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Interactive comment on "Model calibration for ice sheets and glaciers dynamics: a general theory of inverse problems in glaciology" *by* M. Giudici et al.

M. Giudici et al.

mauro.giudici@unimi.it

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This document includes our reply to the anonymous reviewer #2.

The paper entitled "Model calibration for ice sheets and glaciers dynamics: a general theory of inverse problems in glaciology" by Giudici and colleagues discusses the resolution of Inverse Problems in glaciology. Overall, the manuscript is well written and clear, but I am concerned about the novelty of this paper. There is nothing new in terms of Inverse Problem theory. There is a very limited application to ice flow but most of the text is very general and can be found in textbooks.

We thank the Reviewer for his/her appreciation of the writing and clarity of the paper.

We agree with the Reviewer that our paper does not provide new methods or new applications, but we think that its value is on the fact that it helps to close the gap between scientists working on the theoretical aspects (namely non-linear forward and inverse modelling) and those who apply simulation models to elaborate and possibly assimilate observation data.
Moreover, no textbook is yet available on inverse problems in glaciology, at our knowledge. Finally, our paper is focused on some topics that are relevant for inverse modelling, even if not always described in textbooks.

1 General Comments

The purpose of this paper is to introduce a conceptual framework to unify different notations and to facilitate the definition of Inverse Problems and their properties in the field of Glaciology. Even though the paper is supposed to remain general, many aspects that are (presumably) new to the field are not fully developped. For example, the authors claim that "some basic mathematical properties are not fully considered in the applications" of Inverse Problem, but it is not clear what they are (ill-posedness vs stability? Identifiability? etc), and these aspects are hardly discussed in the paper so it is not clear what this paper brings to the community.

Among the basic concepts that are less considered in the existing literature on application of inverse problems to glaciology it is worth to mention identifiability (and its link with uniqueness), ill-conditioning (and its link with instability), global sensitivity (as opposed to local, one-at-a-time sensitivity). From the Reviewer's comments we realise that this is not clear from the original manuscript and we will reinforce the description

of these items in the revised version.

The application, based on the simplest ice flow model (isothermal SIA) does not add any value to the paper. A more in-depth analysis of the adjoint method for ice sheet models could have been interesting but this manuscript does not address any specificity or difficulty related to the use of SIA for modeling ice flow.

The simple paradigmatic example shows that a simple model could reduce the uncertainties related to ill-posedness or ill-conditioning of the inverse problem, but it is also useful to show other items, like the dependence of the inverse problem solution on the minimisation algorithm. We will try to clarify these facts in the revised version. We agree with the Reviewer that our example does not discuss the difficulties and the limitations of the use of the SIA for modelling ice flow, but this is outside the goals of this paper.

p 5520: "At the authors' knowledge, no test has been conducted in glaciological sciences with different hypotheses of pdfs." This statement may be true but it is not done here either.

We fully agree with the Reviewer and, in fact, we do not claim that this is done in our paper. However, we think it is important to stress that practitioners should be aware that the use of a given pdf should be either considered as a basic assumption or supported by data or observations.

2 Minor comments

- p. 5517 line 19: symmetric and positive definite
  We thank the Reviewer who suggested the modification of this expression with a more widely used form.

- p. 5518 line 23: I would suggest the author do not use $\mathcal{O}$ for the objective function as it is most commonly used in Landau notation. $\mathcal{J}$ is more common.
  We agree with the Reviewer that $\mathcal{J}$ is more common, but we prefer to use $\mathcal{O}$, because $\mathcal{J}$ could be very similar to the symbol used for the Jacobian matrix in case of a multi-objective optimization.
- p. 5519 line 4: "the the algorithm"
  We thank the reviewer; we will fix this typesetting error.

3 Changes planned for the revised version

The changes that we intend to include in the revised version of the paper are listed below.

1. We shall modify the introduction to clarify the goal of the paper and its value for researchers in glaciological sciences.
2. We shall include a list of guidelines in the conclusions, so that the paper will be more useful for those researchers who are starting to deal with inverse modelling in glaciological studies. Moreover, we shall reinforce the discussion of those topics which were less deeply examined by researchers in this field (identifiability, ill-conditioning, global sensitivity, etc.).
3. We shall discuss more extensively the example and, in particular, what can be learned from it. For this, we shall include also a short description of the problems that could arise from the application of a more complex model.

Interactive comment on The Cryosphere Discuss., 8, 5511, 2014.