Review

The paper "Geophysical evidence for soft bed sliding at Jakobshavn Isbræ, West Greenland" by A.E. Block and R.E. Bell is a well-written manuscript on the investigation of newly acquired remote-sensing data. The main focus is on recently acquired air-borne gravity and magnetic field measurements. Gravity anomalies are used to infer subglacial sediment layer thickness, and new conclusions about the rapid flow of Jakobshavn Isbræ are presented. The main conclusions are that Jakobshavn Isbræ rests on a very thick ($> 1000\,\mathrm{m}$) sediment layer, and that the flow of the ice stream is dominated by basal motion, while earlier investigations concluded that the thermal structure is such that internal deformation could account for most of the velocity observed on the surface.

The description of methods and data is clear and good. The discussion and interpretation of the findings, however, should be more thorough. Bold statements are made (very thick sediment layer, very high sliding contribution) for which one would like to see more convincing arguments and a better discussion of alternative explanations.

Overall, the paper would gain from a more careful and convincing discussion. I think that this interesting paper might be ready for publication if most of the points below are addressed.

Sincerely, Martin Lüthi

General comments

Two bold statements are made in this manuscript. In particular, the claim that the inland extension of the Kangia fjord is filled with a sediment layer of 2000 to 3000 m thickness beneath the ice stream needs a more convincing analysis, and a more thorough discussion. If the thickness of this sediment fill were real, this would mean that the fjord is incised to a depth of more than $3000\,\mathrm{m}$ below sea level (Figs. 4, 5, 6), which would make it one of the deepest troughs on earth! Comparisons with the sediment fill of other deeply incised, but now ice-free fjords would help make this result more convincing (e.g. Kangerlussuaq fjord, or fjords in Norway).

Frankly, I have a hard time believing that this fjord was carved into granite to $3000\,\mathrm{m}$ depth below sea level, and later filled with sediments. Are there any alternative explanations for this low-density body? Would a different bedrock density on the North and South sides explain this result? What is the meaning of the big magnetic anomaly North of the ice stream? Is this an indication of a different rock types? Is it possible, that the gravity measurements indicate a fault zone with low-density fault gauge (which maybe is water-saturated)? An easily erodible major fault zone would explain why Jakobshavn Isbræ is located where it is, and why this fjord is longer and deeper than those of most other Greenland outlet glaciers.

The discussion of basal stresses is less clear than the rest of the manuscript. The validity of the simple stress analysis with shape factors is not obvious in a geometry where the top half is essentially infinitely wide. Stress calculations for the Jakobshavn Geometry with a full-Stokes finite element model for a 2D cross-section have been published before (Truffer and Echelmeyer, 2003; Lüthi et al., 2003). Figure 5 in the latter shows that the lateral variation of basal traction is mainly a geometric effect, which is nearly independent of the details of the assumed sliding relation. The basal shear stress τ_b is of the order 1.6 to $2\,\mathrm{bar}$ throughout the domain, with an important stress transfer to the sides, which can extend up to $10\,\mathrm{km}$ from the main trunk.

What value for the parameter A in Glen's flow law was used in the study? This value is crucial to determine the amount of ice deformation for a certain driving stress. On (p. 356, l. 8), temperatures between $0°$ C and $-3°$ C are given. Were these values assumed throughout the ice thickness? If so, why is the (mostly) known thermal structure ignored (Iken et al., 1993; Funk et al., 1994)? As a sidenote, the values for A were rather high in Paterson (1999), but those given in Cuffey and Paterson (2010) seem rather low.

The value of A not only varies due to its dependence on temperature, but also with water content, crystal fabric and grain size. For practical applications, these effects are often lumped together in an "enhancement factor" E in simple shear-dominated deformation.

At drill site D, an E-value between 2 and 3 for ice from the late Wisconsin was inferred (Lüthi et al., 2002), but higher values have been measured at Dye3 and GISP2 (e.g. Dahl-Jensen et al., 1997; Thorsteinsson et al., 1999). Given that up to $50\,\%$ of the ice in the ice stream might be of pre-Holocene origin, and that some of this ice shows enhanced deformation, it seems likely that the (constant?) value of A assumed in this study is too low.

The argumentation above is not meant to disprove the author's conclusion that there is basal motion beneath Jakobshavn Isbræ (it would be surprising if there weren't any). The puzzling fact that Jakobshavn Isbræ shows no clear seasonal velocity variations (except for those linked to the formation and disintegration of a floating terminus), but only reacts episodically to events like the drainage of a major lake, is not discussed or explained in this study.

