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The Cryosphere Discussions

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

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This document includes our reply to the anonymous Reviewer #3.

We thank the Reviewer for his/her frank evaluation of the paper. We think to be able to improve the quality of the paper by taking advantage of some of his/her comments.

1 General comments

`This paper aims to provide details on theoretical aspects underlying inverse problems for the calibration of ice sheet`

`models. These aspects are illustrated with a simple numerical example.`
`The paper is generally well written and the theoretical concepts issued are clearly described.`

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

`However I am concerned by the relevance of this paper in a scientific journal dedicated to glaciology. The description of inverse problems and their theoretical aspects remains general and can be found in any good textbook dedicated to inverse problems. There is no focus on potential theoretical aspect of inverse problems specifically encountered in glaciology. The numerical application to ice flow is unfortunately only illustrative and not really representative of inverse problems for the calibration of ice sheet models.`

At our knowledge no textbook on inverse problems in glaciological sciences is still available and some of the topics discussed in this paper are only marginally considered in the textbooks on general-purpose inverse problems (e.g., identifiability, ill-conditioning, global sensitivity). We agree with the Reviewer that the numerical example is very simple, but this permits to stress how some of the difficulties that cause the inverse problem to be ill-posed could be hindered by a simple physical approach. We will modify the paper with a better description of the role and significance of the numerical example.

2 Specific comments

`The paper proposes to provide a "thorough and rigorous conceptual framework for inverse problems in cryospheric` studies" and aims to remain general. However several shortcomings prevent the paper from achieving its goal completely. I list them below:

• The paper oscillates between tackling inverse problems in glaciology or restraining its purpose to the calibration of ice sheet models. This ambiguity is noticeable in the text but also in the title of the paper itself. If the goal is to issue inverse problems in glaciology, then important fields of application such as ice core dating or the calibration of snow models are missing.

We agree with the Reviewer, because our goal was to introduce some general concepts, by making reference to the dynamical modelling of ice-sheets and glaciers. We thank the Reviewer, because this comment will permit us to improve the paper by clarifying in a better way that the exemplifications are mostly related to ice-sheet and glacier dynamics, but that the basic concepts can found applications also in different fields of glaciology.

• It is rather surprising that, after mentioning more than 15 papers dedicated to the use of inverse modelling for the calibration of ice sheet models with different methods, the authors state that "IP theory (...) has not yet become popular in glaciological sciences". The absence of recent key publications in operational glaciology might explain such comment.

We performed a statistical analysis with the Scopus data base and we found the results listed in table 1. The number of papers with keywords related to inverse problems ("invers*", "inverse problem", "inversion", "model calibration", "parameter identification") and keywords related to geophysics ("Geophys*"), glaciology ("Glaci*") and (surface or sub-surface) hydrology ("hydro*") have been extracted from the Subject areas "Earth and planetary sciences", "Environmental Science", "Physics and astronomy".

Table 1 shows that the percentage of papers dealing with glaciological studies whose keywords are related to inverse modelling is one order of magnitude less than that for the whole field of geophysics. Moreover, even if the analysis is compared with other restricted fields of geophysics, e.g., hydrology, the results show that there is a difference by a factor close to 2; moreover, notice that the papers extracted for the keyword "Hydro*" are sometimes related to fields different from geophysics (e.g., biology).

These results clearly show that the use and application of inverse modelling in the cryosphere sciences is much more limited than in other fields of geophysics. This motivates the statement that raised the Reviewer's comment.

• Linked to the previous point, it would have been interesting to recall which variables and parameters of ice sheet models are generally calibrated by inverse modelling and with which data in the list of citations given in the introduction.

We agree with the Reviewer and we will reinforce the paper, both in the introduction and in section 2, to improve the description of the role of field data.

• The first part of section 3 (p. 5518) defining an inverse problem is clearly based on optimal control theory but this point is not mentionned in the text.

The Reviewer is right and we will mention "optimal control theory" as a basis for the definition of IP in the text.

• The authors state (p. 5520, l. 1) that "most of the applications of the Bayesian approach compute the optimal parameters by means of the maximum likelihood method". As a consequence the paper forgets to mention that estimating posterior pdfs is more and more popular in geosciences as it gives more information than a maximum likelihood method. Methods such as Markov Chain-Monte Carlo are even used for

the calibration of ice sheet models in palaeoglaciology (see Tarasov et al., 2012).
We thank the Reviewer for having stressed this good point. We will introduce a short discussion of this item in the text, as it reinforces our remark.

• Section 4 lists different usefull notions for inverse problems such as the identifiability of the direct problem or the conditionning of a system. It also states that the inverse problem might be unstable due to either ill-conditioning, non-identifiability or non-uniqueness. Providing more details on this important issue would have been usefull as it would have helped the reader to select which of these notions is the most important for the resolution of her/his inverse problem.
We thank the Reviewer for his/her appreciation of the discussion of some topics of this section. We thank him/her for the suggestion to improve the paper by helping the reader to focus the most relevant aspects of the problem that he/she is solving. For this we shall expand some remarks in section 4 and we will include a list of guidelines in the conclusive section.

• The selected numerical application is not really usefull as it only illustrates a well-posed and well-conditioned problem. Unfortunately many inverse problems (including inverse problems for the calibration of ice sheet models) are ill-posed or ill-conditioned. Detailing an example in such configuration would have been more usefull.
The example is used to show that a simple model could reduce the uncertainties related to ill-posedness or ill-conditioning of the inverse problem. We will try to clarify this fact in the revised version.

3 Minor comments

• p. 5517, l. 1: p(cal) = (E;M).
We thank the Reviewer; we will fix this typesetting error.

• p. 5518, l. 23: J is more classical for objective functions than O (commonly used in Landau notation).
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, l.4: "the the algorithm"
We thank the Reviewer; we will fix this typesetting error.

• p. 5521, l.27: define SD (I guess it means standard deviation).
We thank the Reviewer and we confirm that his/her guess was correct; we will change the text accordingly.

4 Changes planned for the revised version

The changes that we intend to include in the revised version of the paper, besides the corrections referred to the minor comments, are listed below.

1. The paper (mostly sections 1 and 2) will be revised in order to specify that the definitions and properties are given by making primary to the inverse problems arising from modelling dynamics of ice-sheets and glaciers, but that the basic concepts apply also to other fields of glaciology.
2. The description of the numerical example will be improved and it will be clarified

its role and significance.

3. The discussion on the diffusion of inverse modelling in glaciological sciences will be supported by some statistical analyses, similar to those listed in table 1. Moreover, we are going to evaluate if it is possible and interesting to prepare a figure which shows the temporal evolution of the weight of inverse-related papers in glaciology and geophysics.
4. We will modify sections 1 and 2 in order to improve the description of the kind of data used in inverse modelling.
5. We will mention “optimal control theory” as a basis for the definition of IP in the text.
6. We will extend the remark on the use of posterior pdfs in the Bayesian framework.
7. 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.

	Geophys*	Glaci*	Hydro*
Paper with inverse-related keywords	2,102	222	6,105
Total number of papers	31,556	31,038	501,400
Ratio	6.7%	0.7%	1.2%

Table 1. Analysis with the Scopus data base

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