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Reply to the referees' comments

First of all we would like to thank referees Monika Korte and E. Thebault for providing thorough reviews of our paper. Nearly all changes they have proposed were introduced in the revised manuscript. Here we have copied referees' comments, and added our replies (in bold) to each point raised by them.

<u>Rev#1 (Korte)</u>

Questions and comments to the authors Major points: 1) The re-location to the Stromboli location and the consideration of the westward drift are the most important new aspects in this paper. I found it rather difficult in some parts of the manuscript to understand exactly how the data had been treated and what implications that might have. In my opinion it would be important to include the equations about the re-location using the pole method for declination and inclination data, and also the equations how the westward drift then was taken into account.

We have now detailed in the text the equations used for declination/inclination relocation (from Noel and Batt, 1990), as well as the equations used to account for the westward drift of the geomagnetic field.

2) From the text I understand that the values in tables 1 and 2 and displayed in Figs. 3 and 7 are values from a smoothed polynomial curve. That fact should be mentioned in the captions. I think it could also be useful to include the polynomials with the confidence intervals in Fig. 6, in order to evaluate the effect of the smoothing polynomial.

OK, we have now specified in the captions of Figs. 3 and 7 how the smoothed curve was obtained. We have also tried to add the smoothed curve and the relative confidence intervals in Figs. 2 and 6, but verified that in such way the two figures are hardly readable (many different data are superimposed). Consequently, we have decided to leave Figs. 2 and 6 as unmodified.

3) Geomagnetic jerks indicated in Figs. 3 and 7 and related discussion in the text: It seems to me that two different phenomena have been mixed up here. Note that there is a significant difference between geomagnetic jerks and archeomagnetic jerks. In the plotted curve you might expect to find archeomagnetic jerks showing up as cusps, as these are significant changes in the field directions. Geomagnetic jerks, however, are defined as significant changes in secular variation, i.e. the first derivative of the field components. You would not expect to see cusps at the times of geomagnetic jerks in the shown figures, but you would have to plot the derivatives of the field components.

The discussion has to be modified accordingly. See also M. Mandea and N. Olsen (2009): Geomagnetic and archeomagnetic jerks: where do we stand? EOS, 90 (24), 208.
We are aware of the difference in the definition of Geomagnetic jerks and archeomagnetic jerks, as correctly reported by the referee in her comment. We also understand that with the data set given in this paper it would be difficult to find any geomagnetic jerk in particular. We now just mention that in Fig 6 the famous 1901 geomagnetic jerk could be visible as a change in the slope of declination data series. This would correspond to a V shaped jump in the derivative of the field, as required from the jerk usual definition. The term 'cusp' used in the paper, that is more correctly associate to an archeomagnetic jerk, was wrong. We have modified the text accordingly.

4) Westward drift correction: the westward drift correction factor has been calculated from data only from 1805 onwards. Indeed it seems to me that when using pre-1805 data, one would obtain an eastward instead of westward drift with that method, or would the considerations of Fig. 4 also give negative values in that time period? Why should it be justified to apply the westward drift correction to the whole timeseries?
In fact before 1825 (there was a mistake for the 1805 year) few data are available, and there are not years yielding declination values from multiple sites, with the exception of year 1640. Therefore the westward drift cannot be properly evaluated for the time span pre-1825.

5) p.4, l. 19/20: It is right that in principle the modeling method of Pavon-Carrasco can take advantage of the whole data base. However, the mentioned publication states that the curve of data relocated to Viterbo has been used. The sentence should be modified to avoid misunderstanding.
OK, we have modified this sentence accordingly. Moreover, referee's comment let us notice that the model of Pavon-Carrasco et al. (2009) considers 1600-1790 data from Italy that seem in fact to be derived from the model of Jackson et al. (1990) (see text).

