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Interactive comment on "Utsu aftershock productivity law explained from geometric operations on the permanent static stress field of mainshocks" *by* Arnaud Mignan

Anonymous Referee #1

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The MS presented by Dr. Mignan intends to provide the background of the aftershock productivity law where the number of aftershock is proportional to the exponential of the magnitude (M) of a mainshock. On the basis of "Solid Seismicity Postulate"(SSP), the author derives the formula of the expected number of aftershocks as a function of M which agrees with the productivity law originally suggested by Utsu [1970]. The derived formula has a break in the log-linear relationship between the aftershock productivity and M whereas the break is not found through the analysis of real aftershock data. The author suggests that this inconsistency is caused by an aftershock selection bias with a numerical simulation.

I have two major concerns on this MS as shown below.

a) I do not understand well what new significant results are in this MS. In Hainzl et al [2010, JGR], the aftershock productivity law has already been reproduced with a numerical simulation. The simulation is based on the "clock-advanced" model, which is a simple but realistic physical assumption.

By contrast, SSP is too simple, and because of this simplification its physical background seems obscure and unrealistic. Furthermore, the postulate has not been supported by real data (In some of the author's previous papers, seismicity model derived from SSP has been applied to real seismicity data. Note that, however, only temporal patterns of seismic activity are analyzed. To validate SSP where we have only three seismicity levels in space, it is indispensable to reproduce spatial patterns of real earthquakes.). This MS does not show any convincing motivation to explain the productivity law with such an unsupported postulate.

I understand that sometimes it is important to introduce a (too) simple model/assumption for explaining an empirical law. However, it is also important to provide some new and meaningful perspective as a result of the introduction. The results shown in this MS do not go beyond the results of Hainzl et al. [2010], and therefore the introduction of SSP is unproductive.

b) In the end of Section 3, the author suggests the break in scaling in the aftershock productivity data (Eq.(16)). However, as a result of the analysis of the real aftershock data, no break is found (L.182-183). To explain the result of "no break", in Section 4 the numerical simulation with the ETAS model was conducted. Then, the author ascribes this result to the "aftershock selection bias" (L.206-207) in the numerical simulation.

The author's conclusion is one possibility, but it is also possible that Eq.(16) is incorrect; the numerical simulation shown in Section 4 is inconclusive, and I do not understand what is the meaning of showing such a vague consequence. The application of an aftershock selection approach having a serious problem (the bias in this case) itself is inappropriate. Why does the author use any other approach which does not contain such a problem?

In other words, only a negative possibility for the postulate is shown and no positive support is not given in the present form of this MS. To my opinion, this is another major drawback of this MS.

Some further comments:

1) Introduction of the Zero-Inflated Poisson (ZIP) distribution

The reason of the introduction of the ZIP distribution is described in L.165-166 ("this approach ... zero aftershock"), but this explanation seems insufficient. Behind the ZIP distribution, we have the following assumption. We have two possibilities: the first is that the number of events follows a Poisson distribution, and the second is that it is deterministically equal to zero. One of these two possibilities is chosen through the Bernoulli distribution.

As far as I know, a physical (seismological) process corresponding to the Bernoulli distribution is unclear in generating earthquakes. If the author persists in introducing the ZIP distribution, explain what is the physical process.

2) The simulation shown in Section 4

This simulation is based on the ETAS model, and this violates the self-consistency of this MS. As seen in $g(x, y|M)$ of Eq.(17), the spatial density of aftershocks gradually decays with the distance from a parent event. This property completely disagree with SSP (see Eq.(5)). For the self-consistency, the simulated spatial (and temporal) pattern of earthquakes should be generated on the basis of SSP.

3) $\alpha = 2.04$ (L.197)

I do not understand how the author incorporated this information (value) into Eq.(16).

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