Reply to the comments by G.S. Boulton, K.E. Dobbie, S. Zatsepin on: Deforming soft beds under ice sheets: how extensive were they?

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Below we reply to specific points raised by Boulton et al. (2001, pp. 11–13) in their critique of our paper (Piotrowski et al., 2001, same volume on glacier deforming-bed processes, Guest Editors J.K. Hart and J. Rose). Before doing so, however, we would like to highlight the unusual circumstances under which Boulton et al.’s comments on our unpublished manuscript were included in their paper. It is a matter of professional courtesy that manuscripts submitted for publication are considered the property of the authors until publication. Hence, others should not use information from submitted manuscripts without permission of the authors. In this case, Boulton et al. had access to our manuscript without our knowledge and wrote what amounts to a point-by-point comment on it in their paper. We were not given access to Boulton et al.’s manuscript, nor given an opportunity to reply to their comments directly. This preferential treatment violates the confidentiality of manuscript processing and represents a threat to the integrity of our peer review system.

The purpose of our paper was to point out that widespread application of the subglacially deforming-bed model to Pleistocene glaciations should not be done without critical evaluation of evidence on case-by-case basis. In our paper, we gave examples of geologic evidence that is, in our opinion, incompatible with such application. However, we emphasized (Piotrowski et al., 2001, p. 140) that we accept bed deformation as a geologic process. What we are questioning is its relative importance in the transport and deposition of many Pleistocene age tills, and its significance as a generally applicable mechanism of glacier movement.

In this reply, we focus on the critique of our arguments and reasoning by Boulton et al. (2001). The letters below refer to sections in Boulton et al. (2001, pp. 9–11).

(a) In their paper, Boulton et al. stated that we have made the following claim: “The downward increase in shear strain observed in subglacial experiments should be reflected in a downward transition from highly deformed to undeformed tills.” This is a misquote. What we discussed is a downward decrease in shear strain (Piotrowski et al., 2001, p. 140), consistent with the field observations we cite (Piotrowski et al., 2001, p. 140), and with existing laboratory experiments (e.g. Iverson et al., 1996, 1997; Tulaczyk, 1999; Hooyer and Iverson, 2000a, b; Iverson and Iverson, 2001). We still dispute the assertion of Boulton et al. that sharp, undeformed basal contacts should be typical of deformation tills, with till having protected the substratum. This idea seems inconsistent with one of the key assumptions of the deforming-bed concept, which invokes deformational incorporation of sub-till strata as the major process of till formation and regeneration. The idea is also inconsistent with observational studies suggesting that material can be mobilized into glacial transport from the substrate under the deforming till (e.g. Menzies, 1990). Boulton et al. postulate that deformed sediments are more likely to occur under melt-out tills because they were directly overridden by glacier sole so that stresses could be transmitted to the underlying sediment. This suggestion is restricted to situations where basal water pressure is below the flotation point but, as shown by modern glacier studies
and for areas in North America and Europe (e.g. Brown et al., 1987; Piotrowski and Tulaczyk, 1999), basal decoupling preventing stress transmission is common.

We note that if “the till in an exposure may reflect the progressive accumulation of deforming till” as Boulton et al. postulate, then the thickness of the deforming bed at any point in time is limited to slices much thinner than the total till thickness. This corresponds to our concept of temporarily and spatially transient deforming spots presented at the INQUA congress in Durban (see Piotrowski and Kraus, 1997; Piotrowski et al., 1999). It also represents a retreat from many previous estimates of the depth of till deformation beneath ice sheets, which envisioned several meters thick zones of subglacial deformation being active at any given time. This modification is very much in line with the main point of our paper, which was written to highlight the danger to overestimating the role of subglacial deformation and not to completely deny that such deformation takes place.

Boulton et al. (2001, pp. 8 and 11) disagree with our use of particle diffusion during till deformation, (based on a model of Weertman, 1968), to put an upper limit on the amount of strain experienced by tills. To substantiate their point of view they quote Ehlers and Stephan (1979) who found slickensides and ribs at the base of tills, yet the interface was very sharp. This is a circular argument, based on the unsubstantiated assumption that the slickensides and related features originated within the deforming bed. Such features, however, can have equally been well formed directly at the interface beneath a moving debris-rich ice and the substratum due to ploughing (e.g. Brown et al., 1987; Tulaczyk, 1999, 2001) and later preserved by melt-out. Recent work of Hooyer and Iverson (2000b) has clearly demonstrated that diffusion of particles in deforming till is a reality and that it can be used to constrain strain magnitude in tills.

(b) Our idea that blocks of soft sediment should have smeared contacts with deformation till matrix is rejected by Boulton et al. by reference to the observations of Ehlers and Stephan (1979) and the glacial drifts of Norfolk, East England. Using the Ehlers and Stephan data as evidence of deforming till it is inconsistent with the interpretation by Ehlers and Stephan of their own data. These authors believed that the vast majority of their structures are formed by stones carried on the underside of the ice (pp. 349, 353, 354), consistent with ploughing at the base of debris rich ice that later melted out. Furthermore, they say “in these cases and where a deformation till appears between the actual basal till and the underlying sediments, no useful furrows or grooves (“ribs” at the till base) can be expected to be found” (p. 355). With regard to the intra-till slickensides at Heiligenhafen it should be noted that the displacement was very small (cm-dm scale; Piotrowski, 1997a,b; Kristiansen, in prep.) limiting the particle diffusion. Good examples of a transitional contact between the till and the substratum, as we would predict for a deforming-bed environment, are given in Stephan and Ehlers (1983, p. 224, referred to as zone of intensive shearing) and Piotrowski and Wysota (2001, p. 61).

