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Dear Editor and Reviewers,

Please find below my answers to your comments, as given in the discussion section of NPG. I now additionally refer to line numbers of the annotated version of the modified manuscript to describe specific changes. Those additions are highlighted in red in the present reply.

Sincerely,

Arnaud Mignan

Anonymous Referee #1

Received and published: 12 January 2016

The paper is an interesting attempt to “model” basic features of induced seismicity as observed in fluid injection projects. The author claims that the “extremely complex” process of diffusion dynamics in a poroelastic medium can be replaced by simple geometric operations on a static stress field. It is interesting that this – for the extremely simple case dis-cussed in the paper – seems to be feasible although the value of the procedure remains questionable. The statement that the physical cause of induced seismicity (the overpressured fluid flow in the subsurface) can be replaced by geo- metric considerations – at least to a certain extent in extremely simple cases – may be true. However, it is not obvious what the ignorance of the actual physical processes generates as additional insights from a scientific perspective.

It is not true, as claimed several times in the paper, that the modelling of the pressure diffusion into the subsurface is extremely complex. In the simple case as considered in the paper, a simple diffusion equation can be used and is actually used in the quoted literature from Shapiro, et.al.. Whether the suggested theory holds in cases where the stress field is heterogeneous and anisotropic, where permeability is spatially variable and anisotropic, and where fluid flow modelling becomes more demanding remains an open question. It would be desirable if the author could justify his approach by other reasons apart from the apparent complexity of the physical modelling. Most scientists would not consider a 1D diffusion equation as extremely complex. The theory includes a number of assumptions which may be reasonable but are hard to justify. rdmax is not defined apart from the remark that it needs to be a larger constant envelope. The definition of r0 in formula 9 and 10 is also unclear. The authors should explain that in formula 10 they have essentially the relation between the stress field variation and the pressure change by fluid injection, which can be positive or negative.

They claim that there is only one free parameter left, the normalized background stress amplitude range. However, in Shapiro's theory it is – in the end – also only one parameter that controls the spatio-temporal evolution of the seismicity: the seismogenic index.

Summary: I think that it is certainly worthwhile to publish the paper as it represents an approach to induced seismicity, the limitations of which are subject of further research. The claims by the author that they use a much more simple theory in a much smaller parameter set for modelling induced seismicity is not true and should be removed from the paper, at least in the present form. This requires substantial rewriting so that I evaluate the paper as requiring substantial revisions.

Reply to Anonymous Referee #1

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 (now indicated line 7 of p.9). However this equation has difficulties describing the early stage of injection, which had led to the addition of a non-zero starting time t_0 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 $t_0 > 0$ is difficult to conciliate with linear diffusion. Instead one can use the form $r(t) \propto \sqrt[x]{V(t)}$, with V(t) the injected volume temporal profile, as already promoted in Shapiro and Dinske (2009) – These remarks can now be found lines 5-10 of p.9. 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 was only used once in the manuscript. I never used the expression "extremely complex" that is quoted by the reviewer). Other changes have been made throughout the text to show that the parabolic expression in itself is simple, whatever the underlying physical process involved (lines 3 and 5 of p.2). The term "simple" was removed in front of "static stress model" (line 12 of p.2, line 4 of p.10) to not oversell the proposed model for its simplicity. The simplicity of the geometrical demonstration is referred to "As a side note, …" (line 9 of p.6).

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" is now better explained in the revision (first paragraph of the new discussion section). Figure 4, now 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 (i.e. Biot's theory bypassed, lines 26 of p.8 to 4 of p.9).

Nevertheless, the proposed static stress model is not anymore solely justified by its simplicity (which may be considered anecdotal by some readers) but is promoted as an alternative physical framework worth exploring in more detail (see modified abstract, line 18 of p.1). The discussion on model complexity is now relegated to the discussion section only (all references to it are removed from the conclusions). See in particular lines 11-14 of p.9 that states that the simplicity of the proposed model does not infer that it is superior to poroelasticity but that it is an alternative approach worse exploring in more details.

