EIGHT

Subglacial megafloods: outrageous hypothesis or just outrageous?

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Since the concept of drumlin formation by glacifluvial processes was first proposed by Shaw (1983), a large number of papers have been published interpreting a wide range of subglacial landforms in North America as products of subglacial megafloods (e.g. Shaw & Kvill, 1984; Shaw *et al*., 1989, 2000; Shaw & Gilbert, 1990; Fisher & Shaw, 1992; Rains *et al*., 1993; Brennand *et al*., 1995; Sjogren & Rains, 1995; Shaw, 1996, 2002; Munro & Shaw, 1997; Beaney & Hicks, 2000; Beaney & Shaw, 2000; Munro-Stasiuk & Shaw, 2002). The sheer volume of peer-reviewed publications promoting the 'megaflood interpretation', and the fact that it has featured prominently in at least two recent compendia of earth science (Young, 2000; Brennand, 2004), may lend it an aura of respectability in the eyes of those unfamiliar with the evidence. However, most Quaternary scientists give little or no credence to the megaflood interpretation, and it conflicts with an overwhelming body of modern research on past and present ice-sheet beds. Despite this, there have been few attempts to systematically scrutinize the megaflood interpretation in print. In large part, this is because most working scientists are busy pursuing their own research programmes and are unwilling to invest time refuting ideas which are clearly incompatible with a huge body of mainstream research.

It is to the credit of journal editors that outrageous hypotheses (Davis, 1926) have been published. Science, after all, should proceed by the testing of ideas in the public domain, not by the censorship of papers simply because they are unorthodox. To become accepted, however, research should conform to the requirements of good science. Non-specialists can rarely be effective judges of whether this is or is not the case, and there is a risk that flawed science may appear to be mainstream in the eyes of a wider public. This is a common problem in the public perception of science, due to the unfortunate tendency for contentious theories to attract disproportionate attention. For this reason alone, the reluctance of many Quaternary scientists to publicly engage with the megaflood interpretation is regretable. This, combined with the fact that the megaflood interpretation has been seized by internet Creationist sites as 'proof' of the Noachian Flood, prompts us to undertake here the task of explaining, for the benefit of those unfamiliar with modern sedimentological literature, why the flood interpretation is unscientific, unnecessary and inconsistent with the evidence.

The megaflood interpretation is not a hypothesis in the Popperian sense of a provisional set of ideas that make clear, unambiguous predictions which can be objectively tested against new observations. The clearest example of this is the claim that megafloods can create drumlins by two different mechanisms, (i) by the infilling of subglacial cavities (cavilty fills) and (ii) by eroding away interdrumlin areas (erosional remnants). The cavity-fill interpretation was proposed by Shaw (1983) and Shaw & Kvill (1984), and visualizes drumlins as the infills of scours cut upward into the ice by turbulent waters below. This idea, which was based on the similarity of form between certain drumlins and sole marks below turbidites, and the presence of sorted sediments within some drumlins, makes the clear prediction that other drumlins should also be composed of sorted sediment (e.g. Sharpe, 1987). If this prediction is put to a Popperian test and a drumlin is found that does not contain sorted sediment, then the hypothesis should be rejected. However, this was not the option taken by Shaw *et al.* (1989, 2000) and Shaw (1993), who proposed that till-cored drumlins and flutings are the remnants of pre-existing tills left behind when megafloods eroded the material between them. In other words, no matter what the internal composition, drumlins are interpreted as 'evidence' for megafloods. This being the case, the internal composition becomes irrelevant to the megaflood case, which is shown to rely exclusively on the perceived morphological similarity between drumlins and streamlined forms eroded by turbulent flows. Moreover, the megaflood interpretation apparently does not predict any systematic differences in the forms produced by these two mechanisms. If no such differences are expected, how can a single process (subglacial sheet floods) create two sets of erosional forms which are exactly equal and opposite in morphology? That is, why should moulds of erosional scours cut up into overlying ice look exactly like the remnants left behind by erosion of the substratum in other parts of the same flood? This difficulty is not experienced by Boulton's (1987) deformation model of drumlin formation or Tulaczyk *et al.*'s (2001) ploughing mechanism of substrate fluting (see also Clark *et al.*, 2003), for example, which potentially can explain all drumlins and flutings in terms of a single process, namely the streamlining of pre-existing bed materials be they composed of till, rock, or stratified sands and gravels. The deformation and ploughing models, moreover, make the clear prediction that the drumlins and flutings formed by such processes should be mantled by glacitectonite or till, a prediction that is borne out in our experience. These models do not, as Shaw and his co-workers repeatedly try to assert, champion the cause of pervasive deformation to the exclusion of all other processes.

By invoking the 'erosional remnant' mechanism to account for drumlins that cannot be explained by the 'cavity fill' process, Shaw is using a classic *ad hoc* protection device (Chalmers, 1976), the sole purpose of which is to remove difficulties encountered by the original theory. Any model that is protected from awkward evidence in this way is in effect unfalsifiable. This is not the hallmark of a hypothesis, but of a self-reinforcing belief system in which the interpretation of the evidence depends on pre-existing conclusions. Although many of the papers by Shaw and co-workers invoke the language of hypothesis testing, this does not stand up to scrutiny. For example, Shaw (this volume, Chapter 4) asks us to 'imagine' scenarios 'for the sake of hypothesis testing', then we find later in the paper that the imaginings are facts, which are then used to support further imaginings, and so on. Nowhere is there a serious and objective comparison of prediction with evidence.

