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Interactive comment on "The physics of fault friction: Insights from experiments on simulated gouges at low shearing velocities" *by* Berend A. Verberne et al.

Anonymous Referee #1

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Dear Editor, I have reviewed the paper entitled "The physics of fault friction: insight from experiments on simulated gouges at low shearing velocities" by Verberne et al., as a potential article to be published in Solid Earth. I start by saying that this is a review article and does not contain any new results. The authors summarize a vast amount of work that that has been carried out in the last 20 years or more at the Utrecht University rock mechanics laboratory. The main focus of this review is to give the reader a general overview of the development, and the current state-of-the-art, including strengths and limitations, of the CNS (Chen-Niemeijer-Spiers) microphysical model for fault friction and the earthquake cycle that is based on experimental evidences derived from experiments performed at the Utrecht laboratory. The review is well organized, in fact

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the authors start by clearly describe the experimental facilities used to perform the experiments, then they summarize some case studies that have put the basis for the formulation of CNS model. Afterwards, they describe the theoretical foundation of the model validating it with a comparison with the experimental data. Finally, they discuss the CNS model as applied to earthquakes simulation with a comparison with Rate- and State- Friction (RSF) constitutive equations that is the most used framework so far. Throughout the review, the authors make the appropriate references to direct the reader to the relevant papers that have been published regarding the various aspect of the development of the CNS model. The papers are all in very good journals and highly cited so that the scientific basis for this model is not under discussion. In general the paper is very well written and organized and the figures are appropriate and it represent a necessary step to summarize the work done in developing this model. For these reasons I recommend publication after the authors address some minor concerns as listed below.

Comments to the authors: General: A minor aspect, but it should absolutely improve, regards the figures. From figure 3 throughout figure 10, they look like screen shots taken from the cited articles. This is true to the point that in figure 3 is impossible to read the text in the different figure panels. I strongly recommend the authors to produce high quality figures. I think that one aspect that should be improved in this review, since in the single papers cited is poorly addressed, is the relation between the CNS model and its physical basis with the interpretation of the mechanical work related to dilation as it was developed by Marone et al., 1990 JGR and Beeler et al., 1996 JGR. In particular they interpret the velocity dependence of friction (v-strengthening or weakening) based on an energy balance of the work done against the normal stress (i.e. dilation rate) and relate it with the degree of shear localization. This basis are very similar to the CNS model. However, something that is not very clear to me is that in the observations of Marone and Beeler velocity perturbations that lead to fault dilation are associated with velocity strengthening frictional behavior, and shear localization is associated with a velocity weakening behavior. While in the CNS model increasing in porosity leads to

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velocity weakening and “localization” by ductile mechanisms to velocity strengthening. Those experiments were conducted on crystalline material such as quartz or granite and the CNS model was developed for calcite that notably undergoes IPS. Can the authors implement some comments about these models? Specific: L39: “Seismic fault motion of this type” it is not very clear to me. The authors refer to many fault slip styles in the previous sentence, so I would rephrase maybe with “the full spectrum of slip behaviors”. L84-91: About the frictional-viscous mechanism. I would avoid to generalize too much such mechanism as active at crustal scale everywhere as it reads now in the text. It is true that from the outcrop observation of ancient subduction zones and some phyllosilicate-rich faults (e.g. Fagereng et al., 2014; Collettini et al., 2011 these references may be added to the text) this behavior can be inferred as at play. However, this is not true for all the fault zones and I think that it is not appropriate to generalize it. Alternatively, it should be specified that the seismogenic zone refer only to subduction zones here. L110: since this is a review article I would give credit to the people that put the basis for the friction contact theory such as the work of Bowden and Tabor as well as Rabinowicz and not only Dieterich and Kilgore 1994. L330: in regard to the scaling of the critical slip distance with fault thickness I think that the citation to Marone and Kilgore, 1993 Nature is needed.

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