Minor details and some suggestions for improvement of English usage: I suggest using "via the pole method" instead of "via pole method" (several occurrences throughout the manuscript) **OK, done.** p. 6, l. 4: "from sites" instead of "by sites" **OK**. p. 6, l. 4: "e.g. north of Rome" doesn't really make sense. Do you mean "i.e. north of Rome", or perhaps "mostly north of Rome"? **Yes, we mean "i.e. north of Rome".** p. 6, l. 10: replace "null corrections" by "no corrections" **OK.** p. 7, l. 6: what's the purpose of the two dashes around 100 nT? **That was a mistake. We meant "less than |100| nT".** p. 8, l. 12: "from further consideration" instead of "by" **OK** p. 9, l. 7: perhaps "have concentrated on" instead of "have contended on" **OK.** p. 10, l. 14: Either give arguments why the data are very valuable, or remove the rest of this sentence from **Ok, we have now deleted the sentence from "but the few measurements…" onward.** p. 10, l. 16: correct spelling of "Furthermore" **OK.** p. 10, l. 26: correct spelling of "greater" **OK.** p. 23: Fig. 6 seems to include more data points for inclination prior to 1650 than Fig. 2, although the caption and text say that it's the same data. **Yes, it's indeed the same data, but they come from the 66° inclination measured in Rome by Kircher in 1640 (see pag. 6, lines 12-14), so that they are all virtually superimposed in Fig. 2.**

<u>Rev#2 (Thebault)</u>

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 worldwide 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…
Secular variation of the Earth's magnetic field is a global phenomenon but undoubtedly shows some regional peculiarities. Westward drift in particular is an observable feature with evident regional peculiarities. It is quite manifest in the Atlantic Ocean and in the Mediterranean Sea, while it is almost absent in the Pacific Ocean. So we (as many other authors report) think that westward drift cannot be attributed mostly to the dipole field. Probably is mostly due to non dipolar Earth's core contributions. So the value reported in the paper (0.46±0.19°/yr) is derived specifically and for the Mediterranean region only, by using the available data and simply averaging and reporting the result. Sorry if this was not clear in the submitted manuscript (we have now modified the text in the westward drift paragraph).

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.
Data of fig. 5 were treated statistically. As explained in the text, the average and standard deviation of fig. 5 data is 0.22±0.12, which proves the significance of the westward drift value. In fact, the computing of westward drift at single years is subject to statistical variations. Some errors can be included in single points. In our statistical computation the most important parameter is the mean of westward drift and its statistical error.
In our data we have s=0.12 so the 95% confidence interval is given by [m-s,m+s] where m is the computed mean. In our case we get:
[0.10,0.34]
That represents the interval where the "true" mean of the westward drift is at 95% of probability. So values under 0.10 (and all negative values) have a probability of less than 5%.

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. **OK, following also the suggestion of Rev#1, we have now better explained in the text how we got the 0.46°±0.19°/yr value**
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. **See reply above.**
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 enoughÂ˘ a; 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). **We concur with Rev#2 that the declination shift is mostly evident for Castellaccio (having a ca. 6° longitude difference with respect to Stromboli) than for Pola, located at almost the same longitude than Stromboli. This is clear from Fig. 2, where the two declination time series are apparent. Yet we think that this is a clear proof for the westward drift effect. Thus we fully agree with explanation 2). Conversely, we retain explanation 1) as unlikely. In fact, accuracy of declination measurements from Italy in the XX century can be safely considered well below 1° (Cafarella et al., 1992), while the persistent declination shift at Castellaccio reaches 1.6° (Fig. 2). Thus we would exclude that the persistent declination shift of Castellaccio is a matter of measurement accuracy.**

