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Interactive Comment

Interactive comment on "Static behaviour of induced seismicity" *by* A. Mignan

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Dear reviewer,

Thank you for your comments on the discussion paper by Mignan (2015). Below is my two-part answer to (1) clarify how model complexity is assessed from the concept of description length and (2) discuss how the proposed geometric approach could be extended in the future to include anisotropic/heterogeneous cases.

1 About model complexity

I agree that the spatiotemporal expression $r(t)=\sqrt{(4\pi Dt)}$ with D the hydraulic diffusivity and t the time since the injection start (Shapiro et al., 1997) is relatively simple to derive from Biot's theory (this will now be indicated in the revised version). However this equation has difficulties describing the early stage of injection, which had led to the addition of a non-zero starting time t0 by Shapiro's group (see Figure 2 of Basel data fit in Shapiro and Dinske (2009); see Figure 4 of 2004/2005 KTB fit in Shapiro et al. (2006)). A value t0 > 0 is difficult to conciliate with linear diffusion. Instead one can use the form V(t)ˆ(1/x), with V(t) the injected volume temporal profile, as already promoted in Shapiro and Dinske (2009). Such form is obtained from nonlinear diffusion dynamics, which is by definition already more complex than the linear approach mentioned by the reviewer (In the discussion paper of Mignan (2015), the term "complex" referred principally to nonlinear diffusion dynamics, which requires numerous assumptions to obtain the desired parabolic expression – Moreover that term is only used once in the manuscript. I never used the expression "extremely complex" that is quoted by the reviewer).

It should be noted that the "level of complexity" discussed in the manuscript is not reflected in the number of variables (since a similar expression is obtained in both cases) but in the number of assumptions and steps made to reach a similar expression. The term "lower description length" will be better explained in the revision. Figure 1, to be added to the revision, illustrates the fundamental difference between the poroelastic approach and the new geometric one based on the N-C PAST postulate (Mignan, 2012). It clarifies that the geometric approach is of lower description length than diffusion dynamics in the sense that it only requires process A (overpressure due to fluid injection) to explain process B (induced seismicity) instead of process A yielding process A2 (fluid flow in porous medium) yielding B.

Nevertheless, the proposed static stress model will not anymore be solely justified by its simplicity (which may be considered anecdotal by some readers) but will be promoted as an alternative physical framework worth exploring in more detail (thus requiring a rewriting of the abstract).

Regarding the comments of the reviewer on specific parameters:

The condition rmax $\geq$ max(rA*) can be explained as follows. Let us consider two

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different values rmax1 and rmax2 and $\Delta\mu$ the difference of rate μ(rmax1) - μ(rmax2) (Eq. 3d). It yields $\Delta\mu$ equal to the rate of events in the concentric shell comprised between rmax1 and rmax2. Since rmax > rA*, this rate remains constant over time. In other words, a larger rmax value only increases the linear trend in the cumulative number N(t) time series. If rmax is taken too high, it will in practice tend to mask the non-stationary pattern to be investigated. This will be clarified in the revised version. However the value of rmax has no impact in the Basel application since this term disappears from Eq. 3 due to δb0 = 0. The parameter r0, defined as the infinitesimal radius of an infinitesimal volume V0, is only incidental and disappears for the induced seismicity case (where d=n). This remark will be added to the text.

2 Remark on anisotropy and other heterogeneities

The Basel example can be considered a textbook case for its simple features. I agree with the reviewer that the proposed static stress model has yet to be tested on more problematic data sets. However it should first be noted that the proposed approach already explains the two main empirical laws of induced seismicity without any specific dataset in mind and is based on a theoretical framework not primarily developed for induced seismicity. The demonstration presented in the manuscript should therefore be considered as a general (non site-dependent) result (the Basel example illustration representing only one fourth of the paper).

Although out of the scope of the reviewed manuscript, I give below a few remarks on how anisotropy and other heterogeneities could be included in “seismicity geometric reductionism”, i.e. in geometric operations on a static stress field, or in this context, on a superposition of static stress fields. Figure 2 (modified from Fig. 1a of Mignan (2015)) shows how existing stress heterogeneities represented by fluctuations σ(r) < σ_0^* (background stress amplitude range) can impact the induced seismicity patterns. Such idea was already proposed in Mignan (2011) to explain how tectonic precursory patterns could vary depending on the historical static stress field of a given region. Although addition of such historical background stress profile has yet to be tested for real cases, it could explain propagation of induced seismicity along existing lineaments (e.g., Shapiro et al., 2006) as well as other non-trivial spatiotemporal patterns. In Figure 2 for instance, addition of a stress memory on a nearby fault would lead to two clusters of induced seismicity, one spherical, centred on the borehole and a second, elongated, following the fault structure.

