Dear Editor,

Pleas find attached a detailed answer to the reviewers' comments. I have addressed all the points and made most of the changes suggested.

The reviewers' comments are printed in blue our changes made to the manuscript are given in green.

Sincerely,

Achim Wirth

Answer to referee # 1 (M. Ghil):

The reviewer's comments are printed in blue our changes made to the manuscript are given in green.

General. This is the third in a series of papers that study the modelling of gravity currents across a hierarchy of models. The previous two dealt, respectively, with the fundamental issues of oceanic gravity currents (Wirth, 2009) and with the estimation of friction parameters in a reduced-gravity, shallow-water equation (SWE) model (Wirth & Verron, 2008). The present paper applies the data assimilation methodology of the second paper to the same SWE model, but uses a more complete and detailed Navier-Stokes model in the Boussinesq approximation as well. The latter model is implemented according to the HARO-MOD formulation of Wirth (2004) and is used to produce the "control" or "nature" runs that need to be approximated as well as possible by the SWE model, given the paucity of actual ocean observations that would be needed to fully determine (or "constrain") the friction parameters of this model.

The paper at hand demonstrates convincingly that the sequential-estimation ("Kalman filtering") approach to parameter estimation is well adapted to produce both qualitative and quantitative results on friction laws in such a fluid model. The key result is that the friction law changes from linear to quadratic at a value of the Ekman-layer-based Reynolds number of about 800, in rough agreement with laboratory experiments (Nikuradse, 1933; Schlichting & Gersten, 2000). The numerical experiments are quite careful and the numerical values of the parameters so obtained are clearly useful.

The paper brings in considerable expertise on relevant literature from widely differing areas: SWE models in rivers (Gerbeau & Perthame, 2001), engineering studies on boundary layers (e.g., Schlichting & Gersten, 2000), and even pseudo-random number generation (Matsumoto & Nishimura, 1998). The constant interplay between linear theory and nonlinear computation makes the results not only more convincing but also more easily understood.

I thank the reviewer of the positive judgement of my paper.

Semi-major comments

1. It is a bit odd to read that Eqns. (11) and (12) describe the ensemble Kalman filter (EnKF). In fact, they describe quite generally any type of Kalman-filter-like sequential estimator and were presented first in the geophysical literature by Ghil et al. (1981), who applied them to a one-dimensional SWE model with rotation. It is only Eq. (13) that provides the particular

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EnKF approximation to the general problem of nonlinear sequential estimation.

Yes, the reviewer is right, I refer to eqs (11) and (12) as KF equations, including the citation, and mention the EnKF only at eq. (13).

2. The paper does mention in passing the results of Sun et al. (2002) and of Kondrashov et al. (2008) using another such approach, the extended Kalman filter (EKF). It would be worth stating explicitly that the model of these authors is a coupled ocean-atmosphere model of a physical and numerical complexity that is at least comparable to the SWE model treated here, and probably quite a bit larger. While there is no finite-time method that can solve completely the nonlinear sequential-estimation problem for partial differential equations, the EKF not only preceded in time but is actually quite competitive with the EnKF both computationally and in terms of accuracy. Furthermore, the EKF results on state-and-parameter estimation of Sun et al. (2002) and of Kondrashov et al. (2008) were first announced by Ghil (1987) and extended to a highly nonlinear problem in solid mechanics by Kao et al. (2006).

The passage is now changed to:

An example is given in a series of papers by Ghil 1997, Sun *et al.* 2002 and Kondrashov et al. 2008, where parameters in an idealised coupled ocean-atmosphere model are estimated using an extended Kalman filter. This methodology was further applied to a highly nonlinear problem in solid mechanics by Kao et al. (2006).

Truly minor comments. These include the presence of a fairly high number of typos and misprints.

Yes, I reread the paper and found a few of the typos. I am not a native English speaker and I have not visited an English speaking country for an extended period in the last 8 years. I am aware of my limited skills of the English language. My actual funding situation does not allow for paying someone to correct the English.

1. The Stull (1988) reference is missing.

Done!

2. The German-and-French-speaking author has no excuse for misspelling "ForschungsCheft"d in Nikuradse (1933) or "permaMnent" in Saint-Venant (1871).

Oups, done!

3. Errors of agreement in number between the noun and verb, and similar grammatical ones, include, in the Abstract alone: last-but-one sentence –"The drag coefficient [...] compare [...]" last sentence – "[...] systematically connection models [...]

Done!

Additional references

1. Ghil, M., S. Cohn, J. Tavantzis, K. Bube, and E. Isaacson, 1981: Applications of estimation theory to numerical weather prediction, in Dynamic Meteorology: Data Assimilation Methods, L. Bengtsson, M. Ghil and E. Klln (Eds.), Springer Verlag, pp. 139Ž013224.