As noted above, the megaflood interpretation relies very heavily, if not exclusively, on the morphological similarity between drumlins and certain kinds of small-scale erosional scours on the one hand, and between Rogen moraine and wavy fluvial bedforms on the other. There is indeed a superficial resemblance between drumlins and sole marks, as we illustrated in fig. 11.25 of *Glaciers and Glaciation* (Benn & Evans, 1998). However, this resemblance does not imply that they were necessarily formed by the same medium. The simple explanation is that obstacles below flowing media exhibit shadow effects, such that their presence influences patterns of erosion far down-flow. This effect applies not only to turbulent flows, but also to non-turbulent flowing media, such as ice. One need only to think of fluted moraines exposed on modern glacier forelands to see that this is so. Indeed, drumlins and megaflutings show a much stronger resemblance to streamlined subglacial landforms exposed by recent glacier retreat (in form and in scale) than they do to scours formed by turbulent media. The similarities are not merely superficial, but extend to numerous characteristics at a wide range of scales. Furthermore, historically produced fluting fields such as those in front of Breidamerkurjøkull in Iceland allow us to relate sediment and landform characteristics to genetic processes with a high degree of confidence (Evans & Twigg, 2002). At Breidamerkurjøkull, flutings are aligned parallel to known former ice-flow directions in slightly offset flow sets that terminate at moraines (Fig. 8.1). Tills in the flutings commonly have erosional lower contacts with glacitectonized or undisturbed outwash. Detailed process studies have demonstrated the role of subglacial lodgement and deformation in the origin of the tills and the flutings (e.g. Boulton & Hindmarsh, 1987; Benn, 1995; Benn & Evans, 1996; Boulton *et al.*, 2001). Eskers mark the location of channelized meltwater. This landsystem provides us with a clear process–form model and it is a small logical step to assume that it can be applied to ancient landform–sediment assemblages that have a wide range of closely similar characteristics.

To this end, landform assemblages illustrated through digital elevation models (DEMs) by Munro & Shaw (1997) have been remapped from aerial photograph mosaics, and have been shown to consist of discrete fields of glacially streamlined features (flutings) terminating at a series of inset transverse ridges (moraines) organized in broad arcuate bands (Fig. 8.2). More localized moraine arcs record topographically induced lobation of the ice margin during recession. Minor readvances of these lobes are documented by the localized superimposition of transverse ridges. This is a landform characteristic difficult to explain as a subglacial fluvial erosional ripple mark, the genesis of the transverse ridges suggested by Munro & Shaw (1997). Also evident are misaligned and cross-cutting flow sets, represented either by superimposed flutings or adjacent fluting fields with orientations that are significantly different and which cannot be explained by contemporaneous ice flow deviations. Juxtaposed fluting fields that display different orientations are convincingly interpreted as glacier flow sets (Clark, 1997) and the superimposition of flow sets is often identifiable in cross-cutting flutings (Dyke & Morris, 1988; Boulton & Clark, 1990; Clark, 1993). Flow sets have been interpreted as the subglacial imprint of fast glacier flow in an ice mass with shifting loci of ice dispersal and termination. Cross-cutting can be explained by the ice streamlining hypothesis but not the sheetflood hypothesis of fluting formation. In addition, evidence of subglacial and/or englacial meltwater activity is manifest in fragmented single and anabranched esker networks and occasional elongate water-filled depressions. This assemblage of landforms is similar in every respect to the glacial landsystem reported by Evans & Twigg (2002) from southern Iceland, characterized by inset sequences of integrated subglacial and ice-marginal landforms produced by lobate marginal recession of active temperate glaciers.

Given the availability of clear modern analogues for ice-sheet beds on modern glacier forelands, the case for the megaflood interpretation is seriously weakened, because turbulent flows are shown to be unneccesary to explain streamlined subglacial landforms. Historical jøkulhaups have occurred at Breidamerkurjøkull, but these were associated exclusively with small ice-dammed lakes at the western and eastern margins. Almost all of the glacier foreland (which is extensively fluted) has been unaffected by jøkulhlaups. If sheetfloods are unnecessary (indeed, impossible) as an explanation for streamlined subglacial landforms at Breidamerkurjøkull, why must we invoke them to explain closely similar sediment–landform associations in, say, Alberta? In the fourteenth century, William of Ockham wrote, '*pluralitas non est ponenda sine neccesitate*', which translates as 'entities should not be multiplied unnecessarily'. The principle that explanations of phenomena should not invoke any agencies not actually *required* by the evidence, led the way out of medieval superstition into scientific enlightenment. It still stands as a central tenet of science today. If we already have an ice sheet, and

Figure 8.1 Push moraines and flutings on the foreland of Breidamerkurjøkull, extracted from a 1998 map of the area by Evans & Twigg (2000). Each flow set of flutings is located between push moraines and when compared the flow sets record a slightly offset ice flow direction.

flowing ice can create streamlined landforms, why do we need a flood?

Thus, although the resemblances between streamlined subglacial landforms and features such as sole marks and sastrugi may appear significant, modern glacial landforms provide much stronger analogies. Attempts to boost the argument for turbulent flows by appealing to required Reynolds numbers are spurious. The Reynolds number *Re* is the ratio between inertial and viscous forces in a fluid. If it is assumed that streamlined subglacial landforms must have been made by turbulent flows (high *Re*), then of course it follows that ice was not the medium, since ice flow is not turbulent. But this is mere circular reasoning, in which the conclusion comes first, disguised as an argument.

