issue with the crustal part of *Anahnah et al.*'s [2011] model; we contend that the mantle part of their model is highly suspect and that the evidence for thinned lithosphere is simply nonexistent in their data, and indeed is refuted by their data, but appears to be contrived to fit preconceived notions. Our views are entirely based and stated on objective criteria, and suggestions otherwise are refuted and are attempts to detract from our criticisms.

[3] At issue is that the data used to construct the two-dimensional (2D) model presented by *Anahnah et al.* [2011, Figure 4] are questionable, the adopted strike direction is likely invalid, the model does not meet the dimensionality of the data, the model does not fit the data adequately, and the model was not subsequently appraised sufficiently for resolution. 2D modeling is appropriate at those periods sampling the crust, and is also reasonably appropriate for those periods sampling the mantle, but as the crust and mantle have different geoelectric strike directions, 2D modeling of the whole bandwidth of crustal and mantle periods is not appropriate.

[4] In their reply, *Anahnah et al.* [2012], who introduce new material not in the original, including a new model, and that took 6 months to compose, attempt to address some of the points raised in this comment but fail to address the primary one, that is, that the model misfit is completely unacceptable, mainly due to the model responses not fitting the data at all at long periods. Hence their model, and consequent interpretation, of the lithospheric mantle is highly suspect at best and likely erroneous. Therefore, its tectonic interpretation cannot be believed and should not be accepted.

2. Data Acquired

[5] The data acquired by *Anahnah et al.* [2011] are exemplified in their paper by MT response plots (apparent resistivity and phase curves) of only four of the 19 sites acquired of useful data (of the 21 total sites, sites 08 and 10 are listed as "no available" (sic) in the phase tensor plot in *Anahnah et al.*'s [2011] Figure 3) in *Anahnah et al.*'s [2011] Figure 4. Those selected four, we have to presume, are representative. Unfortunately, error information is not shown for any of them, nor on the phase tensor plot, so we must judge their quality from data scatter and curve shape, taking into account that the processing code of *Varentsov* [2002, 2006], used on the long-period data, employs a regularized approach such that estimates at neighboring frequencies are not independent and the MT curves may appear artificially smooth.

[6] Whereas at high frequencies the data look to be of reasonable quality, notwithstanding the smoothing inherent in the data processing code used, data quality clearly

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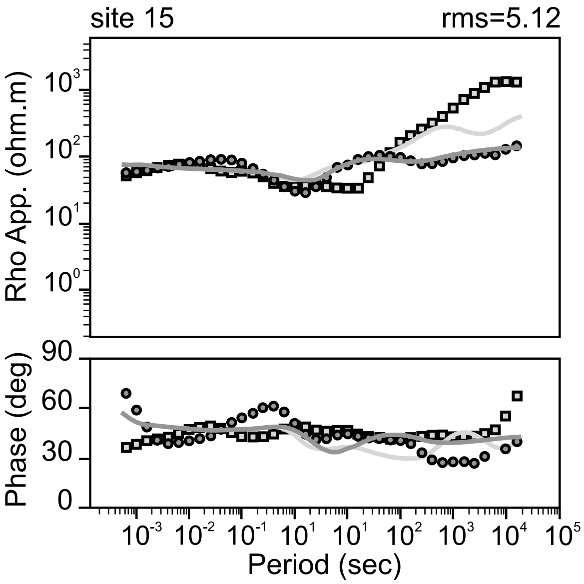

Figure 1. Reproduction of data and model responses from Site 15 of *Anahnah et al.* [2011]. Dots are the data and solid lines are the model responses. Dark gray is for the TM mode, and light gray is for the TE mode.

degrades with increasing period. This may be a direct consequence of low natural electromagnetic signal at long periods as the MT data were acquired in the interval May–July 2009 during the depths of the extended sunspot minimum between Cycle 23 and Cycle 24, with the IPS monthly smoothed sunspot number being 2.3, 2.7 and 3.4 for those 3 months, respectively. This problem of low signal-to-noise during sunspot minimum is especially true at periods beyond 1,000 s. Our own measurements on the Atlas Mountains had been originally planned for the spring of 2009, but we postponed them because of that very low solar activity until autumn of 2009, when the sunspot number increased significantly to 7.0 and 7.5 for October and November, respectively.

[7] Another likely contributing factor to the poor quality of the long-period data is the short duration of recording of only 15 days for the long-period data. More usual in long-period MT practice is to record for a month at a site to ensure that one acquires the full 28 day midlatitude solar rotation where the majority of sunspots are located. This is especially important during times of low signal.