Regarding the comments of the reviewer on specific parameters:

The condition rmax ≥ max(rA*) can be explained as follows. Let us consider two 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 is now clarified in the revised version (see lines 9 and 18-20 of p.4). 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 has been added to the text line 22 of p.5.

Eq. (10) is now described as per the reviewer's recommendation (lines 24-26 of p.5).

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 originally of the scope of the reviewed manuscript, I now give 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 (see second paragraph of the new discussion section). Figure 5 (modified from Fig. 1a of Mignan (2015)) shows how existing stress heterogeneities represented by fluctuations $\sigma(r) < \sigma_0^*$ (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 5 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. See lines 15-33 of p.9 and the new figure 5.

References:

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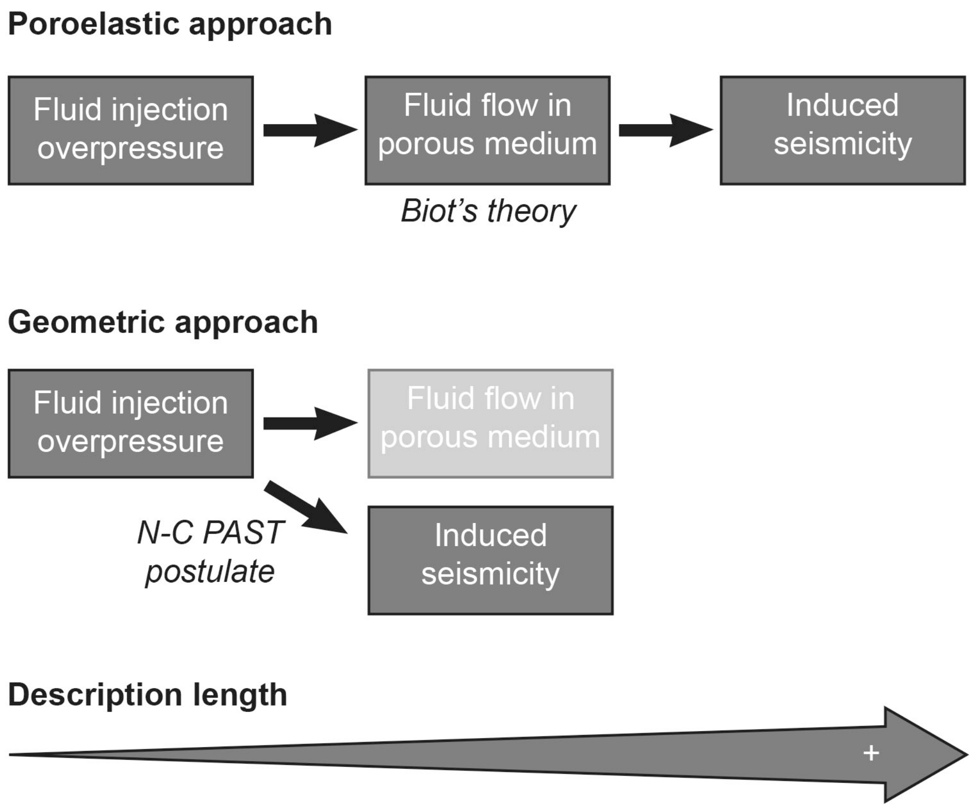

Figure 4. Description length defined as the count of physical steps required to describe induced seismicity, in poroelasticity and in the newly proposed geometric reductionism.