The case for the megaflood interpretation would be strengthened if there were independent evidence that requires us to believe that large volumes of water were stored at the beds of former ice sheets. Shaw (this volume, Chapter 4) tackles this issue by arguing that the presence of tills implies that meltwater was 'abundant' in the inner parts of the Scandinavian and Laurentide ice sheets. Munro-Stasiuk (2000, 2003) uses the occurrence of stratified diamictons in an area of south-central Alberta to support the contention that englacial debris was melting out into large subglacial lakes beneath the southwest Laurentide Ice Sheet. The use of stratified diamictons as indicators of subglacial melt-out is a contentious one, but even if we accept that melt-out was the primary depositional process (and observations on sedimentation rates in modern subglacial lakes (e.g. Siegert, 2000; Siegert *et al*., 2001) suggest that this is almost insignificant), the implied volume of water produced does not come anywhere close to the quantities required by successive manifestations of the megaflood interpretation. Consider the quantities involved. We are told that a megaflood draining into the Gulf of Mexico (only one component of the proposed cataclysm) was sufficient to raise global sea level by 3.7 m. This is about half the mass of the Greenland Ice Sheet. To claim that the occurrence of stratified tills supports the existence of enough water to supply floods of the required magnitude involves a logic jump that bypasses numerous stages of hypothesis testing. One obvious query relates to the nature of water release during melt-out till production—surely it must be released slowly and passively in order to preserve the delicate structures? Moreover, the melt-out process would have continued in areas of the bed of the former Laurentide Ice Sheet well after it had disappeared; it continues today in northern Canada (Dyke & Savelle, 2000; Dyke & Evans, 2003). The very nature of melt-

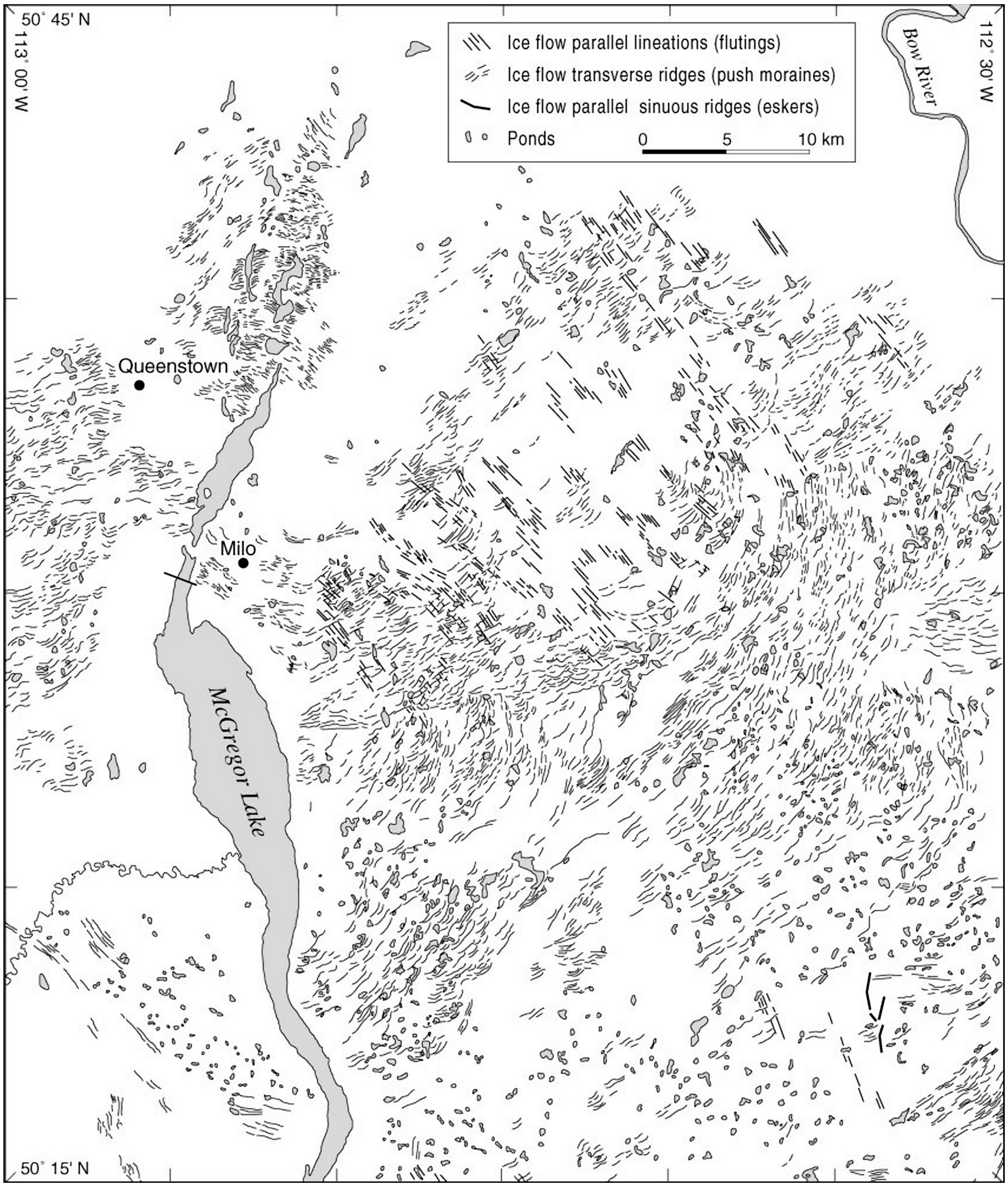

Figure 8.2 Sequences of push moraines and flutings mapped from aerial photographs of a part of south-central Alberta previously mapped from DEMs by Munro & Shaw (1997). Note the overprinting of push moraines with slightly offset alignments and the localized lobation that coincides with topographic hollows.

out till production precludes any central role it could have played in the production of catastrophic subglacial meltwater floods.