3. Data Analysis

[8] As a precursor to modeling, MT data are routinely analyzed for problems using a variety of approaches. For these data, the apparent resistivities and phases at one of the key sites, Site 15 (scanned, digitized and reproduced in Figure 1 here), are not internally consistent, i.e., the apparent resistivities and phases do not obey the dispersion relations, i.e., they are not Hilbert transform pairs, as is formally required in one dimension [*Parker and Booker*, 1996] and in two dimensions in the TM mode [*Weidelt and Kaikkonen*, 1994], and is usually the case in two dimensions for the TE mode [*Fischer and Schnegg*, 1993; *Parker*, 2010] and for the off-diagonal terms of the MT impedance tensor in three dimensions [*Yee and Paulson*, 1988, 1990]. This internal consistency can be checked with the freely available Rho+ algorithm of *Parker and Booker* [1996], and examples of its use are given, e.g., by *Jones and Garcia* [2003], *Spratt et al.* [2005] and *Patro and Sarma* [2009]. The Rho+ prediction of the phases from the apparent resistivities, and of the apparent resistivities from the phases, for the data from Site 15 are shown in Figure 2, and clearly, there is a major incompatibility in both modes at periods above 1 s. This incompatibility is also apparent in the model fits to the data shown in *Anahnah et al.*'s [2011] Figure 4; the model phase response (solid lines) does not come even close to the derived observed phase response at the long periods for all four example sites shown.

[9] In their Reply, *Anahnah et al.* [2012] refer to numerical experiments published by *Berdichevsky and Pokhotelov* [1997] as an example of violation of the dispersion relations. That somewhat obscure paper, discussed by *Berdichevsky* [1999] and also *Berdichevsky and Dmitriev* [2008, pp. 37–44], describes violation in the case of a 2D regional structure perturbed by 3D surficial features, the so-called 3D/2D galvanic distortion case well studied for 30 years (for a pertinent discussion of the history of MT distortion analysis methods, see *Jones*, 2012]). While it is correct that severe 3D distortion of 2D regional data can result in dispersion relations incompatibility between the apparent resistivity and phase curves, *Anahnah et al.* [2011] described how they determined the appropriate regional 2D strike angle (discussed further below) using Bahr's phase sensitive strike approach [*Bahr*, 1988] and so supposedly accounted for 3D distortion effects on 2D data by aligning with regional 2D strike. If this were the case, then the only

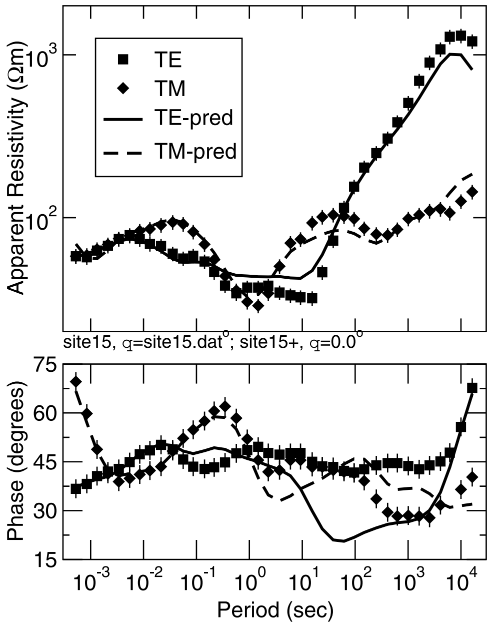

Figure 2. Prediction of phases from apparent resistivities, and apparent resistivities from phases, from the data at Site 15 using *Parker and Booker*'s [1996] Rho+ algorithm. The symbols are the data (digitized from Figure 1), and the lines are the Rho + predictions. (Note different scales from Figure 1 to highlight variations.)

remaining galvanic distortion effects would be static shifts [*Jones*, 1988] of the MT apparent resistivity curves. The shapes of the apparent resistivity curves would be correct and hence compatible with the phase curves. Thus, there should not be any extreme violation due to residual galvanic distortion as exhibited by their data at Site 15 for regional structures that are 2D. Unless of course the regional structure is not 2D and/or *Anahnah et al.* [2011] have chosen the incorrect geoelectric strike angle and still have significant galvanic distortion effects in their data.