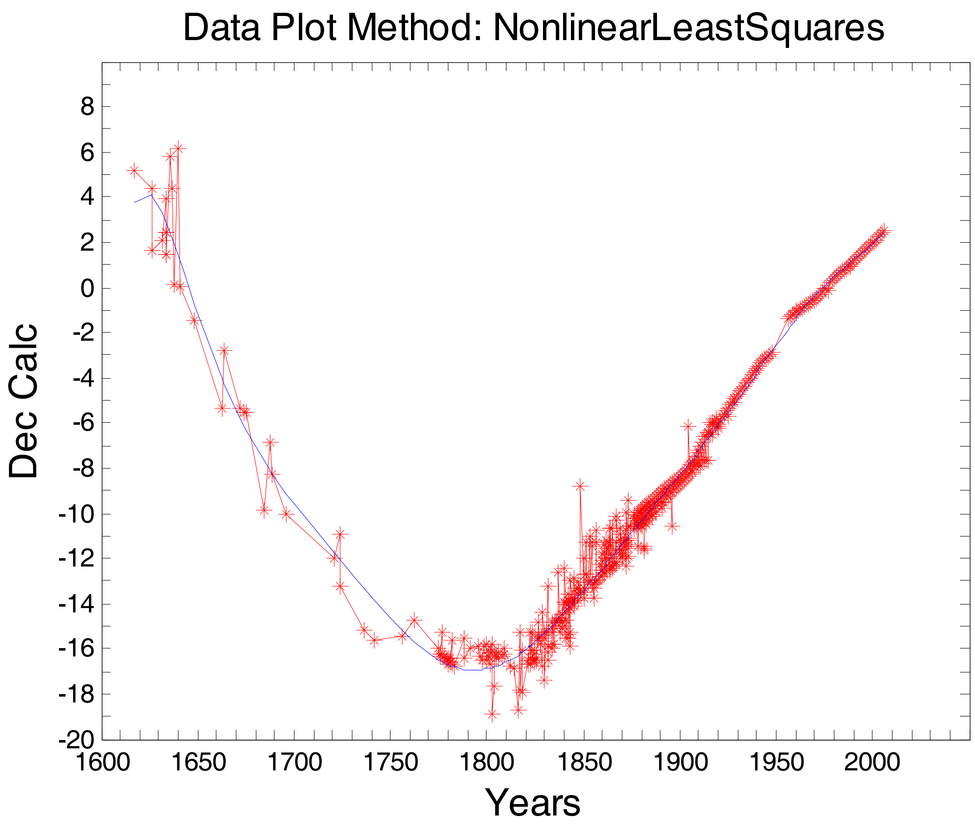
For inclination, it is important to stress that no data exist at these observatories (and not let the reader search in the supplementary data to come to this conclusion). **Well, in the chapter "The Italian historical geomagnetic data set" it is stated that "Some declination-only time series come from the geomagnetic observatories of Pola (formerly Austro-Hungarian Empire, 1881-1922) and Castellaccio (1933-1962)."**
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 questionÂ˘ athat 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 generationÂ˘ a; 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 ? **For the period 1900 onwards Earth's magnetic field global models, like IGRF and Jackson's, are a very good tool for the reconstruction of magnetic data worldwide. However in this paper our contribution has brought the attention on different PSV curve, for different reasons. The first reason is that PSV curve shows continuity deepened in real data with magnetic information that dates back to the 17th century. Another reason is that global models, at their best, can represent regional magnetic values with the spatial resolution allowed by SCHA wavelength. At the highest available degree, IGRF, for n=13 for example, can give the minimum detail (applying the Bullard rule at the equator) that can be roughly computed at 3000 km wavelength. And this is a very large value if applied to the Mediterranean area. Moreover we know that only few of the available data, coming from the Mediterranean area, were used for the global models preparation. In conclusion we then preferred in this paper to use PSV curves as reported in the text.**
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