The idea of floods of such 'unimaginable' (Shaw, this volume, Chapter 8) dimensions is the outcome of taking flawed assumptions to their logical conclusion, a form of *reductio ad absurdum* in which the final absurdity is taken not as evidence of false premises but as fact. The initial flawed assumption, that the form analogy between drumlins and scours made by turbulent flows implies a common mode of formation, led to the conclusion that the Livingston Lake drumlin field in north Saskatchewan records a megaflood event (Shaw, 1983). Once this was accepted, then the close association between drumlins and other landforms (such as Rogen and hummocky moraine) was taken to imply that they too must have a flood origin (Munro & Shaw, 1997). As such landforms are very widespread inside the limits of the Laurentide Ice Sheet, this assumed origin in turn leads to the conclusion that 'unimaginable' megafloods must have occurred. Each stage of this chain of thought is deeply flawed, but has been woven into such a dense network of assumptions and conclusions that it appears solid to its proponents and to unwary onlookers. The apparently supporting 'evidence', such as widespread streamlined forms visible on DEMs (Shaw *et al.*, 1996; Beaney & Shaw, 2000), and the modelling work of Shoemaker (1992a,b) are no more than assumptions in disguise, which serve to perpetuate the myth in a self-reinforcing cycle in which the distinction between assumption and conclusion is constantly blurred.

This process is apparently so beguiling that the evidence itself becomes distorted to fit the interpretation. As an example, Shaw (this volume, Chapter 4) tells us that the landscape within the limits of the last Laurentide Ice Sheet represents a snapshot of the glacier bed at one moment in time, a pristine, synoptic assemblage preserved by continent-wide ice stagnation following a megaflood. The lack of recessional moraines or of cross-cutting relationships within drumlin fields are presented as facts, whereas several researchers have convincingly shown that the opposite is the case (see Fig. 8.2). Widespread cross-cutting streamlined landforms have been thoroughly documented for example by Dyke & Morris (1988), Boulton & Clark (1990) and Clark (1993). As was illustrated above with the Alberta case study, the very features used to support a subglacial megaflood (ripple marks of Munro & Shaw, 1997) in fact constitute the 'missing' recessional moraines that only become 'visible' when mapped objectively and systematically from aerial photographs.

It must be emphasized that our refutation of the arguments of Shaw and co-workers does not force us to conclude that no large subglacial outburst floods occurred during the lifetime of the great Pleistocene ice sheets. Indeed, evidence for such floods, in the form of tunnel channels for example, is found in many places (e.g. Brennand & Shaw, 1994; Patterson, 1994; Ó Cofaigh, 1996; Clayton *et al.*, 1999; Cutler *et al.*, 2002). Subglacial reservoirs, mostly of the order of 10 km across, are now known to be widespread below the Antarctic Ice Sheet (Siegert, 2000), and some such reservoirs appear to have drained catastrophically in the past (Shaw & Healy, 1977; Denton *et al.*, 1993). Tunnel channels and anastomosing channel networks such as the Labyrinth in the Antarctic Dry Valleys provide compelling evidence for catastrophic drainage events, but acknowledgement of this does not lead logically to the conclusion that landforms such as drumlins, flutings and hummocky moraine record vastly larger floods. The juxtaposition of demonstrably glacifluvial landforms (channels) with flutings or other landforms does not imply that all were formed simultaneously by the same mechanisms, as consideration of the landform associations at Breidamerkurjøkull makes clear (Evans & Twigg, 2002). To make the step from local, albeit large floods to events of Biblical proportions falsely polarizes the situation into an 'all or nothing' scenario, which actually distracts attention away from the important business of determining the true importance of catastrophic discharges from former ice sheets.

Reply to Benn and Evans by John Shaw and Mandy Munro-Stasiuk

This response to the comments of Benn and Evans is divided into three parts: fact, omission and philosophy.

Fact

Benn and Evans argue that the basis for the interpretation of the Livingstone Lake drumlins was the form and the presence of sorted sediment in the drumlins. This is a simplification: the interpretation was based on the form of drumlins, the *style***,** not the *presence*, of sediment, clast lithology and rounding, landform association and landform sequence.

They also write that the meltwater hypothesis does not predict any systematic difference in the morphology of cavity fill and erosional drumlins. In fact, Shaw (1996) gives the two types of drumlins two different names because they are distinctly different in form. Shaw (1983) had previously drawn attention to the remarkable difference in form between the Livingstone Lake drumlins and classic drumlins. It was this difference that prompted the meltwater hypothesis.

As far as the Livingstone lake drumlins are concerned, Boulton's (1987) paper is contradicted by the field evidence; where he shows attenuation of fold limbs, the sediment is undeformed (Shaw *et al.*, 1989). Tulaczyk *et al.* (2001) specifically state that their ploughing mechanism does not explain drumlins. How could ploughing explain crescentic scours around the upstream ends of drumlins? As well, Tulaczyk *et al.* (2001) point out that the deep, pervasive deformation required by Boulton to explain drumlins is unlikely in light of observations beneath modern glaciers and experiments on deformation. Clarke *et al.* (2003) write on landforms without any observations on their internal structure. They deal exclusively with form and ignore our field observations on structure.

Benn and Evans state incorrectly that the megaflood hypothesis relies very heavily, if not exclusively, on morphological similarity with other forms. In addition to morphology we studied landform pattern, and detailed sedimentology including sedimentary architecture, landform associations, clast lithology and roundness (Shaw *et al.*, 2000), computational fluid dynamics (Pollard *et al.*, 1996), stone lags (Rains *et al.*, 1993; Munro & Shaw, 1997; Beaney, 2002), hydraulic modelling (Beaney & Hicks, 2000), and valley profiles (Rains *et al.*, 2002; Beaney, 2002).

