Comments on "From Doktor Kurowski's Schneegrenze to our modern glacier equilibrium line altitude (ELA)", by R.J. Braithwaite, submitted to *The Cryosphere Discussions*

Graham Cogley, July 2015

General Comments

This paper traces the concept of the equilibrum line altitude (ELA) from its origins in the work of Kurowski in the late 19th century to contemporary versions of it and of ways of exploiting it. There was considerable interest in the "snowline" at the time of Kurowski's work, and some of the variant interpretations have survived to the present, but it is Kurowski's presentation that has been the most fruitful and practical. The bulk of the paper is devoted to an analysis, based on modern measurements, of relationships beween the ELA and the mass balance. Kurowski's theoretically-derived mean altitude is found, in a sample of moderate size that represents most of the information that is available in practice today, to be systematically a few decametres above the balanced-budget ELA.

I enjoyed reading this paper very much. The history of our subject is intrinsically worth more attention than it gets, but I also found the exploration of the relationship between Kurowski's mean altitude and the balanced-budget ELA absorbing and valuable. My favourite part was the discussion of the balance ratio and the implications of the finding that the balance gradient is usually steeper in the ablation zone than in the accumulation zone. This led me (see comment at P3183 L7 below) to cover some ground that the author has no doubt already covered himself, not to mention Kurowski, but I think it is clear that the outcome of his study is a set of questions that are well worth future attention.

The paper is very well written, although not free of typos and other slips, and I recommend that it be published subject to consideration by the author of the points raised below.

Substantive Comments

P3168

L25 A brief definition of "baseline" as a qualifier of "ELA" would be helpful.

P3171

L12-13 "Modern" mass-balance terminology is as in the *Glossary of Glacier Mass Balance and Related Terms* (http://unesdoc.unesco.org/images/0019/001925/192525e.pdf), to which the author himself contributed, so his citation of Anonymous (1969) is a little surprising. Indeed at L19-25 the discussion of "balanced-budget" and "steady-state", not discussed at length by Anonymous or Meier, could have been taken straight from the *Glossary*. However the notation later in the manuscript does not conflict with the few revisions of Anonymous in the *Glossary*, so this oddity is not of the first importance.

P3172

L15,18 I think the author's earlier interactive comment on b_{it} can be refined a little further. There is no reason in principle why Kurowski's idea cannot be made a function of time. The point, though, is that Kurowski's thinking was dominated by the assumption of what we now call a balanced budget, so that the subscript t is anachronistic.

L19 For the removal of doubt, k could perhaps be defined here algebraically (as db/dh).

L20-25 This rejection of "steady-state" could be made more emphatic. In modern terminology (and for that matter in Kurowski's thinking had he gone to such lengths), steady state is a quite different idea from that of a balanced budget. See the *Glossary*, for example, in addition to the helpful references to Braithwaite and Müller and Radok. The distinction deserves to be emphasized because it is subtle.

P3175

L20-21 Dyurgerov (2010) and perhaps Cogley (2009; whole-glacier annual balances only) should also be cited here, with the Dyurgerov citation perhaps replacing the two that are there now.

Cogley, J.G., 2009, Geodetic and direct mass-balance measurements: comparison and joint analysis, *Annals of Glaciology*, **50**(50), 96-100.
Dyurgerov, M.B., 2010, Reanalysis of glacier changes: from the IGY to the IPY, 1960-2008, *Materialy Glyatsiologicheskikh Issledovanij*, **108**, 5-116.

L27 Mention whether the data are annual or seasonal.

P3178

L23-27 The most striking illustration known to me of the difficulty of securing "representativeness" is Figure 4a of Paul and Haeberli (2008), presenting geodetic elevation changes for 786 Swiss glaciers over 15 years.

Paul, F., and W. Haeberli, 2008, Spatial variability of glacier elevation changes in the Swiss Alps obtained from two digital elevation models, *Geophysical Research Letters*, **35**(21), L21502.

P3180

L6-7 ELA data to accompany mass-balance data are one thing, but hypsometry is another. Version 4.0 of the Randolph Glacier Inventory (http://glims.org/RGI/index.html) provides hypsometry for nearly 200,000 glaciers. In version 5.0, to be released imminently, the number of glaciers will reach nearly 210,000 and 167 of them will be linked to the Fluctuations of Glaciers mass-balance tables through their WGMSIds (with further additions intended in a future version).

P3181

L1-3 The text seems to have become garbled here. My interpretation is that "correlation coefficient between ELA and" should be deleted, and "referred to in Sect. 5" should be "(see Sect. 5)". But I still do not understand why this "[justifies] the inclusion …".

L27 This and the next sentence are in need of clarification. First, the text needs to say that the quoted means and standard deviations are of the difference $\mathrm{ELA}_0 - H_{\mathrm{mean}}$. Second, this quantity has already been mentioned four times (by my reckoning, beginning at P3180 L28), and the case for giving it a symbol of its own seems strong. Finally the case should be made for quoting the standard deviation and not the standard error of the mean, so that the reader can understand why for example the range ±42 m, which includes 0, nevertheless makes the mean –40 m a "significant" overestimate of ELA_0 by H_{mean}. I imagine that the two quantities have been assumed to be independent, and that the standard error has been calculated; in this case it is 42 m divided by $\sqrt{n} = 4.4$, or 9.5 m, so that a statement of the difference with 95% confidence would be nearly –-40±19 m.