The Norfolk drift is as controversial as it is well known, with some workers suggesting its origin to be glaciotectonic (e.g. Hart et al., 1990) while others have suggested a waterlain origin (e.g. Eyles et al., 1989) or a combination of these mechanisms (Lunkka, 1994). Clearly, much of the sequence is deformed, but the rate of deformation differs greatly from place to place. Equally diverse are sediment contacts ranging from sharp to smeared and diffuse (as examined e.g. in October 1995 at the field meeting of the 4th Subglacial Processes Workgroup), and we suggest that this may reflect different strains to which the sediment was subjected. Contrary to the assertion of Boulton et al. (p. 11) we are certainly not suggesting these are melt-out tills.

The end member of penetrative deformation of heterogeneous soft sediment is a homogenized diamicton. An important mechanism to produce a massive diamicton consisting of intermixed particles derived from different source areas is particle diffusion occurring at different scales, e.g. due to grains swirling around rotating cores (van der Meer, 1997) or due to circular motion of particles in a deforming till (Thorsteinsson and Raymond, 2000). We consider smeared contacts between lithologically different sediments to be one snapshot in the process of sediment homogenization, again dependent, among other factors, on the strain rate.

(c) Boulton et al. argue against our skepticism about preservation of fragile materials such as shells and weathered rocks in deforming tills and note that “whether grain crushing occurs or not will depend upon effective stresses in a material”. In modern glaciers subglacial water pressure fluctuates greatly, and it is likely that effective stress in the sediment varies accordingly, influencing the potential for crushing. It is thus tempting to envisage areas in the deforming bed where fragile materials can survive intact. While low effective stresses may occur for short transport distances and short time periods, it is somewhat surprising to postulate that wherever there are fragile weathered rocks or shells preserved, these were never exposed to high effective stresses over time scales and transport distances of entire glacial cycles. This is because the frequency of subglacial water pressure fluctuations is several orders of magnitude higher than a glacial advance/retreat cycle.

Preservation of fragile materials in the allegedly widespread, continental-scale deforming beds is thus unrealistic, and we consequently suggest that wherever such materials occur, strain was low and spatially
restricted. Finally, Boulton et al. argue that delicate shells cannot be preserved in melt-out tills because of high cryostatic pressures (>10^6 kPa) they were subjected to. We reject this argument because it assumes no water pressure elevation during melt-out. Boulton et al. envision high pore water pressure and low effective stress during deformation but fail to recognize the fact that subglacial melt-out can be associated with such conditions as well.

Boulton et al. state that effective stress levels should be low enough in deforming tills to preserve fragile materials. Yet, previously, he and other proponents of all-encompassing deforming beds have argued that till deformation requires high sediment flux rates and high erosion rates (of the order of 1 mm/yr), even over bedrock. There is a high level of contradiction in claiming that effective stress levels in deforming tills are low enough to preserve crumbly, weathered clasts while the same weak (under low effective stress) till can cut into sub-till materials stronger than this fragile clast at rates of ~1 mm/yr. Such contradictions are bound to arise when one attempts to explain all of the complexity of subglacial tills with a single subglacial process, while discounting a priori the possibility of a number of such processes having different relative importance in determining till composition and properties.

(d) Our argument that clast rotation is an important criterion in identifying subglacial deformation is rejected by Boulton et al. because they do not find a physically based theory to explain the process. We refer to a vast body of literature that describes rotation as important attribute of subglacial sediment deformation, e.g. Hart and Boulton (1991), van der Meer (1993), Menzies (2000); see also laboratory experiments of Hooyer and Iversen (2000a) and the model of particle loops in a deforming till of Thorsteinsson and Raymond (2000). A physically based numerical model for such rotation and its effects on vertical sediment mixing has recently been formulated by Hildes (2001). Furthermore, Boulton et al. postulate that if deforming till becomes stiffer with depth, clasts will tend not to rotate but to glide (p. 12). It is not clear what would cause clast gliding in such a case, but this idea is inconsistent with the common observation that clasts are typically flattened and striated on their upper, and not lower surfaces (e.g. in most boulder pavements). Hence, it is the material above the clast sliding over it, not the clast sliding over its substratum that appears most common.

Finally, we have not claimed that till fabrics can be used as a “powerful discriminator of till genesis”, but suggested that fabric patterns in vertical profiles through basal tills yield constraints on the depth of possibly deforming till (p. 144). This holds true also in light of Bennett et al. (1999) who concluded that fabric alone is not able to discriminate between different glacigenic facies, a fact with which we very much agree.