As an example, they state that Munro & Shaw (1997) interpret Rogen and hummocky moraine as flood landforms because hummocks are associated with other landforms interpreted as flood forms and therefore assumptions become based upon assumptions. This is misleading because Benn and Evans are forcing the reader to believe that Munro & Shaw (1997) presented form analogy as their only evidence. They do, in fact, miss the point and ignore the main evidence presented: hummocks are truncated at their surfaces regardless of internal structure and without deformation of the immediate underlying sediments (see Munro-Stasiuk and Sjogren, this volume, Chapter 5, for images), and boulder lags heavily pitted with percussion marks sit on the surface of many hummocks. Although form was the starting point in formulating the megaflood hypothesis and remains an essential element, our hypothesis testing has clearly become more sophisticated.

Benn and Evans have used aerial photograph mosaics to contradict the morphological work of Munro & Shaw (1997) in south-central Alberta. It should be noted that mosaics provide less information on form than stereo aerial photographs or DEMs because they do not give a three-dimensional view of the landscape. It is also surprising then that Benn and Evans criticize us for using form analogy because they also use analogy. When they compare landforms around McGregor Lake Reservoir in south-central Alberta with forms in the forefield of Breidamerkurjökull, they give no indication of internal sediment, or sediment–landform relationships. Instead, they ignore the sedimentary evidence presented by Munro & Shaw (1997) (see previous paragraph). Here we present an aerial photograph with an interpretive map (Fig. 8.3) that lies within the mapped region presented by Benn and Evans. Clearly there are a number of different 'transverse' landforms on this photograph, yet Benn and Evans show all ridges as push moraines. They map undifferentiated mounds, ridged mounds (some with central depressions), linear ridges and some eskers as push moraines. The eskers show a flow direction towards the southwest, towards McGregor Lake Reservoir (transverse to the main regional flow direction). Of all the features on Fig. 8.3, few resemble the features on the forefield of Breidamerkurjökull (compare Fig. 8.3 with fig. 7 in Evans *et al.*, 1999). It is possible that the linear transverse ridges are moraines, as they are quite similar to Icelandic features. There are other features like these in southern Alberta only 20 miles from the McGregor Lake Reservoir near the town of High River that strongly resemble many of the moraines around Vatnajökull in Iceland. However, all other features are dissimilar and require an alternative interpretation. We have tried to provide an alternative explanation based on the similarity of these forms with large-scale ripple marks, *and* based on sedimentology (Munro & Shaw, 1997). It appears that Benn and Evans are confusing landforms. There are some broad arcuate ridges that have a very restricted distribution in Alberta. Although they may represent the ice margins they cannot be used as evidence for ubiquitous ice recession on the Canadian Prairies because they are not present everywhere. On the other hand the transverse features described by Munro & Shaw (1997) as hummocky terrain and the giant ripples described by Beaney & Shaw (2000) are distinctly different. The giant ripples are sinusoidal, and in places show rhomboidal patterns. The forms described by Beaney & Shaw (2000) are eroded from undeformed bedrock so how could they be push moraines?

Benn and Evans refer to the use of Reynolds numbers in our hypothesis as spurious. Considering erosional marks in bedrock (Kor *et al.*, 1991), we find rounded, lag boulders, over 1 m in diameter and with distinctive percussion marks. These boulders rest on erosional marks that are identical in form to those inter-

