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Nonlinear Processes in Geophysics
Discussions

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

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The paper is an interesting attempt to "model" basic features of induced seismicity as ob-served in fluid injection projects. The author claims that the "extremely complex" process of diffusion dynamics in a poroelastic medium can be replaced by simple ge-ometric opera-tions 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 overpres-sured 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 pa-per, 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 am-plitude range. However, in Shapiro's theory it is – in the end – also only one pa-rameter 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 ap-proach 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.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1659, 2015.