[10] However, as discussed further by *Anahnah et al.* [2012], *Anahnah et al.* [2011] consider that the long-period real induction vectors (in Parkinson convention, i.e., pointing toward anomalous current concentration) are demonstrative of regional 3D structure, confirming our querying of the validity of their 2D modeling over the whole period range covering the crust and lithospheric upper mantle. On this point we agree with *Anahnah et al.* [2012] as we find in our data (see below) that a single, period-independent, geoelectric strike is inappropriate, as the crust and lithospheric mantle display different 2D or quasi-2D strike directions.

[11] In any case, the misfit at long periods between the model and the data, as shown at the four selected sites (RMS misfits of 3.47, 5.95, 5.12 and 4.25) representative of the whole data set (overall RMS misfit of 5.3 for error floors of 10% in apparent resistivity, 5% in phase and 0.08 in geomagnetic tipper function), is indicative of dispersion relation violation at most, if not all, locations, not at a single one that could be summarily dismissed as due to residual 3D/2D effects.

[12] In the case of regional 3D structures, there has not been a systematic modeling study of dispersion relations of the responses, but no one has yet reported severe violation of the dispersion relations of numerical responses of any 3D geometry to the extent demonstrated in the data from Site 15 and the other selected sites.

4. Geoelectric Strike

[13] *Anahnah et al.* [2011] conducted a 2D modeling exercise, using a well-known inversion algorithm [*Rodi and Mackie*, 2001], of MT data that clearly display 3D effects. This 3D nature is visibly evident in the phase tensor plots of *Anahnah et al.*'s [2011] Figure 3, the strikes of the ellipses rotate with increasing period (proxy for depth), and in the induction vector plot of *Anahnah et al.* [2012] (unfortunately plotted without error). Also, comparing the strike directions at one period (1000 s in this case) from sites over a large region is fraught with complications and pitfalls due to the range of penetration depths possible given the vast range of electrical conductivity of Earth materials [see, e.g., *Hamilton et al.*, 2006; *Jones*, 2006; *Miensopust et al.*, 2011]. A period of 1000 s may penetrate deep within the lithospheric mantle at some sites, whereas at others, due to the presence of crustal conductors, may not penetrate beyond crustal depths. A simple skin depth rule of thumb indicates that at 1000 s the data at Sites 3 and 17 are penetrating approximately 85 km (resistivity of 30 Ω m) and at Site 15 to greater than 200 km (resistivity of >100 Ω m), whereas at Site 9, penetration is to only 27 km (resistivity of 3 Ω m). Thus the 1000 s strike directions at some sites refer to the crust, at others to the lithospheric mantle, and at others to deep within the upper mantle.

[14] Determining the geoelectric strike direction for interpretation in two dimensions is a key component of data analysis prior to modeling. There are a number of approaches that have been proposed to address this problem over the last 50+ years, and many of them are discussed and reviewed recently by *Jones* [2012]. Jones shows just how sensitive most mathematical methods, even the well-established ones such as Bahr's approach and phase tensor, are to noise and levels of distortion and the superiority of fitting a physically defined distortion decomposition model to the data. The multisite, multifrequency distortion decomposition code of *McNeice and Jones* [2001], based on the approach of *Groom and Bailey* [1989], is made freely available for academic purposes, and indeed, the two MT experts in the *Anahnah et al.* [2011] paper, J. Pous and W. Heise, have had the McNeice-Jones code since 2002.

[15] The more sophisticated depth-based geoelectric strike analyses of TopoMed MT profiling data along the same profile by *Kiyan et al.* [2010a] (known to and cited by the authors) and *Kiyan et al.* [2010b], using an advanced form of the distortion decomposition code of *McNeice and Jones* [2001] (demonstrated by *Miensopust et al.* [2011]) to deal with varying penetration depths along the profile, showed that the crust and mantle beneath the High and Middle Atlas mountains have different 2D, or quasi-2D, geoelectrical strike directions, with a crustal strike of N50°E consistent with surface tectonic features, as found in the analyses of *Ledo et al.* [2011] (also cited by the authors) for crustal-penetrating periods, and a mantle strike of N20°E. In such a situation of varying crustal and mantle geoelectric strike directions, 2D modeling over the whole period range for crustal and mantle structures is invalid. *Anahnah et al.* [2011] adopt a single strike direction for both the crust and mantle of N80°E that is 30° different from our own crustal value of N50°E and 60° different from our mantle value of N20°E. This incorrect angle for 2D modeling will result in at best poor and at worse inaccurate resolution of crustal and mantle features. Indeed, it is likely that the authors are interpreting the wrong MT modes in the mantle by assigning N80°E as the strike direction, i.e., the TE mode data are being interpreted as TM mode and vice versa, whereas our data suggest N20°E. Either the data should be modeled in fully three dimensions, or the crustal structures modeled in two dimensions and then the mantle structures modeled in two dimensions in a different strike angle, after removing the crustal effects as a "distortion" on the mantle responses.

