Reply to comment by F. Speranza et al. on "Recent eruptive history of Stromboli (Aeolian Islands, Italy) determined from high-accuracy archeomagnetic dating"

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[1] We are grateful to *Speranza et al.* [2005] (hereinafter referred to as SSM) for the opportunity to clarify the respective merits of the "large sample" and "core-drilling and stepwise demagnetization" methods to determine paleomagnetic directions. The former was initiated in the '30s by Thellier and made popular in the fields of archeomagnetic research. SSM's criticism can be summarized as seven points.

[2] The first concerns laboratory cleaning procedures, use of demagnetization diagrams, and rock magnetic investigations. SSM feared that we might have used an inappropriate blanket af demagnetization. The Saint Maur paleomagnetic laboratory has decades of experience and full demagnetization with both af and thermal techniques, use of vector demagnetization diagrams and principal component analysis have been applied to thousands of samples. We have checked on a selected subset (more than 20 samples) from the Stromboli study that af demagnetization from NRM to 40 mT produced single component remanent vector going through the origin (e.g., sample S1-5, Figure 1a). Detailed thermal demagnetizations do not show any change in direction larger than 1–2° (e.g., sample S1-9, Figure 1b). We are using only single component samples with recent magnetizations that have not been overprinted. The mini-core method has its own drawbacks: possible heterogeneity of magnetization on a larger scale, lower accuracy of orientation, risk of producing a secondary drilling induced remanent magnetization, or DIRM. Such DIRM [e.g., *Genevey et al.*, 2002] may result in a curved demagnetization diagram up to large af (60 mT) and lead to larger dispersion and different paleodirections as observed by SSM in their samples. Such DIRM cannot occur in our case. Magnetic mineralogy (hysteresis, susceptibility, etc.) may be different from one sample to another, but we have repeatedly checked that this has no bearing on the stability of the paleodirection. Thus, the large samples result in good control of potential small-scale heterogeneity in magnetization. Therefore, SSM's understandable concern is unsubstantiated upon analysis.

[3] The second comment regards the number of discarded samples which SSM find too large, resulting in artificially small confidence intervals. In only three sites have we rejected more than one sample, and this is by no means arbitrary. For lava spatters lying on unstable ground, it is almost inevitable that some parts of the outcrop have been displaced after cooling, and *Speranza et al.* [2004] (hereinafter referred to as SP2004) themselves envision this possibility for three of their sites. But they give only the "number of cores giving reliable directions" in their Table 1, so that the effect of final sample selection cannot be evaluated in their own case. We illustrate our most criticized site, S1 (α_{95} = 1.0°, k = 1507) in Figure 1c. Five of the 17 samples were clearly displaced: 1) their paleodirections do not change during af demagnetization, and 2) they lie far away from the *McFadden* [1982] rejection circle, which contains the other 12 samples used in the average paleodirection. These 12, widely distributed, kg-size samples can hardly be considered as "undersampling" compared to 9 mini-cores taken from the same site (Str04 given by SP2004, with α_{95} = 4.3°, k = 147). Further evidence that our accuracy is not due to "arbitrary rejection of data" is provided by the more steady lava flows (sites S2 and S5 of *Arrighi et al.* [2004, Table 1] (hereinafter referred to as AR2004)), where all 22 and 11 samples, respectively, were used for the average paleodirection, giving α_{95} of 1.5° and 1.8°. We recall that over the last three millennia, secular variation in Europe is contained within a cone with a 12° half-angle, which requires uncertainties no more than 20 per cent of this, or about ±2°, in order to draw a high-resolution master curve. SSM are right to note two S1 samples whose declinations should be "of 3° and 7°, consistent with the mean declination of site Str04": this was due to an East-West sign inversion in several figures by *Arrighi* [2004], the true values being negative (samples S1-5 and S1-16 in Figure 1c). We thank SSM for spotting this misprint.

[4] As noted by SSM in their third comment, the geomagnetic field itself could be too distorted in volcanic terrain to properly retrieve the true geomagnetic direction; this potential problem equally applies to SP2004. The importance of this effect can be limited by adequate sampling, as shown by a detailed experimental study [*Tanguy and LeGoff*, 2004]: in 11 out of 12 archeomagnetic sites distributed over the whole area of Mt. Etna, the field

