Progress in Physical Geography 27,1 (2003) pp. 107–111

Classics in physical geography revisited

Figure 1 Barclay Kamb (left) and Ed LaChapelle (right) working in the Blue Glacier ice fall on Mt Olympus, Washington, in 1967. Kamb is making borehole observations in connection with a tunnel excavated into the icefall below. LaChapelle is holding a stick of dynamite in preparation for blasting down a serac that threatens the mouth of the tunnel (this is a later tunnel than the one from which Kamb and LaChapelle obtained the results in the paper under discussion)
Source: Photographs and caption supplied by Barclay Kamb

Kamb, B. and **LaChapelle, E.** 1964: Direct observation of the mechanism of glacier sliding over bedrock. *Journal of Glaciology* 5 (38), 159–72.

In an age when more is published than can possibly ever be read, and where quality is often measured in terms of rounds fired rather than targets hit, it was refreshing, in preparing this review, to re-read a paper that was not only a seminal contribution to its field, but also a piece of work of genuinely high quality.

I first encountered Kamb and LaChapelle's paper 20 years after it was published. Starting my PhD research in glaciology from a position of total ignorance, I was given an emergency introduction that consisted of a trip to Greenland, a couple of indispensable textbooks (Sugden and John, 1976; Paterson, 1981) and a handful of key papers divided into 'recent work' and 'classics'. Kamb and LaChapelle was at the top of that

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final pile and, two decades further on, it retains its place at the front of the file of classics that I now recommend to students of my own.

Beyond the fact that it was written long before the current crop of young glaciologists had their first school geography lessons, this paper can claim classic status on several grounds. First, Kamb and LaChapelle made new and important contributions that both influenced contemporary debates and remained significant for future generations of researchers. Secondly, they combined theory, field observation and experimentation in research of elegantly logical design and execution. Thirdly, both the science and the paper itself stand as paragons of clarity, detail and transparency that put many of us to shame. Modern glaciologists and geomorphologists with an interest in subglacial processes still read this paper because of its original findings. They should also read it to learn from its style and approach.

I Big news

In the early 1960s, when Kamb and LaChapelle carried out this work, the mechanisms of glacier sliding over bedrock were imperfectly understood. Direct observations of the glacier bed were limited, and theoretical models of the mechanisms of motion remained largely untested. The most immediately apparent of Kamb and LaChapelle's contributions was to provide the first detailed observations of processes at the ice–bed interface, to relate those observations to prior theory, and to produce a new model of the sliding process based on a combination of theory, field observations and new experiments. The paper includes measurements and observations that proved fundamental to subsequent developments both in the theory of glacier dynamics and in the understanding of basal ice formation and debris entrainment.

The primary focus of the paper was the testing and development of Weertman's (1957) theory of the basal slip mechanism. Weertman had proposed that motion occurred by a combination of regelation slip and plastic flow. Regelation slip accommodated motion around small obstacles and roughness at the bed, while plastic flow accommodated motion around larger obstacles. An intermediate 'controlling obstacle size' where motion was accommodated equally (or equally poorly) by regelation and plastic flow was the rate-limiting control on basal motion. To examine the theory, Kamb and LaChapelle excavated a tunnel to the bed of Blue Glacier, Washington, and actually observed these processes in action. They observed the regelation of ice around bedrock obstacles and the formation of a regelation layer that incorporated debris particles from the bed. They observed plastic deformation of the basal ice in warping of foliation planes and of the regelation layer. They measured ice velocity at the bed, at the glacier surface and in a vertical profile at 10-cm intervals through the basal layer of the glacier. This was a feast of new data about basal processes.

II Comprehensive science

They did not stop there. Nowadays, it often seems that one small idea or observation is enough to justify a handful of very similar papers published by the same authors in different journals. Kamb and LaChapelle's paper reflects a different ethos. Here the research is carried right through all of its logical stages, and the results published together as a single comprehensive paper. Additional material from the project was published elsewhere, but this paper stood as a complete and self-contained account treating a single research question from start to finish. In addition to making ground-breaking new observations at the glacier bed, Kamb and LaChapelle compared these observations with existing theory. Finding that their observations did not quite match what theory predicted, they re-wrote the theory. To do so, they had to engage in a third line of research – laboratory experimentation. Having through their field observations introduced to glaciology the concept of the regelation layer (now a fundamental topic in any introductory class in Glaciology), Kamb and LaChapelle tested the concept by reproducing the regelation process experimentally. They then applied the outcomes of their experiments to Weertman's previous theoretical treatment of the process to demonstrate that, although it was qualitatively correct, it was quantitatively inapplicable. Kamb and LaChapelle demonstrated that the 'controlling obstacle size' suggested by Weertman should in fact be a 'controlling obstacle spacing'. In their terms: 'the natural distance scale for transition from regelation slip to plastic slip expresses itself basically in terms of the wavelength of the irregularities rather than in terms of their amplitude'. Important though this is for glaciology, the exemplary thing about their paper is the way in which this suggested change to existing theory was founded in a tightly interconnected set of theoretical analyses, field observations and laboratory experiments.