P3183

L7 Eq 10 lacks the slash that should separate its numerator from its denominator!

The balance ratio represents a significant advance over what Kurowski was able to achieve in the absence of balance measurements. But of necessity the ratio has to be calculated from the measurements, and even when seasonal balances are available for the entire elevation range they do not capture the more fundamental control on the shape of the $b_a(h)$ curve. That is, the annual balance b_a is the sum of the annual accumulation c_a and the annual ablation a_a (the former non-negative and the latter non-positive).

There are no measurements of these two quantities, but there are solid reasons for hypothesizing that they are two independent linear functions of elevation. The evidence and the physical reasoning are strong for annual ablation, but less strong for annual accumulation because we have to reckon with the possibiity of the air mass being squeezed dry before it is lofted to the crest of the mountain range; of sublimation and condensation becoming dominant terms at the highest elevations; and perhaps of accumulation being reduced at lower elevations if some precipitation falls as rain (the rain-to-precipitation ratio will decrease with elevation).

Setting these complications aside, and further assuming that there is no dry snow zone (where $a_a = 0$), the profile of the sum of annual accumulation and annual ablation will be a third straight line (whether the ELA lies below or above h_{max}).

Why, then, is there such abundant evidence in the literature that the profile of b_a is a dog-legged curve with an inflection in the neighbourhood of the ELA? The accumulation gradient seems in general to be shallower than the ablation gradient, but this does not answer the question. It seems that the answer must lie in the neglect of the complications, which must compromise the reasoning above – leading to deviations from linearity of $c_a(h)$ or $a_a(h)$ or both. The author makes a worthwhile start on this problem in his discussion (P3184 L12ff.), but there is clearly much more work to be done. It may be that insights can be found in simulations of the mass balance, such as those of Huss et al. (2009).

Huss, M., A. Bauder and M. Funk, 2009, Homogenization of long-term mass-balance time series, *Annals of Glaciology*, **50**(50), 198–206.

Stylistic Comments

P3165	
Header	Most of us do not know what "SEED" stands for, so spell it out (as in the Acknowledgements).
P3166	
L12	"areas".
L23	Omitting the definite article before "snow line" and several similar terms is obviously a deliberate choice, because it occurs hundreds of times. However I found it very distracting, and more distracting the more I saw of it. It is contrary to everyday usage in scientific English, and I do not think it serves any good purpose.
P3169	
L2	A semicolon or full stop after "misquoted" would be better.
L15	Perhaps add "of equal area" after "pixels".
L18	Italicize "*N*".
P3171	
L6	No need to capitalize "century".
P3173	
L1	"over the whole elevation range of the glacier".
L10	Comma before "the mean". Or simply delete the repetitive definition of $\bar{h}$.
L17	Remove the dot before "w.e.".
P3174	
L5	"150 m height" would be preferable to "150 m width".
L10	"Aletschgletscher".
L16	Delete the unnecessary "due to climate change".
P3175	
L22	"1990s".
P3176	
L10	The parentheses are unnecessary.
L3177	
L1	Mention the number of years.
P3180	
L14	"dependent".
L26	Presumably "E_0" should be "ELA_0".
P3181	
L4	"Goldbergkees".
P3183	
L15	Why is Rea referred to here and in several other places as "Brice Rea"?

L21	"Soruco", but as noted in the author's earlier comment the reference is not in the bibiography.
L23	Perhaps "validates" rather than "confirms".
P3184	
L7	"ratios".
L19-20	"960±405 m and 978±499 m respectively (confidence limits are standard deviations)".
L22-23	"might improve the detectability of contrast … zones by enough to".
P3185	
L3	Why is "precipitation" preceded by the adjective "meteorological"? I cannot think of any other kind.
L15	"ELAs".
L26	"95%".
P3186	
L19	"Ötztal".
P3189	
L29	It may not survive copy-editing, but the author's corrected journal title for Kurowski 1891 should be *Pencks Geographische Abhandlungen*. There is no final *r* in *Geographische*, and the German possessive noun has no apostrophe (a point that is moot because *Pencks* is just a convention; the formal title does not actually have Penck's name in it, so we do not know whether or not to anglicize his name).
P3192	
Table 1	"H_{mid}". "Balanced-budget"".
P3195	
Fig 2	"Cumulative hypsographic".
P3197	
Fig 4	"Student's *t*". Same in the captions of Figures 7 and 9-11.
P3198	
Fig 5	Insert "as the sample of *X* glaciers" after "standard deviation". But what is the point of plotting the Gaussian curve? Under certain constraints that probably apply here, the distribution of sample correlations is described by Fisher's *z*. (I last thought about this when I published Cogley, J.G., 1999, Effective sample size for glacier mass balance, *Geografiska Annaler*, **81A**(4), 497-507; see equations 2 and 3 therein.)
P3200	
Fig 7	*Y* is not defined, but I think it should be ELA_0. The numerator of the expression needs a minus sign: "($ELA_0 - H_{min}$)".

Supplementary Information

P0	I applaud the provision of an accessible version of Kurowski's long paper.