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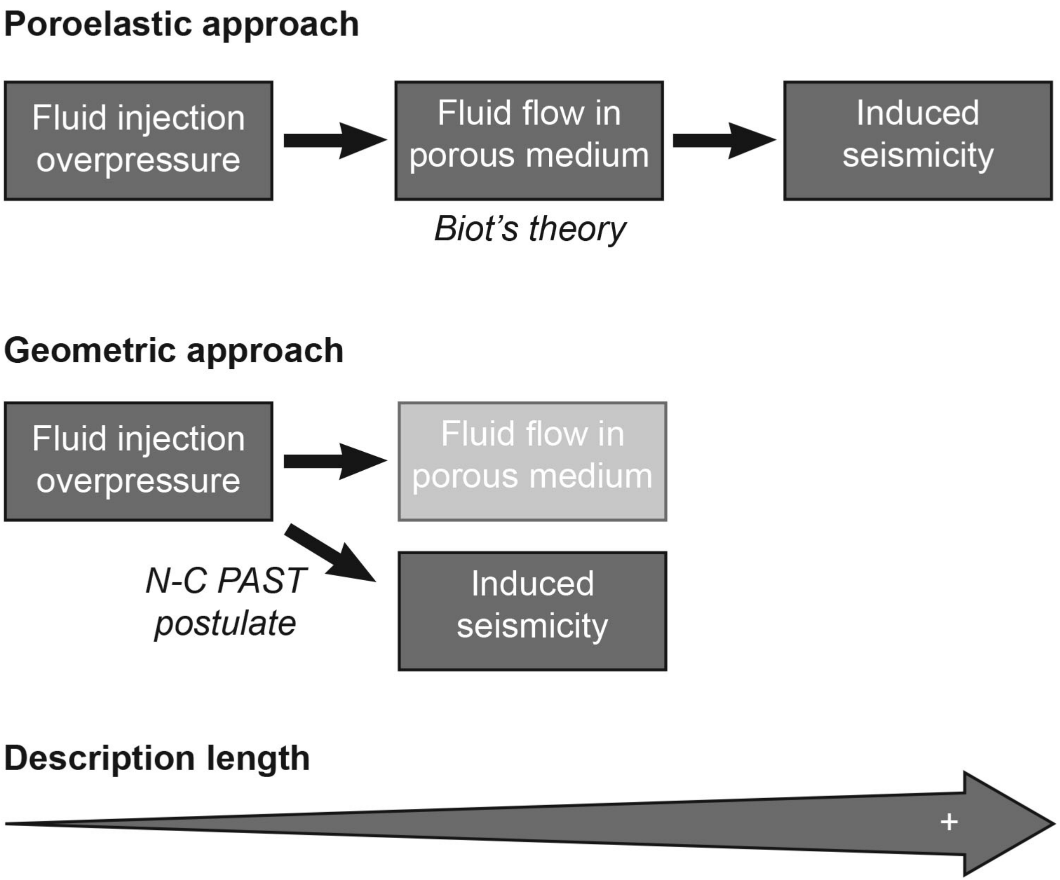

Fig. 1. Description length defined as the count of physical steps required to describe induced seismicity, in poroelasticity and in the newly proposed geometric approach.

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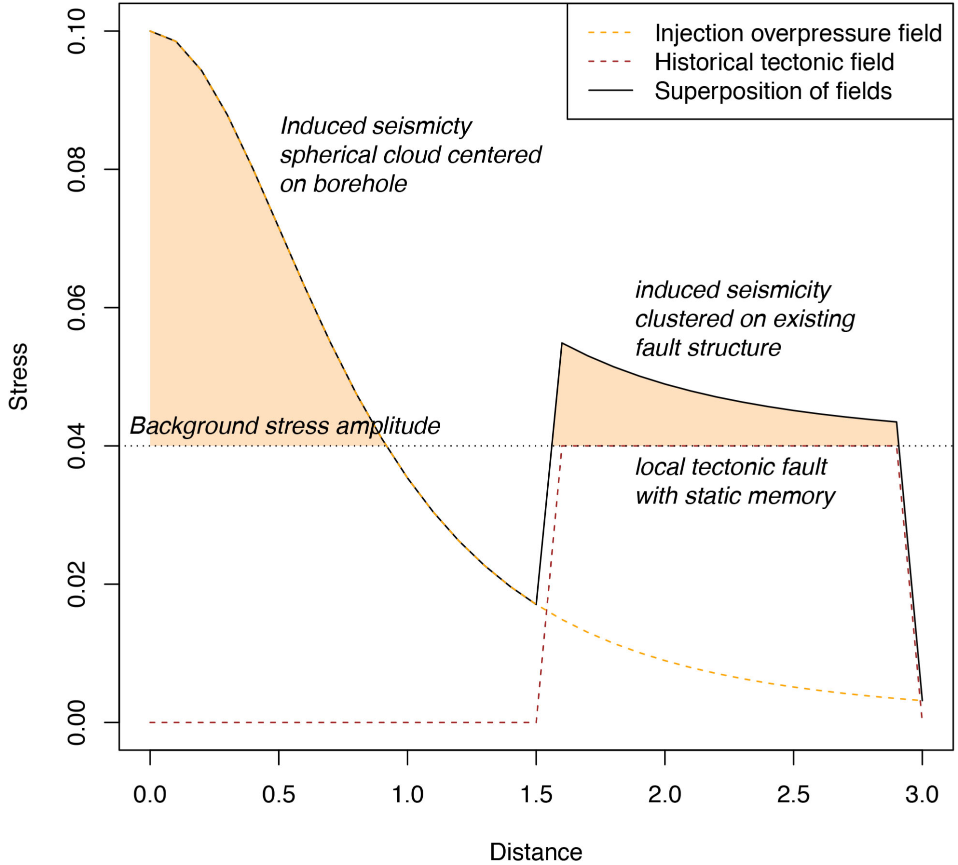

Fig. 2. How anisotropy and other types of heterogeneities can be implemented in the geometric approach by adding a historical tectonic static stress field (ad hoc parameter values used for illustration)

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