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Solid Earth
Discussions

# *Interactive comment on* "A re-evaluation of the Italian historical geomagnetic catalogue: implications for paleomagnetic dating at active Italian volcanoes" *by* F. D'Ajello Caracciolo et al.

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In this paper, the authors propose a way to update the Paleo Secular Variation Curve (PSVC) at Stromboli, South Italy, for the past four centuries. Their curve is derived from indirect and direct inclination and declination measurements. Obtaining a local PSVC is of general interest for geomagnetic field reconstruction during historical times, although one may wonder if global models are not better suited for recent epochs covering the XIXth and Xxth centuries. I think that the chosen methodology proposed in this paper is interesting but the authours should provide more details. I have the feeling that the conclusions are currently not sufficiently supported by the data. I expect this would not



be the case after some clarifications are made. I have three major comments that I try to explain below. Minor issues could be dealt with in a future version.

1) My first and major concern is about the Westward drift correction that is presented as the novelty of this paper. I found this discussion not convincing and I suspect this is a problem of presentation. I am not familiar with the way Paleomagneticians estimate the Westward drift. However, from what I understood from Merrill et al. (1996, Chapter 4) and their Fig. 4.1, the Westward drift is estimated thanks to data distributed world-wide along all longitudes. The westward drift is therefore a characteristic attributed mostly to the dipole field. To me, it does not make much sense to estimate the value of the westward drift from from data covering such a restricted range of longitudes (between 9° and 14°) because the estimation may be very uncertain. The non-dipole field dominates the estimated values. In such a case, I read in Chapter 4, first §of Merrill et al., (1996) that there is no consensus so far (at least up to year 1996, maybe things have changed; if this is so, please add some references) that the non-dipole features should drift westward... The drift estimates you obtain in page 27, line 13, indeed seem to support the idea that a westward drift over Italy cannot be ascertained. In some cases, the drift is indeed found to be westward (positive slope values, about 21 values in Fig.5), but some others values suggest a eastward drift instead (negative slope values, about 3 values in Fig. 5), and one result could let us think that the field is standing (nearly zero slope, 1 value in Fig. 5). Since zero cannot be excluded, the correction is stricly speaking statistically insignificant, I think. My reasoning may be wrong however because I did not understand how the value 0.46 +/-0.19°/yr (page 27, line19) was obtained. Maybe your methodology may be better described. Note that you could have also simply used Fig 4.2 of Merrill et al., (1996) showing the westward drift as a function of latitude. You would have got directly something like 0.41 °/yr.
2) Later on, you argue that the offset observed at Castellaccio is reduced after westward drift correction. I far as I could see, you put this example forward to assess your method. I do not think this is sufficient. It appears very puzzling that the westward drift correction affects only this dataset and the declination, not the inclination. I think

you could better discuss this (and recall, for instance, the reader that Castellaccio are observatory data). Castellaccio data indeed are observatory data (see page 23) and as such are (should be) more accurate than indirect measurements. I suspect, if your assumption is correct, that you see some benefits in correcting the data because the signal to noise ratio is sufficiently high. Is it correct if I state things this way ? Castellaccio show an offset in the declination because 1) They have a sufficiently good quality 2) They are far enough in longitude from Stromboli, the center of your analysis, so that the westward drift error is significant enoughÂă; then this better explains why Pola data (also observatory data), which are almost at the same longitude as Stromboli, have no offset (it would be interesting to reduce all data to the longitude of Castellaccio and see if Pola then shows an offset ? This could be a possible demonstration). For inclination, it is important to stress that no data exist at these observatories (and not let the reader search in the supplementary data to come to this conclusion). This would make clear that the inclinations are reconstructed from adjacent areas. This, in turn, could explain why both data have nearly the same inclination offset (unless this offset can be explained by their comparatively higher latitude that Stromboli?). My point is that the interpreation is maybe non-unique. Moreover,in other places, the data uncertainty seems larger than that the relocation error. Therefore, the advantage of correcting the data is marginal. This also raises a questionÂăthat is eluded in your work. The IGRF models are available since 1900 and allows computing the magnetic field up to SH degree 13, at least to 8 for earlier epochs (see Finaly et al., 2010 Geophys. J. Int. for the 11th generationÂă; http://www.ngdc.noaa.gov/IAGA/vmod/igrf11coeffs.txt). Why not trusting these global models for that time period and relying instead on an approximative relocation technique? Are these global models very different from your PSV Curve and other models ?