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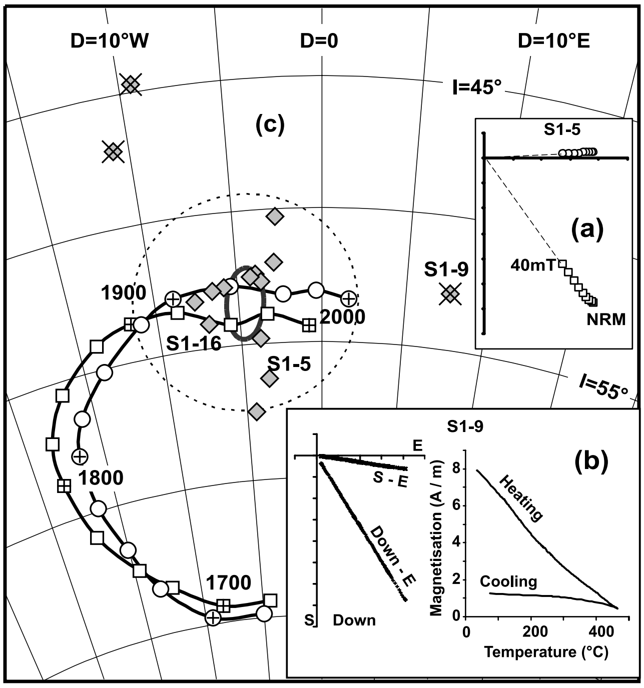

Figure 1. Results (Inclination-Declination) from our Stromboli site S1. (a) An example of a vector diagram of an af demagnetized sample (S1-5, 5 to 40 mT). (b) Example of a continuous thermal demagnetization (320 measurements) on sample S1-09. (c) A stereographic projection showing 1) samples from site S1 used in the mean (diamonds), samples being presumed to have been displaced after cooling (crossed diamonds) out of the dashed circle representing McFadden's limit of rejection (two are out of the figure), 2) the solid oval of the bivariate Fisher's 95% confidence cone, and 3) the 1675–2000 parts of the secular variation curves in Sicily (circles) and in Paris relocated to Sicily (squares), using *Jackson et al.*'s [2000] model.

measured at present does not differ from that outside the volcano area by more than 1.5° at the 95% confidence level.

[5] For the determinations of spatter ages (their comment), we agree with SSM, and pointed out in AR2004 that several possibilities exist, which must be constrained by other data. This also holds for SP2004 to a larger extent given the larger uncertainties. For instance, their site Str04 encompasses three possible dates between AD 400 and 1600, of which none is consistent with the presumed age of AD 1930. Conversely their very fresh site Spa11, close to the summit, does not fit any recent part of the reference curve and can only be associated with the Roman epoch.

[6] The fifth point regards the concern that we might be using circular reasoning when defining reference points. As for archeological materials in France, our "volcanic" curve in Italy is drawn by using historically dated eruptive products from the present to AD 1300. When going backward into the past, an increasing number of sites may present results that disagree with their presumed ages, although the path of the curve is correctly traced. In order to calibrate this curve as a function of time, we can compare ages from the volcanic curve and ages from the French archeological curve. The close agreement between the latter and our "benchmarks", represented by unequivocal eruptions on Ischia island (Gulf of Naples) in AD 1302, and Vesuvius in AD 1139, 787, 472, and 79, provides strong checks of the validity of our curve (see also the 146 BC archeological site of Carthage, close to Sicily) [*Tanguy et al.*, 2003].

[7] The next point concerns westward drift of the non-dipole field. Although such concern is justified, it has been recalled by *Dormy et al.* [2000] that the "traditional" views on westward drift are often over-generalized. Actually, as an example, when we transfer the reference geomagnetic direction from Paris to Sicily for the period 1675–2000 (using the model of *Jackson et al.* [2000]), we observe a time-lag of about 25 years (or 1.7° to 2.2°, as expressed as a spherical angle; see Figure 1c) in the 20th century, much less during the 18th century. As recognized by *Evans and Hoye* [2005, p. 159], "there is, of course, doubt concerning the geographic extent over which the VGP method can be employed, but a posteriori the results of Tanguy at al. suggest that the distance from Sicily to Paris is not too great. Our experience is the same." We point out that our age determinations take into account not only the French reference curve, but also the Vesuvius and Etna historical benchmarks, which do not suffer from any drift.

[8] SSM are finally concerned that we might "ignore the direct measurements of the geomagnetic field in Italy": they simply do not exist. If one excepts Kircher's 1640 measurement in Rome (which fits our reference curve), no other direct historical measurement (both I and D) was made in Italy before the 1800s.

[9] We appreciate the reasons for SSM's concerns and thank them for spelling them out, but we believe that they were due to lack of space to produce all our intermediate results and reasoning. Yet, essentially none of these concerns finally hold and our original conclusions stand. Certainly, accurate magnetic data from the many archeological sites in Italy would provide the best reference, and this is just what we are doing by jointly developing a large sample laboratory in Italy. Blending whenever possible the best of the two "archeomagnetic" and "paleomagnetic" methods is of course desirable (though in some cases it may be cumbersome or time consuming) and is actually done by proponents and users of the "large sample" method more often than realized. Though indeed "unconventional" and due to Thellier long ago, the method is therefore not at all obsolete and we show how valuable it remains.

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