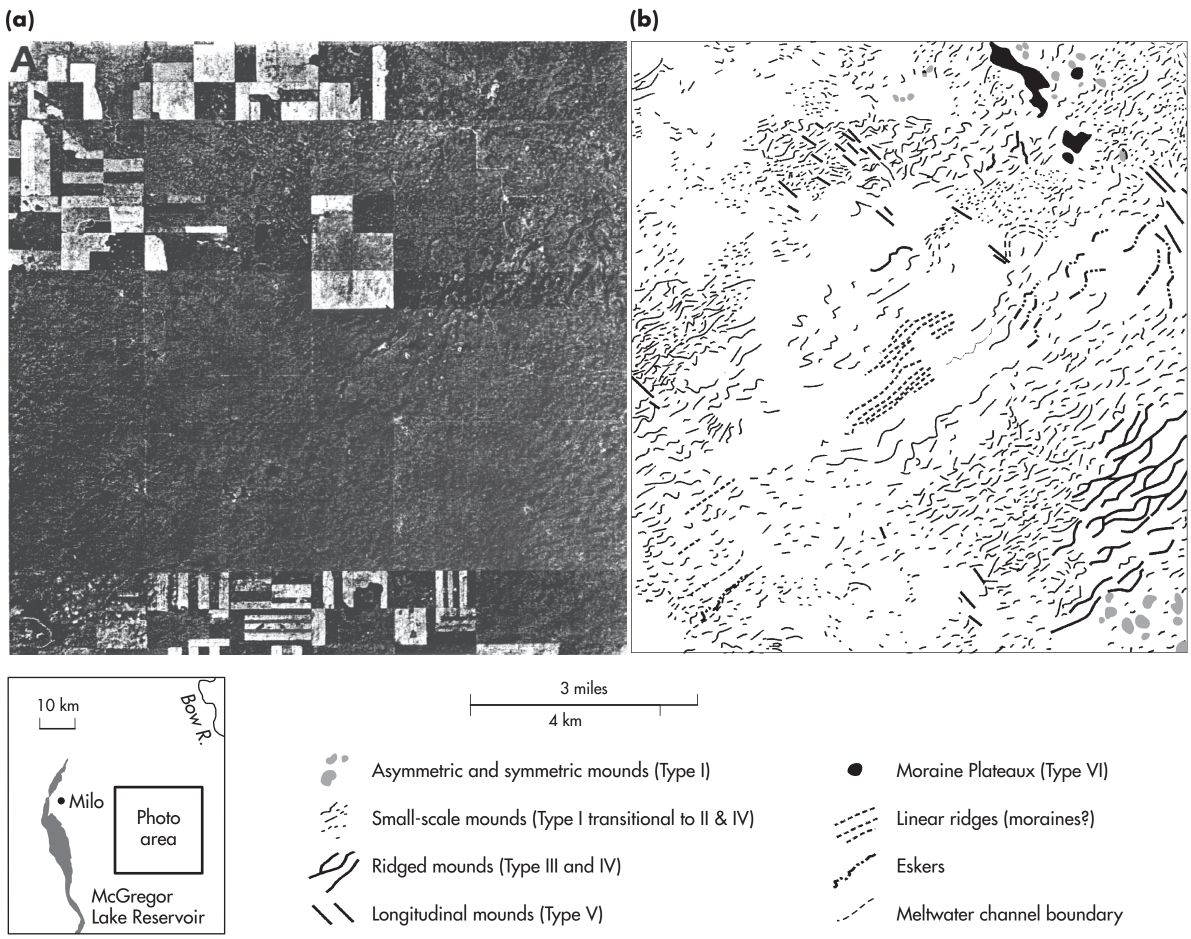

Figure 8.3 (a) Aerial photograph from the McGregor Lake Reservoir region. (b) Interpretation of ridges on aerial photographs based on photograph analysis and field work. Small map inset at base shows the position of the photograph relative to the area presented in Fig. 8.2.

preted by Allen (1982) as products of turbulent, separated flow. It is literally impossible for laminar flow to have transported the boulders and formed the erosional marks. Contrary to the opinion of Benn and Evans, Reynolds number calculations follow from the field observations; the flow depths and velocities required for these calculations are not assumed.

Benn and Evans provide an extensive discussion on the source and amount of water for the floods. They miss the point. We do not argue that melt-out produces *all* the water for floods. Munro-Stasiuk (2000) specifically noted that the melt-out processes (associated with small volumes of water) preceded the flood events and that the water for the major flood was part of the Livingstone Lake Event and was derived far to the north of her study site. She also provided extensive evidence for sedimentation into subglacial reservoirs that occupied the local pre-glacial valley system (Munro-Stasiuk, 2003). The sedimentary facies representative of the reservoirs consists of a range of subaqueous deposits (mostly gravity flow deposits and *not* melt-out deposits) that are chaotically deposited and range from entirely undisturbed to pervasively deformed (interpreted as ice-reactivation). Although Benn and Evans go on at length arguing that the subglacial reservoirs discussed by Munro-Stasiuk (2003) were not large enough to provide the volume of water required for a megaflood, Munro-Stasiuk noted that the reservoirs were 'small', never contended that the reservoirs were the source of the megaflood waters, and clearly stated that the presence of reservoirs was followed by till deposition and then by the event responsible for creating the erosional landforms. In this volume, Shaw (Chapter 4) refers to abundant meltwater to refute the notion of frozen bed conditions near the centre of the ice sheet and to account for hydraulic connectivity between the central zone of the ice sheet and the margins. Shaw (1996) proposed a supraglacial origin for the meltwater, a suggestion that is well supported by observations on modern glaciers and by modelling of past ice sheets (Marshall *et al*., 2002; Zwally *et al*., 2002a). This proposal clearly establishes that melt-out is not considered as the primary source of meltwater for the flood events.

Benn and Evans argue that modern glacial environments provide all the necessary analogues for past glacial landscapes. This claim is unlikely. For example, there are no known modern glacial landforms resembling Rogen moraine, nor any that show the sedimentary and morphological characteristics of hummocky moraine on the Western Plains of Canada. Furthermore, the streamlined, loess hills of the unglaciated Channelled Scablands in Washington are identical in form to drumlins 100 km or so to the north. Also, the fluted bedrock above Dry Falls in the Scablands is identical to similar bedrock fluting in glaciated areas. Both of these Scabland landforms required immense water sheet floods for their formation. They were formed *beyond* the ice limits and it is therefore impossible that these could have been created by glacial processes. At the same time they provide powerful analogues for drumlins and erosional marks. Obviously, we need something other than analogues from modern glacial environments to explain these non-glacial landforms. Thus, it is reasonable to use Scabland flood landforms as part of a hypothesis on subglacial megafloods.

Shaw never claimed that drumlins and fluting do not show cross-cutting relationships. In fact, Shaw (1996, fig. 7.39) presents a map clearly showing such cross-cutting. We cannot imagine why megafloods could not cause cross-cutting. All that is required is a variation in flow direction. Indeed, Shaw & Gilbert (1990) differentiated the Algonquin and Ontarian events on the basis of the cross-cutting of one flow path by the other. The Algonquin came first. By contrast, there are vast (hundreds of kilometres long) tracts of drumlins and flutings, the Livingstone Lake flow path for example, that do not show cross-cutting relationships, nor do they display systematic superimposed forms. It is our view that these tracts represent pristine subglacial surfaces.

In summary, Benn and Evans state that the megaflood case 'is shown to rely exclusively on the perceived morphological similarity between drumlins and streamlined forms eroded by turbulent flows'. As discussed above, the megaflood case is grounded in sedimentary, hydrological and glacial theory, and is supported by extensive morphological and sedimentary observations and interpretations.

Omission

As is commonly the case with those who review our work, Benn and Evans fail to relate bedrock erosional marks to the meltwater hypothesis. Such erosional marks in granite and gneiss in the French River area of Georgian Bay provide very strong evidence for broad, catastrophic subglacial floods (Kor *et al*., 1991). There is no possibility of interpreting them otherwise (Shaw, this volume, Chapter 4). The meltwater hypothesis is well supported by evidence from the streamlined hills and fluted bedrock of the Scablands and from the French River erosional marks, all of which require extensive, turbulent sheet floods for their formation.