[16] It is puzzling that to justify their choice of geoelectric strike, *Anahnah et al.* [2012] state that "the main geological features are in general ENE-WSW (N80°E) trending, particularly in the High Atlas, the region of greatest interest." The dominant tectonic feature along the profile in terms of its electrical response is the Middle Atlas, not the High Atlas. The Middle Atlas is the location of the middle and deep strong crustal conductor beneath their Sites 10–12, and the Middle Atlas has a tectonic strike of NE-SW, consistent with our own adopted crustal geoelectric strike of N50°E. The only conductive feature of the High Atlas is the putative "conductive lithospheric mantle anomaly" (*Anahnah et al.*'s [2011] Figure 5c), interpreted from their model (*Anahnah*

et al.'s [2011] Figure 4), that is in question, even in their own data (see below).

5. Modeling

[17] Notwithstanding these differences in definition of appropriate strike angle, the model presented by *Anahnah et al.* [2011, Figure 4] does not fit their data, either in a global (whole data set) sense or in a local (site specific) one. The global misfit level, given by the root-mean-square (RMS) misfit, is quoted *Anahnah et al.* [2011, paragraph 11] as "The model obtained has an RMS of 5.3 applying an error floor of 10% for apparent resistivities, 5% for the phases and 0.08 for the tipper." This means that on average the apparent resistivities are fit to 53%, i.e., to a factor of 2, and phases to 7.5°, at best. The residuals of the misfit are, however, serially correlated in that the model responses fit the data well at short periods and very poorly at long periods. Some data likely (we have to say "likely" as no errors are shown on any data parameters) have error bars greater than those assigned error floors, so the actual misfits are even larger. An error of 50% for apparent resistivity is sometimes adopted when modeling MT data to deal with statics shifts [*Jones*, 1988], but in this case, *Anahnah et al.* [2011] used the facility within the 2D inversion code to find the shift factors, so the apparent resistivity data should have been fit to far lower levels of misfit.

[18] In detail, especially where it counts, the forward responses from the model do not fit the observed data, or at least as best as the reader can assess from the four data and fit plots shown. In almost all MT papers there are plots of the observed and model predicted data from all sites [e.g., *Muñoz et al.*, 2008, Figure 4; *Pous et al.*, 2011, Figure 4], often in the form of pseudosections [e.g., *Solon et al.*, 2005], Figure 5; *Spratt et al.*, 2005, Figure 8; *Rao et al.*, 2007, Figure 2; *Ledo et al.*, 2011, Figure 3; *Evans et al.*, 2011, Figure 4] or of misfit sections [e.g., *Solon et al.*, 2005, Figure 7], so the reader can assess for himself/herself whether the features in the model represent features in the data.

[19] In the original paper the reader is shown the fit of the model to the data at only four sites, which are selected because they are presumably representative. The misfits at these sites demonstrate well the problems with the model. Particularly, Site 15 (reproduced here as Figure 1), which lies above the purported anomalously conducting zone in the mantle discussed in detail by the authors, and also Site 17 are indicative of the misfit problem. The site RMSEs are reported as 5.12 and 4.25 for Sites 15 and 17, respectively, but the misfits are clearly serially correlated, indicative of a poor fit statistically of the model to the data. The data are fit reasonably well at short periods, to within scatter for the most part (apart from the unbelievable high TM phase at the lowest three periods), but are very poorly fit at periods greater than around 10 s, which are the mantle-probing periods. For Site 15, although the TM apparent resistivities are fit reasonably well, the long-period TM phases are not fit at all, particularly the strong phase minimum between 100 s and 10,000 s, which is a phase signature indicative of entering a resistive region at depth, is totally missed by the model. Skin depth arguments would place this resistive feature at some 60–80 km or so, exactly the location of the upper mantle conducting feature in their model.

