

Interactive comment on “Diagnostic and prognostic simulations with a full Stokes model accounting for superimposed ice of Midtre Lovénbreen, Svalbard” by T. Zwinger and J. C. Moore

T. Zwinger and J. C. Moore

thomas.zwinger@csc.fi

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The specific comments will be considered later. We would comment on issues that have not already been addressed in the reply to referee 1:

However the sole impact of superimposed ice on the glacier’s thermodynamics, and hence on its overall dynamics (e.g. velocity field and surface evolution) is not distinguished. This could have been easily done by comparing corresponding results

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from the models with or without superimposed ice.

We explicitly mentioned on page 481: "Initial model runs not accounting for the additional release of latent heat caused by refreezing processes produced unrealistic bedrock temperature distributions in region 2." In our opinion, it would not have contributed a lot to a deeper understanding of the physics to add yet another figure to the already quite intensively illustrated article. The big difference is the distribution of temperate ice, the difference in velocities are less significant, as we do not alter temperatures and consequently viscosity in such a significant way.

The constraint of the model is that it does not include basal sliding, although the presence of warm basal ice (Fig. 5) is likely to cause sliding (p. 480, line 1-4).

The utilized FEM code, Elmer, in principle could account for sliding. It was omitted due to absence of exact information on the physical nature (linear, Weertmann with what exponent?) of sliding as well as the sliding velocities (needed to tune the parameters in the sliding law).

To me, the only major issue to be commented on is unnecessarily long explanation of dip angle. Authors spend about two full pages (p. 489-491) describing the dip angle, which certainly diverts readers' mind (at least mine) from the main theme of the paper. In the context of this paper, a brief description of how well dip angles of modeled isochrones match with the ones obtained from GPR data (Fig. 9), and why this is useful on explaining the age of ice would have been sufficient. Note that, in the abstract of the paper, authors do not include even a single sentence about such a long explanation of dip angle, which reflects its worthlessness.

We must respectfully disagree with the referee's estimation that the age and dip angle

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discussion is worthless. One thing that we wanted to do in this paper as a modeler (TZ) and field glaciologist (JM) was to attempt to use the radar isochrone dip angle to validate model results. The age of surface exposed glacier ice in polythermal and temperate glaciers is a matter both of practical concern (through possible climate proxy data extraction), and of intense curiosity to most if not all field scientists who travel and work on the ice. To our knowledge this dating problem has seldom been tackled, and the experience in this paper shows why – it is a badly posed problem in practice given the numerical limits inherent in modeling. Despite this, the model results match quite well the observed dip angles along the profile in the lower part of the glacier. It may be also worth noting, as we addressed referee 1, that the stake line does not follow a flow line in the lower parts of the glacier. This accounts for the reduced ages with distance seen where the flow line deviates strongly from the stake line. Clearly, we will add some lines on the comparison of isochrone dip angles in the abstract, as suggested.

[Interactive comment on The Cryosphere Discuss.](#), 3, 477, 2009.

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