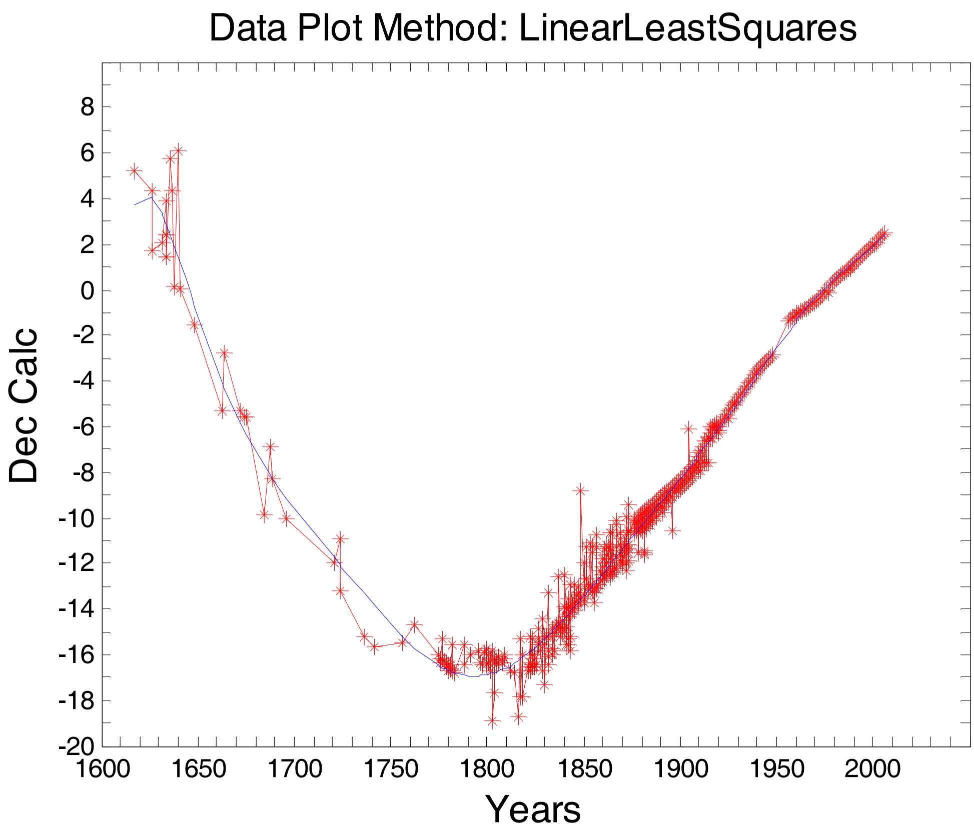
errors. This information, currently missing in you work, is essential if the curve is to be used as a dating tool (first §of the introduction). **OK. We have now investigated this issue. Unfortunately, the Jackson's model does not provide error bars. Error bars derived from the Pavon-Carrasco model are now displayed in Fig. 3. This shows that the accuracy of our SV curve is significantly greater for the XVI-XVII centuries, while the two curves yield comparable accuracies for the XIX century. We think that the greater accuracy of the SV curve we obtained for Italy may justify the analysis presented in our paper.**
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 ? **Yes indeed, do not considering the westward drift effect would introduce a bias, and this is clear from the Castellaccio data.** (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). **OK, we have revised the syntax accordingly**.
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 densest? **The reason for the choice of Stromboli as a common relocation site is explained in the text:…"to the common site of Stromboli (38.8°N, 15.2°E), selected as the rough centre of the active volcanoes of southern Italy (i.e. from Vesuvius to Pantelleria) where paleomagnetic dating has been used so far. This choice would allow the SV curve derived by us to be compared with few or null corrections to paleomagnetic data gathered from active Italian volcanoes."**
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 illconditioned (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)^n, for t=1690 this reaches 3.8e19 and for 2010 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?
The data plotted in Figures are the measured / relocated data, while we aimed at having an estimate of inclination and declination also for years lacking measured data. This is the reason why we needed to interpolate. Our polynomial interpolation does not have a central point. We