Benn and Evans also ignore some of the main evidence for large-scale erosional events such as erosion into undisturbed preglacial gravels in the Blackspring Ridge flute field in south-central Alberta (Munro-Stasiuk & Shaw, 2002). They make the blanket statement that glacitectonite and till should appear in these landforms. They also ignore the observation that many landforms have truncated surfaces representative of a landform unconformity (e.g. Munro & Shaw, 1997; Munro-Stasiuk & Shaw, 2003, Sharpe *et al*., 2004).

Philosophy

Benn and Evans seem to be intent on discrediting the meltwater hypothesis on the basis of questionable reasoning and philosophy. We would like to comment critically on their approach.

They state that most Quaternary scientists give little credence to the megaflood hypothesis and that non-specialists need to be protected from its flawed science. Did they conduct a poll to determine that most Quaternary scientists do not believe the hypothesis? Hardly: there are many Quaternarists who embrace these ideas. Surely Benn and Evans are not suggesting that these researchers are 'unfamiliar with the evidence' or that their research is also 'unscientific, unnecessary and inconsistent with the evidence'. As well, is there really a wider public that needs to be protected against our unscientific thoughts? The alternative to Benn and Evan's view is that the megaflood hypothesis is not really flawed; rather it challenges and threatens establishment research. It would not be the first time that establishment figures have railed against a fruitful hypothesis because they found it repugnant.

The reasoning on the dual interpretation of drumlins misrepresents our work and the work of Popper. We do not hold that there is a need for two types of drumlins *because* there is sorted sediment in one and not in the other. We hold this view *because* the sedimentary structure and architecture, clast shape and clast lithology in one type are so different from those in the other type. As well, the forms of cavity fill drumlins, spindle, parabolic and transverse asymmetrical, are so unlike classic drumlins that we felt obliged to give them different names—Livingstones and Beverleys (Shaw, 1996). Beverleys, with troughs wrapped around the proximal ends, are almost certainly erosional, but we do not, as Benn and Evans assert, base this argument 'exclusively' on perceived morphology. We consider morphology related to the action of horseshoe vortices, turbulent structures at forward-facing steps, truncation of internal structure, stone lags on the erosional surface and landform associations (e.g., Shaw *et al*., 2000). We are well aware that the internal structure and composition is less important in deducing the formation of erosional landforms, compared with that of depositional forms. In the case of landforms such as the French River erosional marks cut in granite and gneiss, this point is so obvious it is taken for granted. By contrast, we go to great lengths in the case of hummocks and large-scale fluting to demonstrate that internal structure and surface form are largely independent (Munro & Shaw, 1997; Shaw *et al*., 2000; Munro-Stasiuk & Shaw, 2002). We certainly do not accept that sedimentary structure, architecture and clast lithology are irrelevant to understanding subglacial landforms in the megaflood hypothesis. Why would much of our work involve sedimentology if it were so evidently irrelevant to the meltwater hypothesis? Erosional drumlins may contain sorted and stratified sediment. Benn and Evans appear to have overlooked this point and insist that erosional drumlins are till cored. Hence their confusion on the duality of drumlins and their assertion that the megaflood hypothesis for drumlins is unfalsifiable. In reality, details of form, architecture, sedimentology and lithology allow us to distinguish cavity fill drumlins, Livingstones, from erosional drumlins, Beverleys. There are very specific predictions on the characteristics of erosional and cavity fill drumlins and we continue to use these predictions when interpreting landforms. We wonder why Benn and Evans make such a fuss over a matter that we have treated exhaustively in a number of papers.

We do not make our hypothesis unfalsifiable by protecting it from awkward evidence: the hypothesis is easily falsifiable. For example, the hypothesis would be rejected if the properties of the sediment in depositional landforms contradict the specific predictions for cavity fills (e.g. it is aeolian or marine or is deformed into the shape of the landform) or, for the case of erosional landforms, the hypothesis is rejected if the patterns of defining erosional troughs show cross-cutting rather than bifurcating and merging relationships. Rather, we have amended the hypothesis as it became apparent that it was contradicted by certain observations. Thus, we introduced an erosional version because drumlins in bedrock, for example, demanded it. Benn and Evans argue that this is sleight of hand. We suggest that it is sensible. It seems illogical to argue that erosional drumlins and cavity fills cannot both exist. Sedimentologists and geomorphologists describe some bedforms as erosional and some as depositional without concluding that hypotheses on their genesis are unfalsifiable (Allen, 1982). Since Benn and Evans fail to recognize, rather they ignore, certain observations (e.g. the truncation at the land surface, the presence of boulder lags over erosional landforms, the origin of bedrock forms), they are the ones using the *ad hoc* protection device they accuse us of using. They are protecting their own models from the 'awkward' evidence we present. For instance, they state that in their experience all flutings and drumlins are mantled by glacitectonite or till and thus they can be easily explained by ploughing or deformation processes. They ignore observations that contradict their prediction (e.g. Rains *et al*., 1993; Munro & Shaw, 1997).

Shaw (this volume, Chapter 4) does not present imaginings as fact as Benn and Evans assert. More than any other modern hypothesis on subglacial bedforms, our work is grounded on fieldwork, experiment and imagery. We believe that whereas our facts are based on observation, much of the literature on subglacial deformation is based on modelling. We also believe that the most powerful explanations of these bedforms will come ultimately from a combination of both approaches.