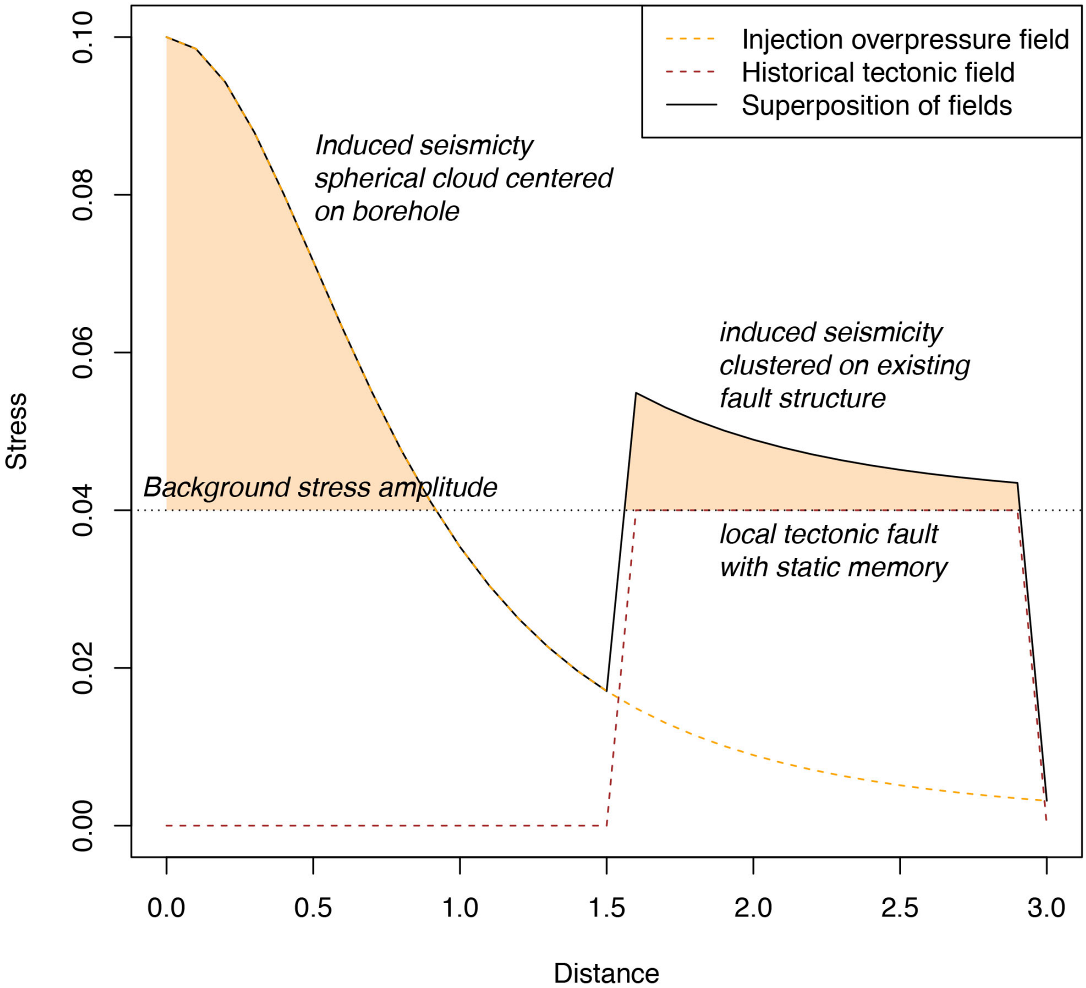

Figure 5. How anisotropy and other types of heterogeneities can be implemented in the geometric approach by adding a historical tectonic static stress field (ad hoc parameter values have been used for this illustration).

Anonymous Referee #2

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This is a very interesting paper, in which the author attempts to model the phenomena of induced seismicity using the principles of NC-PAST. I agree with Dr. Mignan's proposed revisions to the text, based on the comments of the first reviewer, although I am not convinced that the new figure 1 is necessary. I also think that Figure 2 from his reply is very informative.

Reply to Anonymous Referee #2

Dear reviewer,

Thank you for your comments on the discussion paper by Mignan. Part of my previous answer, titled “Remark on anisotropy and other heterogeneities”, is now added to the manuscript in a new discussion section (including the new Figure 5 – see lines 15-33 of p.9). Regarding Figure 4 of the same answer, I still believe that it can be useful to some readers. It clearly illustrates the “short cut” allowed by the NC PAST, which is to model induced seismicity without the need of poroelasticity (while poroelasticity is still required to model fluid flow, which can be of interest for different reservoir analyses). The figure is used/described in the new paragraph lines 26 of p.8 to 4 of p.9.