[20] In addition, neither the long-period TE apparent resistivities nor the TE phases are fit, especially the very strongly rising apparent resistivity at periods >1000 s and the odd, and suspect, rapidly rising phases for the longest three data points. Neither of the two apparent resistivity curves show a dropping long-period response that would be indicative of the existence of a moderate conductor within the mantle. If the data at Site 15 are questionable in their internal consistency and are furthermore not being fit by the model, then obviously the model is an inaccurate representation of the data, and one of the primary interpretive features, of thinned lithosphere indicated by the moderately conducting lithospheric mantle beneath the High Atlas, is not proven and is highly questionable.

[21] The presence of thinned lithosphere beneath their sites on the High Atlas, Sites 12 to 16, should be obvious in their data. The phase tensor plots in *Anahnah et al.*'s [2011] Figure 3 show low phases (blue) below 45° at long periods for the geometric mean of the maximum and minimum phase tensor phases. When transitioning from more resistive strata above (in this case, lower crust and uppermost mantle) to less resistive strata below (in this case, the putative "conductive lithospheric mantle anomaly" of *Anahnah et al.*'s [2011] Figure 5c) phase should increase above 45°, i.e., be yellow to red, which is not observed in their data. Contrarily, their phase tensor data are indicative of transitioning to a region of higher resistivity with increasing depth, not lower resistivity.

[22] *Anahnah et al.* [2012] in their Reply include a new figure of induction vectors. If an upper mantle conductor existed between their sites 10 to 16, as shown in *Anahnah et al.*'s [2011] Figures 4 and 5c, then the induction vectors on resistive mantle at the neighboring sites, Sites 5–9 to the north and Sites 17–21 to the south, would be pointing toward this anomalous region, i.e., the induction vectors at Sites 5–9 would point to the south, and at Sites 17–21 to the north, and this behavior they clearly do not exhibit. This is further evidence in the authors' own data that the mantle part of their model, particularly the "conductive lithospheric mantle anomaly" of *Anahnah et al.*'s [2011] Figure 5c, is inconsistent with their data.

[23] Finally, in their re-revised Reply, *Anahnah et al.* [2012] present a new model that is substantially different in many features from that presented in the original paper [*Anahnah et al.*, 2011]. Primarily, the new model appears to be smoother than the older one. Direct comparison is hindered by the different color scales used, but first-order differences are glaringly apparent. Gone is the extension to depth of the Anti-Atlas resistive mantle. New is the extension to depth of the Prerif resistive mantle. Reduced significantly in lateral and vertical extent is the deep crustal/upper mantle conductor beneath the region between the Middle Atlas and the High Atlas. Gone are the thick conducting top layer sequences beneath the Prerif and the Middle Atlas. Now the mantle beneath the Middle Atlas appears to be of lower resistivity than in the prior model. Even with all of these alarming changes, the total RMS has only been reduced from 5.3 (old model) to 4.06 (new model), and 4.06 is still far too high and is indicative of data features that are not being fit. Hardly surprising given the inconsistencies between the apparent resistivity curves and the phase curves. Inspection of the data and model response pseudo-sections,

which should be done with care as they are smoothed, shows area in data space that are significantly misfit. No site misfits are given for this new model. In summary, this new model is a marginally better fit than the prior one, but still suffers from all of the criticisms raised above.

6. Model Appraisal

[24] Finding a model that fits the data is the first, and by far the easiest, step in the whole modeling/inversion process. There are an infinite number of models that can fit the data, so subsequent to model finding, there must be an exhaustive effort to understand resolution of model features. In the case of MT data modeled by the authors, this should first be undertaken by varying the inversion parameters in the Tikhonov regularized inversion code of *Rodi and Mackie* [2001] used by the authors, but none of which are discussed in the paper. At a basic level, the optimum (in some sense) regularization trade-off parameter tau, that trades off misfit against roughness, is usually determined in an exploratory inversion phase using an L curve approach [*Hansen*, 1992] plotting roughness against misfit. Recent examples in published MT results are, e.g., those of *Booker et al.* [2004, Figure 2a], *Spratt et al.* [2009, Figure 6], *Patro and Sarma* [2009, Figure 7], *Matsuno et al.* [2010, Figure 5], and *Pous et al.* [2011 Figure 9].

[25] In addition to this single parameter, the code of *Rodi and Mackie* [2001] allows the user a lot of control on the inversion, including which sort of regularization Laplacian to use (either a standard grid Laplacian or a uniform grid Laplacian), regularization order, the horizontal to vertical smoothing (regulated through two parameters, alpha and beta; the optimum values of these depends on the type of regularization Laplacian adopted. Unfortunately, the default settings for some of these two parameters are incorrect in the WinGLink package.), whether to invert for the best model or the model closest to the initial model, and finally, the initial model itself. Choices of all of these explore model space in different ways and dramatically influence the models found; *Matsuno et al.* [2010] recently showed a range of models obtained by varying a range of the inversion parameters.