2. Ghil, M., S. Cohn, J. Tavantzis, K. Bube, and E. Isaacson, 1981: Applications of estimation theory to numerical weather prediction, Dynamic Meteorology: Data Assimilation Methods, L. Bengtsson, M. Ghil and E. Klln (Eds.), Springer Verlag, pp. 139–224.

3. Kao, J., D. Flicker, K. Ide and M. Ghil, 2006: Estimating model parameters for an impact-produced shock-wave simulation: Optimal use of partial data with the extended Kalman filter, J. Comput. Phys., 214 (2), 725–737, doi: 10.1016/j.jcp.2005.10.022

Answer to referee # 2 (N.R. Edwards):

The reviewer's comments are printed in blue our changes made to the manuscript are given in green.

General comments. This is the third in a series of papers addressing the important issue of the detailed dynamics of gravity-driven plumes at the bottom boundary of the ocean. Such flows are relatively small-scale and restricted to a few critical locations, very difficult to observe in the ocean or reproduce in the laboratory and challenging to model, but exercise a profound influence on the large-scale flow with implications for global climate. Careful and detailed studies such as this are therefore very welcome. Because of the range of scales involved - the dynamics are driven by small-scale turbulence, but plumes can be coherent over long distances - this is a class of problem where progress demands the application of a hierarchy of models. The two previous papers presented a 2-D non-hydrostatic model of gravity current behaviour and an Ensemble Kalman Filter (ENKF) technique for assimilating observational data into a shallow-water (SW) model of a gravity current. The present paper is the logical conclusion of the study, presenting the application of the assimilation technique to estimate the friction laws acting in the nonhydrostatic (NH) model by fitting the output to the SW model. The paper is therefore interesting as an application of the ENKF to model hierarchies as well as being of direct application to gravity current dynamics. Generally the paper is scientifically clear and thorough and results are well presented. Some questions and comments follow. In particular, the application to model hierarchies beyond this specific case deserves a little more discussion.

I thank the reviewer of the positive judgement of my paper. To my understanding, the application of data assimilation to connecting models in a hierarchy is rather new so the futur will show how this subject evolves. Discussions on applications beyond this specific case are speculative and possibly controversial. So I prefer not to engage in this direction in this paper and ask the reviewer to understand this point of view. (see also comment below).

Specific comments. In applying the ENKF for joint state and parameter estimation, the paper follows the two papers of Annan et al. (2005) and Hargreaves et al. (2004) on the GENIE model, so these might have been referenced. The principal difference is that the GENIE application concerned long-term climate change, for which the critical unknown is the propagation of the struc-

tural error covariance far beyond the domain of any calibration dataset. This is fundamentally different from the weather forecast domain but the present application is arguably intermediate in the sense that the true values of model error are observable in the present in principle but largely unobservable in practice. The climate case puts the emphasis on the estimation of structural model error. The GENIE papers fudge this issue, which is dealt with much more rigorously in the Reification approach to model hierarchies of Goldstein and Rougier (2008). It seems the same has happened here and a perfect model seems to have been assumed, using observational error as a proxy (hence the final collapse of the parameter estimate to a point value in the case without ensemble inflation). In other hierarchy applications neglect of structural error would be terminal. How has that affected the modelling and what are the implications? While the model refinement process, including the extra parameters beta and r, is a very useful and important part of the process, would the explicit inclusion of structural error have allowed for progress without these extensions?

I am grateful to the reviewer to have pointed out the papers by Annan et al. (2005) and Hargreaves et al. (2004) they are now referenced in the paper. I did not reference the paper by Goldstein & Rougier 2009 as I disagree with their conceptual view. More precisely they define a "simulator" as the "sum" of a math. "model" boundary conditions "treatment" and the numerical model "solver". In the introduction of my HDR-thesis (http://www-meom.hmg.inpg.fr/Web/pages-perso/wirth/hdr_achimWirth.pdf) (in french) I clearly show that there is a hierarchical structure between the physical model, the mathematical model (including boundary conditions, which to my point of view can not be separated) and a numerical model and they can not be "summed" as they are conceptually of a different nature. Well, these are more philosophical questions which should be discussed elsewhere (and which I definitely like to discuss (they are a main point in the introduction of my HDR thesis) as they are key to our understanding of science and of how we do science).

I added the following sentence to the introduction:

Parameter estimation using the extended Kalman filter in an intermediate complexity earth system model are given in Hargreaves et al. (2004) and Annan et al. (2005). They adjust the climatology of the ocean and atmosphere model to observed data by estimating $O(10)$ parameters and subsequently perform simulations of climate change scenarios.