III Doing it right

An outstanding characteristic of Kamb and LaChapelle's paper is its clarity. This applies not only to the lucidity and accessibility of their text, but also to the transparency and logic of their scientific approach.

It is salutary to compare the way Kamb and LaChapelle challenged and developed contemporary theory with the way modern research is pursuing one of today's hot subglacial topics. Observations at the Matanuska Glacier (e.g., Lawson *et al*., 1998) and elsewhere have suggested that subglacial water flowing upwards towards the margin from a subglacial overdeepening may become supercooled, and may freeze to produce substantial layers of debris-rich basal ice even in temperate environments. This is an important theory, and will probably be very important in our developing understanding of subglacial processes. However, it has engendered a major controversy in the discipline. Some investigators have embraced the theory comprehensively, even to the point of arguing that because the mechanism is theoretically possible, and because field observations are consistent with it, the supercooling mechanism can now replace all other mechanisms in our interpretation of basal ice sequences. Others (e.g., Spedding and Evans, 2002) have argued that such certainty is premature, and that other explanations of observed phenomena remain tenable. What has not been forthcoming is a straightforward testing of the applicability of the supercooling theory along the lines of Kamb and LaChapelle's testing of Weertman's theory. Supercooling research has blundered directly from a good idea to its thoughtless and arbitrary application, missing out the critical steps that define good science and that are exemplified by Kamb and LaChapelle's work. Fundamentals of research design and scientific propriety are routinely flouted by new authors in glaciology and glacial geomorphology, and the referees employed by journals are not doing enough to enforce good practice. Case studies of good science, such as Kamb and LaChapelle's work, should be compulsory reading for all new researchers in glaciology.

Even for the newest researcher, Kamb and LaChapelle would not be difficult reading. The paper is clearly organized and written in an accessible style. This is evident from the outset, with an abstract that synthesizes the essentials of the paper succinctly yet completely. The paper itself uses extended sets of numbered points for the uncomplicated reporting field and laboratory observations. The interpretation of these observations is both thorough and self-critical. Limitations and uncertainties are recognized, and the conclusions clearly grounded in this context. Here are no outrageous claims, no wild speculation, no unfounded inference from limited data. Here are conclusions logically derived from transparent and carefully grounded observations. Here is science at its best.

Figure 2 Barclay Kamb (upper right) and Ed LaChapelle (left, at middle height) with a couple of participants in the excavation of the Blue Glacier 1967 tunnel. The excavation team numbered 12 people. Despite the tunnel darkness, all faces are heavily roasted by the sun. The picture was taken in the Snow Dome hut on Mt Olympus
Source: Photograph and caption supplied by Barclay Kamb

IV A lasting impact

One difference between a paper that is a classic and a paper that is just old is that, for a classic, the ground-breaking contribution that made it valuable when it was first published remains relevant to a modern audience. Eighteenth-century work on the supposed mechanism of glacier flow by 'dilatation' is old and it is interesting, but, however good the work may have been in its day, it lacks much relevance to modern glaciology. Kamb and LaChapelle's work is not yet as old as the dilatation theory, but its relevance remains undimmed. Processes at the glacier bed remain a key area of research in glaciology and at the interface between glaciology, geomorphology and glacial geology. Ice-sheet models still incorporate only imperfect representations of complex basal systems. The relationship between basal processes, sediment entrainment and the characteristics of debris-rich basal ice remain incompletely understood, as illustrated by the supercooling debate. Our interest in the glacier bed now explicitly incorporates environments other than bedrock alone, but the mechanisms of glacier sliding over bedrock, and the processes of regelation and plastic deformation, are still important topics. The simplest testament to Kamb and LaChapelle's work is that their observations continue to be cited routinely in new research nearly 40 years after they were first published. This is an important paper, and it is a good one.

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