Reply to "Comment on 'Diffusion of epicenters of earthquake aftershocks, Omori's law, and generalized continuous-time random walk models' "

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Several methods have been proposed to study the spatiotemporal correlations between earthquakes. Marsan and co-workers proposed a method based on correlations between all earthquake pairs, without distinction between mainshock and aftershocks, and interpreted their results in terms of a space-time coupling in the triggering process between events. In contrast, we studied the diffusion of aftershocks by analyzing the average distance between a triggered event ("aftershock") and a previous large earthquake (the "mainshock" which initiated the aftershock sequence). We reply to the comments of Marsan and Bean on our previous paper and discuss the applicability of both methods to unravel the spatiotemporal coupling of earthquake triggering processes.

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As stated by Marsan and Bean in their comment [1], and earlier in Ref. [2], the two methods used by Marsan and co-workers (MC) on the one hand and us on the other hand do not study the same process, and are therefore difficult to compare. We focus on the properties of earthquake triggering. We measure how the distance r between triggered events ("aftershocks") increases as the function of the time t since the mainshock which initiated the sequence [2,3]. We then determine the exponent of aftershock diffusion H defined by $r \sim t^H$. A value $H=1/2$ corresponds to normal diffusion, $H=0$ implies no diffusion, and $0<H<1/2$ corresponds to subdiffusion. We analyzed in Ref. [3] a branching model of seismicity ["epidemic-type aftershocks sequence" (ETAS)], which assumes that each earthquake can trigger other earthquakes with a rate that decays in time according to Omori's law and with a spatial distribution of first generation aftershocks independent of the time since the triggering earthquake. The cascades of aftershock triggering induce a diffusion of aftershocks relatively to the mainshock. Our methods of estimation of H have been validated on synthetic catalogs generated by the ETAS model. The theoretical results on the ETAS model have been compared with real aftershock sequences in Ref. [2].

In contrast, MC study the average distance r between any pair of earthquakes as a function of the interevent time t (corrected to remove the long-term uncorrelated seismicity) [1,4–6]. They characterize the spatiotemporal correlations between any pair of earthquakes by the diffusion exponent $r \sim t^H$.

Marsan and Bean [1] first criticize the applicability of our method to real data. In particular they discuss the difficulty of how to "reset the clock" and how to remove the influence of uncorrelated "background" events. These points have already been discussed in Ref. [2]. The influence of past and of background events should be to increase artificially the diffusion exponent, because the distance between uncorrelated events, which are more important at long times after the mainshock, is on average larger than the distance between a mainshock and its aftershocks. The conclusion of the analysis of real data with our method [2] was that diffusion in real seismicity is very small if any. This suggests that our conclusion is not biased by background seismicity, despite the criticism of Ref. [1]. We also suggested in this paper an alternative method to remove the background seismicity, based on appropriate wavelet transforms.

We agree with Marsan and Bean when they write that "a temporal decorrelation of the seismicity field with a rate depending on the epicentral distance, as seen by MC, does not necessarily imply a direct diffusion of aftershocks." The problem is that MC claim in Refs. [1,5,6] that their method "is appropriate for capturing the space-time coupling present in earthquake triggering process," i.e., they interpret their observations of earthquake diffusion in terms of aftershock diffusion. They compare their observations with models of aftershocks [6], while the diffusion observed by their method may have a different origin. Their method provides a useful tool for characterizing the space-time coupling in earthquake catalogs, but the interpretation of these results in terms of physical properties of earthquake triggering is problematic.