(e) Our argument about preservation of old landscapes and palaeosols beneath till as evidence against thick deforming beds is distorted by Boulton et al., in that they imply we refer exclusively to areas near the limit of glaciation. We suggest our interlocutors carefully read p. 145 of our paper where we give examples from areas tens to hundreds kilometers from ice limits of different glaciations. Boulton et al. refer to two examples where till thickness distribution is as predicted by the deforming-bed model. We cannot accept these examples as universal model for Pleistocene glaciations because of conflicting evidence from e.g. vast areas of the Central European Lowland and parts of the southern Laurentide Ice Sheet. Detailed cartographic data based on thousands of boreholes from Poland and Germany (geological maps 1:50.000 and 1:25.000) do not reveal any systematic trend in till thickness distribution that would correspond to the Boulton et al. model, and no such trend was noted in recent syntheses of Quaternary and glacial geology there (e.g. Mojski, 1993; Benda, 1995). We thus warn against selective data treatment and advocate generalization based on unfiltered data. In accord with Haebelri (1981) and Paterson (1994, p. 171) it should also be noted that in an ice-sheet-scale deforming-bed system operating as a sediment conveyer belt, till concentration would be expected at the terminal position of the conveyer belt. This would also be consistent with transport by ploughing or as debris-rich ice because even during advance, ice and sediment are transported toward the margin and deposited. It is the lack of such concentrations in places that is more consistent with other modes of till formation, such as lodgement and melt-out.

(f) Clark and Walder (1994) and Walder and Fowler (1994) showed that drainage systems under deforming-bed conditions should consist of shallow, high water pressure “canals”. In contrast, geological data from large parts of North America and Europe indicate mainly deep and relatively narrow meltwater channels (the so-called tunnel valleys). Boulton et al., however, see no reason why tunnel valleys should not occur under a glacier with a deforming bed. As one possible reason we suggest a drop of pore water pressure in the sediment in the catchment area of a tunnel valley, which would tend to stabilize the bed. Such pressure drop in the vicinity of subglacial channels (as postulated by, e.g. Piotrowski, 1994, 1997a,b and demonstrated by, e.g. Boulton, 1999) may not only strengthen the subglacial sediment due to increased effective pressure, but also facilitate ice penetration into the bed and basal sediment entrainment, as shown by Iversen (2000).

(g) We are pleased to learn that Boulton et al. accept our argument that the deforming-bed hypothesis cannot explain Heinrich layers. We would mention, however, that surge events that triggered the large-scale iceberg
releases needed to create Heinrich layers could have been caused not only by bed deformation (Boulton et al.), but equally well by basal decoupling and enhanced sliding, as has been suggested for many surging glaciers world-wide (Benn and Evans, 1998, pp. 171–174 and references therein).

(h) Boulton et al. claim that our skepticism about the scarcity of present-day analogues of deforming beds is unsubstantiated. They refer to their Table 1, which should provide such examples. Here we mention just two, possibly most often referenced sites, i.e. Breidamerkurjökull and Ice Stream B. Apart from our objections against Breidamerkurjökull as good analogue for past ice sheets expressed in our original paper (which Boulton et al. do not question), we note that the reliability of experiments conducted under this glacier by Boulton and his co-workers, and the validity of their conclusions have been seriously questioned by van der Veen (1999) in his recent textbook on glacier dynamics (pp. 79–84). According to one line of this criticism, “...it seems possible that the till near the margin of Breidamerkurjökull is being squeezed out from underneath the glacier, rather than deforming under an applied shear stress” (p. 83). If correct, this conclusion would further question this site as adequate analogue for widespread till deformation beneath large Pleistocene ice sheets.

Boulton et al. consistently cite Alley et al.’s (1987) inference based on seismic data about a 6-m-thick deforming bed beneath Ice Stream B, despite the fact that direct measurements support a much thinner deforming till (ca. 3 cm according to Engelhardt and Kamb, 1998). Recently, Alley (2000, p. 173) acknowledged that he and co-workers “may have used the seismic data incorrectly in building models” and suggested a scenario in which true sliding would prevail over bed deformation there. In accord with Alley (2000, p. 173) we repeat that high porosity and high water pressure in subglacial material cannot be taken as evidence of deformation. Despite its low strength, such material will not deform when basal decoupling occurs, a likely situation under Ice Stream B and in other modern glaciers as well.

For the reasons given above we reject the criticism of Boulton et al. as one-sided and unsubstantiated. As we did in our article, we again clearly state that we accept subglacial sediment deformation as geological process, but question its first-rank importance as an ice movement and sediment transport mechanism during past continental glaciations. We postulate that a combination of basal sliding and ploughing, and englacial sediment transport can explain most of the phenomena observed in geological record. We are also warning of the danger of not keeping an open mind on this issue, thus falling into the trap of assuming a deforming bed, describing characteristics of a “deforming till”, then using these characteristics to “identify” deforming beds based on what may be a faulty initial assumption.

In addition to responding to comments of Boulton et al. on our paper, we hope we have underscored the need for a multifaceted effort to shed more light on processes of subglacial sediment transport and glacier movement over soft beds, the importance of which cannot be overestimated.

References