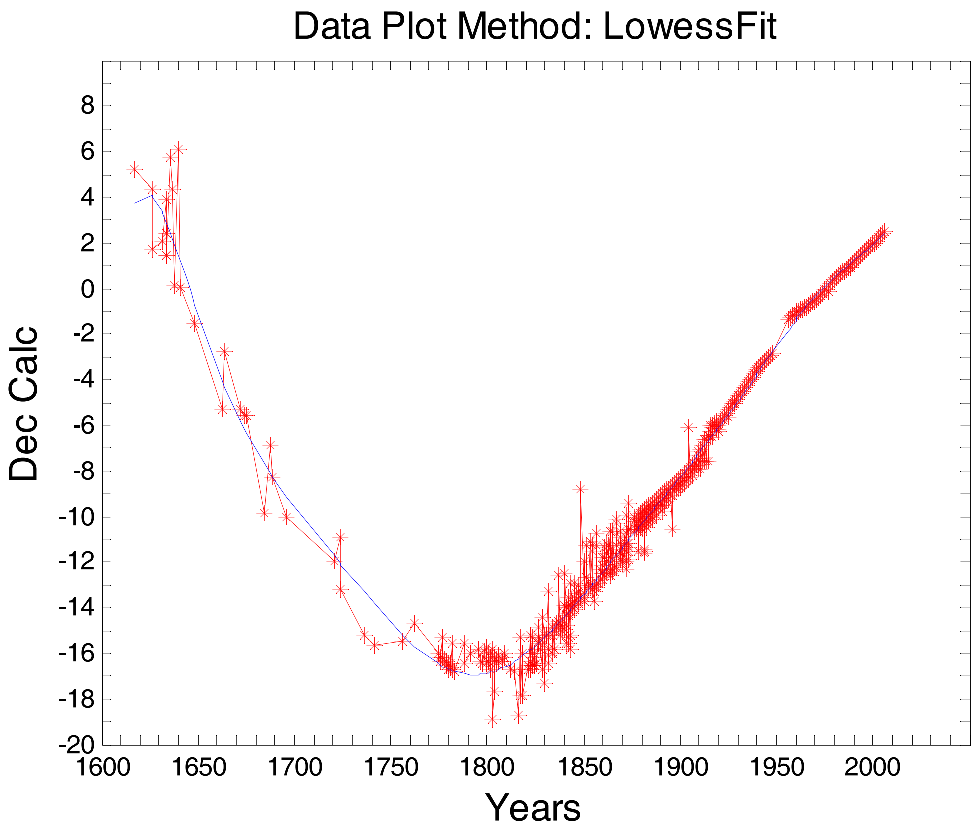
just found the 9-th degree polynomial curve regression coefficients. We used such polynomial curve:

$$y = ax^9 + bx^8 + cx^7 + dx^6 + fx^5 + gx^4 + hx^3 + ix^2 + lx + m$$

Where x is the year to compute the value of inclination or declination.
In order to find the coefficients we used a matlab library that can use many minimization methods. All the methods showed identical results: we saw that the choice of minimization method did not affect the polynomial coefficient values. Here we show, as example, the results obtained with three different methods that we do not include in the paper. The non linear minimization method is based on the Levenberg and Marquardt technique (Press et al., 1992).







We used the polynomial regression because we needed not only the polynomial coefficients but also their 90% confidence interval degree in order to compute the ellipse errors on data. We developed all our data interpolation in matlab and, using the interpolation library, we found that is not possible to compute the 90% confidence interval for spline method and, in

general for all the "interpolant" methods. So we decided to interpolate data with a 9-th degree polynomial, high enough to interpolate well all the data and to compute the errors.
9)In Figures 3 and 7, why the ellipses of confidence are not more supplied for epochs after 1940
Because since 1933 we have the regular time series from Castellaccio (then from L'Aquila since 1960), thus there is no need to perform statistics and calculate confidence ellipses.
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: 100km2)... which gives room for strong local anomalies. **This is true, we concur with comment of Rev#2. However, Zanella (1998) has calculated that even an anomaly as strong as the 1650 nT anomaly observed at Pantelleria (Strait of Sicily) cannot yield a field deflection exceeding 1°. Thus we retain unlikely that the 1.6° declination shift observed at Castellaccio is entirely due to local magnetic anomalies. The discussion on this point has been implemented in the text accordingly**
11) I do not think we can see geomagnetic jerks with a time step of 10 years in Figures 3 and 7 ?
OK, we have deleted this part, following also advice of Rev#1.

Yours sincerely,

Francesca D'Ajello Caracciolo (on behalf of the co-authors)