3) All these questions arise because the error bars are not displayed. This also makes it difficult to be convinced by your discussion about the agreements and mismatches between the PSV Curve and the global or regional models. I assume it is difficult to produce a significant error bar for the local PSV curve (since one component, inclination



or declination, is sometimes missing), although you do produce ellipse of confidence (how exactly?), but at least the regional and global models should be displayed with their errors bars. To me, it seems that all models more or less fall within the same errors. This information, currently missing in you work, is essential if the curve is to be used as a dating tool (first §of the introduction).

From these three comments, I am bound to believe that the westward drift correction gives rise to an interesting discussion but is not very useful in practice considering the indirect measurement errors. From the beginning of the XX century, the direct measurements are available worldwide so that IGRF models and the likes (Jackson et al., 2000) do in general a good job, particularly in Europe where observatory measurements are the densest. For earlier epochs, the data coverage is too poor to derive accurate global models and your approach could be favored. Unfortunately, the data quality being equally poor, applying your correction seems to bring little improvements except, as you say, at some times during the XIXth century (what you say page 20 line 23-25 of your abstract). I agree that scientists should encourage data reduction as correct as possible. In this respect, the westward drift correction should certainly be applied. My main reluctance is that the westward drift is apparently difficult to quantify with accuracy and one may wonder what would be the adverse effect of a badly estimated correction. Could it introduce some bias ? (by the way, you estimated the westward drift with +/- 0.19°/yr... what is the effect of this error bar on your final result?). All this should be better described, presented, and argued in your paper, I think.

Other specific comments

4)I recommend revising the syntax. 5)Page 22: To me, any model, in particular the model of Pavon-Carrasco et al. (2009), is a kind of relocation technique. The PM2009 is not exactly a Pole Relocation Method but it is close to it when considering very low SCH expansion (up to SH=2 according to PM2009). 6)I do not fully understand why the relocation was performed at Stromboli... If one wants to avoid relocation errors and geographical bias as much as possible, why not relocating the data in regions where the dataset is the densestÂă? 7)In general, it is not always clear when the data or the polynomials are plotted in the Figures. I did not understand either why you needed to compute a polynomial. Don't you have enough data in the surrounding areas to fill the data gaps? 8)The polynomials is not sufficiently well described. I have the feeling that 1790 is the central epoch set in the regression (but I am not sure, just a guess from page 26 line 6). In Page 26 line 4 you say that you solve a non-linear least-squares... how is it so ? A polynomial fitting is a linear inverse problem even though it is ill-conditioned (why not favoring a spline representation by the way?). Also, if the central epoch is 1790, then the function becomes something like: cn(t-1790)ˆn, for t=1690 this reaches $\sim$ 3.8e19 and for 2010 $\sim$ 1.2e20, respectively... I have the feeling that this may be computationally unstable (and lead to coefficient estimates as small as 1e-20). Please, could you better describe what you do and if you normalize something? 9)In Figures 3 and 7, why the ellipses of confidence are not more supplied for epochs after 1940 ? 10)Page 25: As a general comment, using the magnetic anomaly map is not a strong argument to discard local anomalies. The spatial resolution, according to Chiappini et al., 2000 is about 10x10km (page 984: 100km$^2$)... which gives room for strong local anomalies. 11) I do not think we can see geomagnetic jerks with a time step of 10 years in Figures 3 and 7 ?

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