The tests discussed in Ref. [3] were performed by Marsan using two synthetic catalogs provided by Helmstetter. Marsan latter found that the software he used for these tests was flawed, and the initial results of Ref. [3] should be disregarded. When the corrected software is used, the results are in closer agreement with the theoretical values, but still some

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points of disagreement remain. The first catalog was constructed by the superposition of a uniform uncorrelated seismicity (background) with a Poissonian temporal distribution of ten aftershock sequences. Aftershocks were generated using a power-law decay in time (Omori law), a power-law decay in distance from the mainshock (without spatiotemporal coupling or cascading), and with a power-law distribution of the number of aftershocks per mainshocks. By construction, this catalog has no diffusion. The second catalog included one very long aftershock sequence, without an additional background rate. It was generated using the ETAS model with a diffusion exponent $H=0.2$, characterizing the diffusion of aftershocks relative to the mainshock (first event of the sequence). Because there is no background seismicity in this model, the average distance from the mainshock continuously increases with time, and the rate of seismicity decreases with the time since the mainshock.

While the corrected software used in Ref. [1] was much better than the previous one to remove the influence of uncorrelated seismicity (which was responsible for the large spurious diffusion exponent $H=0.5$ obtained with the first catalog), we still observed some spurious diffusion when tested on synthetic catalogs without genuine diffusion. While MC's method corrects for the effect of uncorrelated seismicity, the method becomes unstable for large distances and long times, when the correlated seismicity becomes comparable to the uncorrelated seismicity [2]. Marsan and Bean [1] claim that their method is not appropriate for the first catalog because there is no relaxation to the background seismicity at large times. This catalog was, however, generated with a significant fraction of background events (20%), probably as much as for real seismicity.

According to Marsan and Bean [1], "the second catalog does experience the same type of relaxation as observed for the real data analyzed in MC." By construction, this catalog has no uncorrelated seismicity, and the rate of seismicity decays down to zero as time increases, as can be seen in Fig. 1(b). The method of Marsan *et al.* thus should not be applied to this catalog. It should, however, be appropriate for the first catalog which has a significant fraction of uncorrelated seismicity, similar to real seismicity. We agree that "the diffusion exponent for this second catalog, $H=0.072$, cannot be compared to the expected theoretical value $H=0.2$ of the ETAS model since these exponents do not measure the same phenomenon." The method of MC characterizes the spatiotemporal correlation of seismicity but is not appropriate to measure aftershock diffusion. Marsan and Bean [1] have thus confirmed that their method is not able to recover the diffusion exponent of aftershocks, and that their results should not be compared with theoretical results for the diffusion of triggered seismicity, such as Dieterich's model of seismicity [5,7].

We agree with Ref. [1] that "observations of seismicity diffusion for the ETAS model do imply anomalous stress diffusion, since the stress generated by the subsequent earthquakes diffuses with these earthquakes." We should have written in Ref. [3] that "observations of seismicity diffusion for the ETAS model do not necessarily not imply an anomalous diffusion of the stress change induced by the mainshock," in contrast with models of aftershock diffusion based on fluid flow or viscoelasticity.

We also agree with Ref. [1] that "observation of seismicity diffusion for the ETAS model does not save us from investigating what is the physics at work in the process." We have already mentioned in Ref. [3] some physical processes leading to Omori's law. (See also Ref. [8] for a detailed review of the physical mechanisms leading to Omori's law.) Actually, our approach amounts to a two-scale analysis: the physical processes enter in the determination of Omori's law in a first step while the cascade of triggering renormalizes it and may create (anomalous) diffusion.

Finally, we have not "introduced an arbitrary law with algebraic decay for drawing the distance between the trigger and the aftershock." This choice of a spatial power-law kernel was justified by a comparison of different kernels by Ogata [9], who showed that a power-law kernel better explains (in terms of likelihood) the data. In addition, we show in Ref. [3] that the seismicity rate $N(t,r)$, in the limit of sufficiently large times t and distances r, is essentially independent of the specific shape of the temporal and spatial kernels: in particular, all spatial kernels with finite second moments give the same results in the scaling regime. This derives from the large t and r expansion of the Fourier and Laplace transforms of the kernels. For example, where the spatial kernel is a power law, $\phi(r) \sim 1/r^{1+\mu}$, the above statement holds for $\mu \geqslant 2$ (finite variance) [3], which seems to be the appropriate regime for real data [9].

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