It is stated clearly in the manuscript that no inflation was used p176, l 4-8. The non-convergence without including essential aspects of the dynamics and its convergence after inclusion is an interesting feature showing that data assimilation does not do miracles, that is, giving good parameter values with bad models and should not be performed without it. And it furthermore shows, that data assimilation can be a powerful tool to increase our understanding of the physics. So I did not further try to perform data assimilation without the extra parameters.

Of course including a better representation of the model error might have also done the work, but once I found the physical reason of why it did not work and the solution to the problem by including the missing processes, I did not

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consider the problem of improving the structure of the model error rather than improving the model. Other approaches are clearly possible.

Another contrast with the GENIE case is the iterative use of a time series of observations. Does this help or hinder, compared to assimilating a single dataset of time-averaged values?

The experiment is non stationary, the gravity current spreading and descending in time so averages are not well defined.

Section 4. I find it hard to imagine how turbulent the NH model simulations are. Perhaps it would help to show a velocity or vorticity snapshot, or at least to offer some statistics on the proportion of modelled to parameterised momentum flux contributing to the drag forces being analysed.

Yes, the reviewer is right, but showing just a snapshot without a thorough discussion might be very misleading. This turbulent dynamics was the subject of my publication "On the basic structure of oceanic gravity currents" and I really would like to just refer to this paper instead of discussing only part of it. I added to the introduction the sentence:

The reader interested in the detailed description of the data assimilation is invited to consult the latter and the reader eager to know more about the dynamics of the gravity current and its turbulent characteristics should consult the former paper.

Section 5. Similarly at the end of this section, the discussion on disagreement between models and parameter drift could be pinned down quantitatively.

I now added:

Parameter values obtained by assimilating only the first half of the time series differed by less than 20% to assimilations with the total length.

Technical comments.

p. 167 l. 3 Why is the integration time limited in this way if the box is periodic? Is at actually spreading that limits runtime cf p. 175?

Yes, the reviewer is right it is the spreading that limits, however as the upper boundary of the gravity current is almost stationary the descend of the lower part is equal to the spreading. I changed the sentence to:

The time of integration is limited due to the spreading of the gravity current, the upper boundary of the gravity current is almost stationary the lower boundary slides down the incline. The rate of spreading depends on the initial density anomaly.

There are multiple small problems with the English grammar and syntax (eg plurals, incl. data and dynamics). These are too numerous to list, but need attention. Some possible corrections and modifications to clarify the meaning are suggested below:

Yes, I reread the paper and found a few of the typos. I am not a native English speaker and I have not visited an English speaking country for an extended period in the last 8 years. I am aware of my limited skills of the English language. My actual funding situation does not allow for paying someone to correct the English.

p. 163 l. 4 "not [simply] to use data ..."
Done!

p. 163 l. 7 [non-]hydrostatic dynamics?

Done

p. 163 l. 18 "mostly perturbed -¿ perturbed principally" ?

Done

p. 165 l. 15 "an [inclined] rectangular box"?

Done

p. 165 l. 18 define h(x) here, or did I miss it earlier?

Thank you, I added:

The thickness of the gravity current is denoted by the variable $h(x,t)$.

p. 171 l. 19 [measure] typo

????

p. 179 l. 9 what exactly is meant here?

The sentence is now changed to:

When friction processes are considered experimentally, numerically or analytically the friction laws and coefficients depend on the Reynolds or surface Rossby number. The laws and coefficients are than compared in the different experiments and theories.

Appendix

eqn A3 how can this not be a function of ezp(-h/delta), at least before cancellation using beta? And if cancellation via beta occurs, shouldn't A7 look more precisely like A6$\hat{2}$ (I confess I haven't repeated all the algebra myself) eqn A4 missing "=" eqn A6 missing exp(-z) factor??

concerning the first point I now added:

and neglecting terms containing $\exp(-h/delta)$

Errors are now corrected (Thank you !)

References

Annan, J.D., Hargreaves, J.C., Edwards, N.R. and Marsh, R. 2005 Parameter estimation in an intermediate complexity earth system model using an ensemble Kalman filter. Ocean Modelling, Vol. 8, pp. 135-154.

M. Goldstein and J.C. Rougier (2009), Reified Bayesian Modelling and Inference for Physical Systems, Journal of Statistical Planning and Inference, 139(3), 1221-1239.

Hargreaves, J.C., Annan, J.D., Edwards, N.R. and Marsh, R. 2004 An efficient climate forecasting method using an intermediate complexity Earth System Model and the ensemble Kalman filter. Climate Dynamics, Vol. 23, pp. 745-760