Specific comments

- The name of the ice stream is not "Jakobshavn", but "Jakobshavn Isbræ". If this is too long, use an abbreviation like "JI". ("Jakobshavn" is the danish name of the town Ilulissat).
- The authors use the term "Ice Bay (IB)" for the area which is known as the Kangia ice fjord. Out of respect for the local culture, and in accordance with the naming adopted in most publications, the "Ice Bay" should be named "Kangia".

p342,l27 The accuracy of the radar in the trough might be much lower than in the surrounding areas, where clear bedrock signals were recorded. Also, the trough is filled with polythermal ice, with a very thick temperate layer, which influences the radar signal velocity.

p346,l2 An ice density of $917\,\mathrm{kg\,m^{-3}}$ seems rather high for cold ice, and might be closer to $900 - 910\,\mathrm{kg\,m^{-3}}$. The temperate ice close to the base might be denser, depending on water content.

p346,l11 Why is only a uniform bedrock assumed? Doesn't the magnetic anomaly hint at potentially different rock types?

p347,l3 "North of" (upper case).

p347,l20 Dense crevasses are not a sign of fast flow, but of large transverse and longitudinal strain rates, possibly quite far upstream of their occurrence. The central parts of the fast-flowing Jakobshavn Isbræ are rather uncrevassed, in contrast to the margins.
Crevasses of $1\,\mathrm{km}$ depth seem rather extreme (some might reach $100\,\mathrm{m}$), but are on the save side for this argument.

p349,l16 Why is a sediment density of $2130\,\mathrm{kg\,m^3}$ assumed? Would sediment always be of the same density, even if it is $2000\,\mathrm{m}$ thick? How would different degrees of water-saturation alter sediment density?

p350,l3 I don't understand this. Why would a low sediment density limit the overestimation of sediment thickness? One would think that low sediment density overestimates sediment thickness, and high sediment density leads to underestimation of sediment thickness.

p351,l21 The fjord is usually choked full with floating big ice bergs. This does not alter total mass within the fjord, but the geometric distribution of the mass, which will affect gravity calculations.

p352,l8 The radar accuracy is correct for most parts of the ice sheet. The errors could be much higher within the trough of Jakobshavn Isbræ, since a thick layer of temperate ice and the complex geometry make the interpretation of the radar signals difficult.

p352,l16 A sediment thickness of several $1000\,\mathrm{m}$ seems fantastic. A $1500\,\mathrm{km}$ sediment fill under Jakobshavn Isbræ would mean, that this fjord reaches not only $1500\,\mathrm{m}$, but $3000\,\mathrm{m}$ below sea level (as shown in figures 4-6). Have similarly thick sediment fills been observed in other deeply incised fjords (e.g. Kangerlussuaq, Norway?).

p353,l7 Basal sediments alone don't lubricate a glacier bed. Highly pressurized water within the sediments is needed for sediment failure, and rapid sediment deformation.

p353,l14 This is for $3 - 8\,\mathrm{kPa}$ shear stress.

p353,l29 Also cite Iken et al. (1993).

p354,l1 There is no "englacial water", since the ice is very cold. The water is either produced by dissipation in the temperate layer close to the bed, or by frictional heat production at the ice-bed interface. Occasionally, water from the surface reaches the bed, either episodically, or more steadily through moulins.

p354,l11 The geometrical effect on geothermal heat has not been considered in these studies, as it is probably not very important. The thick layer of temperate ice within the trough is due to high dissipative heat production when the ice "falls" into the deep subglacial trench, and by molding of the softer temperate ice into the deep trough.

p354,l19 These special circumstances are shown in Truffer and Echelmeyer (2003) and Lüthi et al. (2003).

p354,l20 -l29 This discussion is hard to follow. T_b should be called "driving stress".

p355,l3 What value of the flow law parameter A has been used? This is crucial for the whole discussion. How has the polythermal structure been taken account of? What are the values for F that have been used in this study? Has enhanced deformation within the late Wisconsin ice been taken into account?

p355,l21 The doubling of flow speed at the terminus is mostly a geometric effect due to the disintegration of the floating terminus. Also the surface slope has steepened, such that the basal shear stress must have increased, as compared to the 1980's.

p356,l2 One would like to know how shape factors for elliptic or parabolic channels were selected, given that the top half of the ice is basically infinitely wide. There is stress transfer to the sides, but this is rather non-trivial (Lüthi et al., 2003).

p356,l8 Why are temperatures between -3° C and 0° C considered? The basal ice is at the pressure melting temperature everywhere, so that 0° C is never reached (except at the surface). Is this the assumption made in this study?

p356,l11 I do not understand this. Is the claim, that basal traction exceeds the driving stress? Why do the authors not consider that their chosen (constant?) value of A might be wrong, or variable with depth (due to temperature, water content and ice fabric).

p356,l17 The area in vicinity of the grounding line, and probably many other areas, are under extensive stress conditions. This has an effect on ice viscosity (the ice becomes softer), which should be taken into account when calculating the surface velocity part due to deformation. The shallow ice approximation is not valid in such a situation.

Bibliography Correct the spelling of "Anandakrishnan"(not Anandikrishnan)

Bibliography In times of UTF-encoding, please use the correct spelling "Lüthi" (not Luthi or Luethi).

Fig 1 This figure shows the main data sets used in the study. I think it should be page-wide, or even be split in four individual figures. In any case, the labeling of the axes and of the flight lines is too small at the present scale. Also the contour intervals in all four figures should be labeled with nice numbers (rounded). One could even debate, whether the magnetic anomaly data set should be published here, but it certainly is very interesting.

Fig 2, 3 and 4 please indicate North and South on the profiles. Also add a hint whether dotted lines are observed or calculated.

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