March 25, 2011

Dr. Theresa Diehl
Review of Block & Bell (2011) *The Cryosphere*

General Comments (The overall quality of the discussion paper)
This paper discusses a topic of much interest: the fastest moving glacier in Greenland. Therefore, it is inherently interesting and becomes more so by presenting new data over the glacier. In that respect, it is a very good match for *The Cryosphere*.

However, I feel that the treatment of the gravity processing and gravity residual calculations was too basic, and in places also incorrect (see comments below). The residuals need to be recalculated and the manuscript, unfortunately, majorly changed to accommodate this reviewer's suggestions. Once that is done, I would be happy to review this article again for publication because it is both an important topic and important analysis of the data.

I hope that the authors view this review as a guide to tightening up the manuscript (especially with regards to the treatment of data errors) and correcting a few analysis mistakes.

Specific Comments (Individual scientific questions/issues)
1. p341, Line 11, An additional sentence stating why you use the 2008 grounding line is needed. Is it because no newer one has been estimated since 2008 or because the 2008 grounding line was the only one available at the time of this work?

2. p341, Line 15, Please add a couple of key citations to support the statement that "research over the past 2 decades has shown the trough to be central to understanding the flow of Jakobshavn".

3. p361, Figure 1 caption. For (A) please state that the data is ice-penetrating radar but that the map shows subglacial topography, as was stated already in section 2.3 of the text. (B) Are the free-air gravity anomalies the combination of both datasets (2008 and 2010?) (C) Same question as in part B. Also, what is the colored background image? Is that LANDSAT and what is the colorbar/units of the image? Please state this at the beginning of the caption. Also, I'd like to see this figure be much larger than it is.

4. p.341, Lines 25-26. What altitude were the lines flown at? The altitude controls the minimum size of features that can be recovered from perfect and well-sampled gravity measurements. (The actual minimum size recovered depends on the flight speed, data processing, filtering, and line spacing… as you discuss in following sentences).

5. p342, Line 7. Much more detail of the gravity processing is needed here. What software package/methods were used to process the data? Who processed the data? Please cite or acknowledge those who did this work, if it was not the authors. What datums are the data referenced to (WGS84 and ITRF00 or other datums) and do those match the datums of the topography data sets? What filter type was used at 70s? Is there a roll-off associated with this

filter? The type of filter and parameters used says a lot about the character of any ringing or bleed-in from suppressed (but not necessarily eliminated) frequencies in the final gravity data.

No data error is reported. A standard evaluation of internal consistency for airborne gravity data is the calculation of crossover errors. However, no cross lines were flown during either the 2008 or 2010 surveys. Some estimate of data error is needed. Were any repeat lines flown? At very least, the laboratory resolution of the instrument (in mGal) could be reported and acknowledged that the data error will be at least a magnitude higher than that (in my experience this has been true with zero-length spring gravimeters, though Sander Geophysics could provide a better estimate of data error due to the instrument and flying conditions).

6. p. 341-342. Airborne gravity quality is very dependent on the GPS used to position the flights. Please add a paragraph on the GPS units, GPS processing (kinematic or PPP and software type), and a measure of the GPS positions' quality for both the Twin Otter and P-3 flights.

7. p. 342, Line 23. This last sentence of the paragraph is a bit vague. It would be helpful to add here that the "additional constraints" of the OIB lines are to constrain ___ and ___ methods used later in the paper.

8. p. 343, Magnetic Section. As I am not a magnetic specialist, I could not comment on section 2.2, though it seemed thorough to me as a non-specialist.

9. p. 343-344. Excellent descriptions of the bedrock and surface topography sources, including their errors.

10. p. 345-346. Comments on 2-D models: GM-SYS is a very good tool for 2-D modeling, when working in an area that can be approximately accurately in 2-D. The narrow, slightly winding trough of Jakobshavn is close to 2-D but the area where the trough bends to the south (T0 to T14) and then north again (T29) will induce some error in the calculated gravity due to the 3-D change in geometry, as will the changing depths of ice and trough along the length of the glacier. I'd suggest that using a 3-D program to calculate the gravity effect of topography (i.e. Bouguer correction) as a more robust method than calculating 2-D profiles and then gridding them. For my dissertation, I tried both 2-D and 3-D methods of calculating the gravity correction due to topography (ice surface and basal) of Thwaites Glacier in Antarctica and I found the 3-D method to be much more accurate. I would be willing to share the code I have, written by researchers at Ohio State and the methods presented in von Frese (1981, "Spherical earth gravity and magnetic anomaly modeling by Gauss-Legendre quadrature integration." *J Geophys* **49**, p.234-242). If a recalculation in 3-D is not done, then the errors in the 2-D method should be acknowledged and subsequent interpretations of the gravity acknowledged to have more accumulated error than the free-air measurements. Interpretations of the residuals can't be given with absolute certainty because of unknown error from the topographic correction.