[26] Having though tested for optimum inversion parameters and having found a favored model, or preferably a range of favored models, then it (or they) must be appraised for resolution. There are linearized appraisal tools available, such as that of *Schwalenberg et al.* [2002], but electromagnetic problems are notoriously nonlinear, so hypothesis-testing approaches must be employed, as done by *Ledo et al.* [2004] and *Solon et al.* [2005].

[27] None of these model space exploration or model resolution tests are undertaken by *Anahnah et al.* [2011]. None of these issues are discussed or explained by *Anahnah et al.* [2012].

7. Conclusions

[28] The tectonic interpretation construct of *Anahnah et al.* [2011] lies on top of a very shaky base, namely, the poor electrical resistivity model that cannot be trusted in its mantle part. Notwithstanding the excellence of their tectonic interpretation, any geological interpretation of geophysical data must lie on the firm foundations of solid geophysical data acquisition, analysis, modeling and inversion. All four of these are shown to be suspect in this comment.

[29] The data are poor at long periods, the data were not properly analyzed prior to modeling, a single crustal and mantle strike direction is inappropriate, the strike direction adopted was inappropriate (evident in their own phase tensor data and induction vector data, and also in our own analyses), the rotated impedances were not properly modeled, model space was insufficiently explored, and the model was not appraised. The reader is thus totally unable to have any confidence in the interpretation presented, particularly of the mantle features and especially of the existence of a "conducting lithospheric mantle anomaly" [*Anahnah et al.*, 2011, Figures 2a and 5c] inferred to indicate thinned lithosphere. The indications are that some data features are not consistent with some parts of the interpretation, but the reader cannot make any objective assessment.

[30] Crustal features of *Anahnah et al.*'s [2011] model are likely to be generally correct, however, and their model is virtually identical to the prior crustal model published by *Ledo et al.* [2011], with the difference being that the data of *Anahnah et al.* [2011] are modeled at an angle of N80°E, whereas crustal strike is N50°E. This means that the structures are more "fuzzy" and their geometries are less well resolved in *Anahnah et al.*'s [2011] model compared to that of *Ledo et al.* [2011]. We are pleased that *Anahnah et al.* [2012] accept this point and agree with this remark of ours. We are somewhat perplexed though that *Anahnah et al.* [2012] continually make comparisons between their model and the crustal model of *Ledo et al.* [2011]. We repeat that we have little issue with the crustal part of *Anahnah et al.*'s [2011] model; it is a fuzzy version of our own. We contend that the mantle part of their model is highly suspect and that the evidence for thinned lithosphere is simply nonexistent in their data and indeed is refuted by their data but appears to be contrived to fit preconceived notions. The new model presented in their Reply [*Anahnah et al.*, 2012] also has reduced resistivities in the mantle beneath the High Atlas, although, as the authors themselves state, it is "not very low". Indeed, the resistivity is of the order of some hundred ohm.meters, which is not indicative of typical asthenospheric resistivity of 5–25 ohm.meters.

[31] Given that seismic velocity varies over a few percent, seismic data are usually very forgiving of incorrect modeling. Not so for modeling electromagnetic data; the parameter that MT data are sensitive to, namely, electrical conductivity, varies over many orders of magnitude. Poor modeling of MT data will result in inaccurate, imprecise and/or unreliable models leading to inaccurate, imprecise and/or unreliable tectonic interpretation, and that is the case with this paper for the mantle structures. Nothing written by *Anahnah et al.* [2012] at all addresses any of our main concerns. As we state, the crustal structures in *Anahnah et al.*'s [2011] model are likely correct but are fuzzier to our own [*Ledo et al.*, 2011] due to the fewer number of sites recorded by Anahnah et al. (approximately half) and to the strike direction being incorrect (N80°E instead of N50°E).

[32] Finally, we are very pleased at the public offer by *Anahnah et al.* [2012] of combining the two data sets, and we are more than willing to agree, although we recognize that their long-period data are problematic, being poor in quality and internally inconsistent, and will not add to our

own database in terms of imaging the resistivity structure of the upper mantle beneath the Atlas Mountains. This data exchange is something we have been requesting of the Granada-Barcelona group from the outset of our TopoMed MT experiment, most recently in early January this year well prior to submission of *Anahnah et al.* [2012].

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