William of Ockham's tenet—'entities should not be multiplied unnecessarily'—is very helpful in this discussion because the multiplication of entities has been necessary. Analogues from modern glaciated areas cannot explain Rogen moraine. Nor can they explain streamlined forms in bedrock and loess in the Scablands, lying beyond the limits of the Cordilleran Ice Sheet. Obviously we need additional entities to those from modern glacial environments. Hence, the Scablands streamlined forms are excellent analogues for erosional drumlins and fluting in bedrock and it is proper to use them in a hypothesis stating that *some* subglacial landforms resulted from megafloods.

The processes Benn and Evans champion as generally applicable fail to explain the characteristics of many bedforms. For example, Tulaczyk *et al*. (2001) point out that their ploughing method does not explain drumlin patterns. We invite Benn and Evans to explain the topology of a ploughing mechanism that bifurcates around the upstream ends of drumlins and does not cross-cut downstream crescentic troughs. It is topologically impossible that ploughing can behave in this way. Indeed, Tulaczyk *et al*. (2001, p. 64) write: 'Whilst this (*the carving of intermediate grooves by ploughing*) does not explain the pattern of all bedforms such as drumlins and Rogen moraine, it may explain the observed form of megalineations'. Italics added. Chris Clark freely admits that this is a problem for their hypothesis and is the reason why they only deal with megalineations and not with drumlins. It is worth noting that Tulaczyk *et al*. (2001) argue very strongly against the ideas of Boulton (1987), yet Benn and Evans present the contradictory views of these authors as supporting their conclusions. Such inconsistency detracts from their arguments.

The modern glacial analogue approach is incapable of explaining either Rogen moraine or the scale of megalineations. Ploughing by ice keels cannot explain the form and pattern of drumlins. The megaflood hypothesis is attractive because it is not faced by these difficulties. More to the point, such difficulties make it *necessary* to introduce new entities; the megaflood hypothesis presents such new entities. There is no quarrel with William of Ockham here.

The comments on melt-out till and the generation of meltwater for megafloods set up a red herring. We have been at pains to point out that the melt-out till precedes the megaflood and is commonly a remnant within erosional drumlins (e.g. Shaw *et al.*, 2000). To demonstrate this, we present some of the most detailed field sketches of fluting sediment together with structural and fabric data. A stone lag, interpreted to have been produced by flood erosion, lies on an erosional surface, truncating melt-out till and diapiric mélange. It is this erosional surface that defines the fluting. The melt-out till preceded the megaflood that eroded the drumlin and meltwater involved in the formation of the till was probably of little consequence to the flood which originated far to the north. Of course, as pointed out since 1982, the water for melt-out till was released slowly; we present estimates of thousands of years for melt-out till formation (Shaw *et al.*, 2000). Once again, we are misrepresented and the arguments we make for a supraglacial origin for the megaflood discharge are overlooked.

Obviously, there is a major difference in perception between Benn and Evans and us. They consider that the megaflood hypothesis flies in the face of a huge body of mainstream research. In our defence, there is no known observation that contradicts the hypothesis. Nor does this hypothesis violate any fundamental principle in science. It might be incompatible with mainstream research, but the same can be said of any new paradigm. In answer to their assertion that our work is unscientific and unnecessary, our only response is—we do not think so. It would help if Benn and Evans were more specific where they write that our work is inconsistent with the evidence. Again, there are no known observations that *contradict* the megaflood hypothesis.

NINE
Groundwater under ice sheets and glaciers

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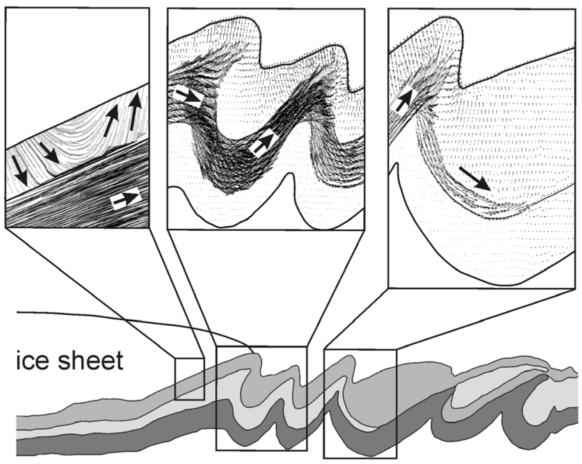

9.1 Introduction

It has been realized only recently that groundwater under ice sheets and glaciers is an important, integral part of the hydrological system in environments affected by glaciation. The late start in research and the resulting scarcity of published work on subglacial groundwater was caused by its position in the no-man's land between glaciology and hydrogeology, despite its relevance for both. It is now recognized that water in permeable rocks and soft sediments overridden by glaciers, through a system of feedbacks, influences glacier stability, movement mechanisms, sediments and landforms. Water discharged from melting ice contributes to the renewal of groundwater resources.

Large-scale groundwater circulation patterns and dynamics experience fundamental changes in glacial–interglacial cycles subjected to repeated loading and relaxation by kilometre-thick ice. Old glacial groundwater trapped in low-permeability areas yields important information about past environmental changes, and modifications of future groundwater flow dynamics in areas likely to be affected by prospective ice sheets must be considered in disposal strategies of toxic waste. It is therefore clear how important the impact of glaciation on groundwater is, which explains the recent interest in this field.

9.2 Water source and drainage systems

Subglacial meltwater originates from a range of sources, mainly from melting of ice by geothermal heat trapped at the glacier sole and by the frictional heat caused by ice movement past the substratum. These two sources yield up to some 100 mm yr^{-1} of water. Close to the ice margin, in the area where englacial conduits extend to the bed, surface ablation water may reach the ice sole with recharge several orders of magnitude greater than the basal meltwater alone. It is difficult to estimate how wide this area is, but as deeper conduits tend to close under cryogenic pressure or they bend horizontally towards the ice margin, it is probable that ablation water would reach the bed only where the ice thickness