Also, there is no correction for seawater, although bathymetry was available in the topography models. Within ~15 km (rule-of-thumb) of the grounding line, neglecting the water correction (which seawater is ~100 kg/m^3 more dense than ice) will produce a very slightly increasing error in the gravity calculations as the lines get closer to the sea. The gravity residuals will be when neglecting seawater are more positive near the grounding line than they should be (having not subtracted enough mass from that area), making any estimates of sediment thickness in this region slightly too shallow. This should be stated in the article.

11. p. 346, Lines 21-22. This important paragraph and the following calculations of the gravity residual are *mostly incorrect*. The following sentence is correct: "The residual gravity anomalies are defined by subtracting the 2-layer modeled gravity anomalies from observed free-air anomalies." If the authors did indeed calculate residual=observed – modeled, then the results in the trough for all cross-sections shown in Figure 2 would produce negative gravity residuals. That's because all cross-sections show a "more negative" gravity anomaly and a "less negative" modeled gravity (e.g. eyeballed for T0: residual= -100 mGal – (-90 mGal) = -10 mGal). Instead, the residuals presented in Figure 1D and in the text have the opposite sign as expected for the residual, likely due to accidentally subtracting model minus observed. The magnitudes of the residuals will not change, but the sign should be opposite what is presented in the paper. This must be fixed prior to publication.

12. Line 21-22. This statement is incorrect for the standard calculation of residual=observed-modeled. It should say: "A positive residual gravity value can indicate an un-modeled HIGH density body, and a negative residual gravity value can indicate that the model's density was too high and there is actually an un-modeled LOW density body."

Being that these gravity residuals are a keystone of this article, this mistake must be addressed before final publication. And, the whole text must be changed to reflect the inversion of positive and negative anomalies as referred to in the subsequent sections. From here until the end of this review, I will refrain from mentioning all the times that the sign of the residuals are incorrect and instead concentrate on evaluating the rest of the methods and results.

13. p. 347, Line 21. The sign of the described residuals from air-filled and water-filled crevasses is correct. This was a very interesting section! Except that the conclusion must be modified to account for the sign mistake of the residuals. Also good was the discussion of the distributed till gravity effect.

14. p.348, Line 21-22. Did you low-pass filter the gravity residuals before making the comparison to the modeled crustal signal? A comparison by eye can often be misleading when other large, medium-wavelength gravity features (like the sediment-filled trough) are present. Also, if the gravity tie for these data (which is not discussed in this paper) is at all in error, then the modeled crustal gravity could only be compared with the long-wavelength residuals for its shape and relative magnitude, not for the absolute magnitude of the features.

15. p.348, Line 28. This brings up a good point. Be careful interpreting any residual less than 5-10 mGals, since they are likely to be in the range of error for that data… although a more rigorous study of errors should be added to this paper (as suggested in above comments). But I agree that the residuals look like bodies at or just below the ice-rock interface.

16. p. 349, Line 22. Fitting to 1mGal is probably too precise for the amount of error that I'd estimate is in the data. More appropriate would be to calculate error and then report a range of

sediment thicknesses possible by fitting the range of gravity residuals +/- their error. Also, a regional gravity signal should have been removed from the data before sediment modeling because the models assume that all the residual gravity is the result of sediment in the trough, which is likely to be untrue due to heterogeneous rock densities nearby.

At this point, I will not comment on the results of the modeling or discussion because the data need to be reanalyzed in a second version of this paper and the other referees (with more glaciology experience) have already commented heavily on these sections.

Technical Corrections (Typographical or other errors)
1. p340, Line 19, add a hyphen “seismically-detected”
2. p341. Line 5, add a hyphen “15 km-long”
3. p.341, Line 8, make “Side” lowercase
4. p. 347 and onward, add hyphens for “water-filled” and “air-filled” crevasses
5. p. 348, Line 8, misspelled “centered”
5. p. 348, Line 11, add hyphen “seismically-detected”