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Present-day sandurs are not representative of the geological record—reply

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The interesting remarks made by Marren (2003) are precisely what our earlier contribution (Zielinski and Van Loon, 2002) was aimed at: raising a discussion about the representativeness of the currently used models for sandurs. Other comments that reached us also show that these models need reconsideration.

Marren's remark in his opening sentence—stating that we believe that present-day sandurs are not representative of the geological record because the Weichselian sandurs in NE Poland are different—is, obviously, not correct: we noticed the differences with classical models, tried to find an explanation for them, and came to the conclusion that the present-day situation is most probably exceptional because it is likely a short-lasting stage in the ongoing ice retreat after the last glaciation. This retreat has, incidentally, led to a large number of sandurs that are now, probably for a limited time, situated in a narrow zone between mountainous areas and the sea. If the ice had retreated less far after the last glaciation, it would have reached the sea at many of the Icelandic and Northern American sites, and there would have been no space at all to develop sandurs. If the ice would have retreated farther than is the case nowadays, the ice caps would have split up in a number of isolated, much smaller mountain glaciers and there would have been no lowland sandurs as we see them now.

This implies that the current situation should be considered as an incidental one, representing a specific stage in a development that may proceed in one way or another (ice readvance or retreat), but that cannot be considered as the one and only stage in which sandurs develop. On the contrary, it seems only logical that the end phases of ice movement (full glaciation or disappearance of the large ice caps) are the two most stable and long-lasting situations. Following this reasoning, it must be concluded that the present-day situation cannot be considered—from a geological point of view—as the most common one.

Marren is, obviously, right in that not all present-day sandurs are situated in a narrow zone between mountains and the sea. It is also true that sandurs exist that comply with a situation that is more representative for the past. Unfortunately, however, these more representative sandurs have not been taken as the basis for the models that were developed in the past few decades (Krigström, 1962; Boothroyd and Ashley, 1975; Boothroyd and Nummedal, 1978). The fact that researchers based their models on what we consider as nonrepresentative sandurs is understandable: these sandurs (in particular the Skeidararsandur in Iceland) were much better accessible than the remote sandurs in mountainous areas. In addition, the models were developed in a time when relatively little experience existed with sedimentary modelling, and even much less with the variation that features such as sandurs may show, depending on site-specific conditions.

Marren is also right in that present-day sandurs occur in different settings, partly because of the complexity of deposits and topography in front of an ice sheet. The complexity of the proglacial zone has been dealt with in extenso by Brodzikowski and Van Loon (1991), so that Marren's remark that we are insufficiently aware cannot be taken seriously. The variety in sandurs that results from the complex proglacial topography does not imply, however, that no generally applicable model can be established. We will deal with this in the next section.

1. Generalization and modelling

Earth sciences differ from other natural sciences such as physics and chemistry by the wide variety in

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the appearance of the research objects. Whereas pure sulfur is the same all over the world, sandstones from different places may be (and commonly are) highly different; the same environments (e.g. fluvial environments) are equally different at different places. This implies that it is impossible to speak about 'the sandur' (such as physicists may speak about 'the electron') but that one has to decide whether one should talk about one specific object (e.g., the Skeidararsandur) or about a more generic one (like sandurs in general). In the latter case, one can do so only if site-specific characteristics are deleted, and if some kind of 'average' characteristic is considered. This is precisely what is done in modelling.

It seems that part of the points raised by Marren is based on different ideas about what models are, or what they should be. He refers, for instance, to three publications that would contain proper models of valley sandurs (Fahnestock and Bradley, 1973; Bluck, 1974; Aitken, 1998). Unfortunately, we were unable to trace a copy of the work by Fahnestock and Bradley (1973), but we are well aware of the two other papers. Bluck's (1974) analysis concerns depositional forms and structures, current directions and accumulation in outwash channels; his emphasis is on directional data in sandurs, and we do not think that such an analysis can be considered as a model. Aitken (1998) details the Pleistocene deposits in the Don valley (Scotland), but these concern fan deposits rather than outwash deposits. Moreover, Aitken does, in our opinion, not present a true model, nor does he claim to do so.

It seems that this part of the discussion with Marren can be solved only if agreement is reached about what models are. In our opinion, a sedimentary model refers to a specific environment in which net deposition takes place. The model should be based on observations at as many relevant sites as possible, and be based on the characteristics that are typical for all the deposits formed in this environment. The model should at least deal with (1) the spatial distribution of the subenvironments that can be distinguished within the environment, (2) the processes that contribute to the (erosion and) sedimentation within these subenvironments, (3) the resulting spatial distribution of the various types of deposits formed in these subenvironments, (4) the shift of the various subenvironments in time as a result of the natural development within the environment, (5) the resulting vertical successions (with all their sedimentological characteristics) that are thus formed, and (6) an overview (preferably in plan view) of the various deposits during a characteristic stage (or, if relevant, during characteristic stages) of the environment's development. We did so in our contribution under discussion, but is seems that Marren has a different philosophy about what a model is; he does, however, not detail his idea about models. Part of the detailed comments made by Marren are due to this difference in views regarding the term 'model'.

2. Fans and sandurs

Another part of the confusion raised by Marren is due to a comparable problem: the discrepancy between his and our views about what are sandurs. He devotes an entire section to the questionable equivalence of sandurs to fans, and refers to the recent debate as to whether braided outwash rivers can be classified as alluvial fans. We are well aware of the resemblances and differences between sandurs and fans. In fact, we carried out detailed research on fans developing at an ice front (Zielinski and Van Loon, 1997, 1999a,b) and also provided a sedimentary model for these fans (Zielinski and Van Loon, 2000), based on a wealth of field data. Marren does not refer to these works, thus neglecting both the field data presented in them and the discussion included about what are fans and what are not. We therefore consider his remark that we ignore the recent debates about this topic as unjustified.

In fact, the differences and resemblances we found during field work between deposits showing characteristics that are interpreted by some researchers as typical of fans and by others as typical of sandurs initiated our studies modelling into these two types of sedimentary bodies. Indeed, intermediate forms exist, but—as far as we could find out—only because the local conditions changed during sedimentation (primarily as a result of ice advance or retreat). Under more stable conditions, significant differences between fans and sandurs exist. This is exactly why we started developing a new sedimentary model for glacigenic fans (existing fan models are old—as Marren emphasises himself—and these models developed for present-day fans are, without much discussion, still commonly applied to Pleistocene settings,

particularly by geomorphologists and geographers), and it is exactly why we then started developing a new sedimentary model for outwash deposits. We hoped that these new models would contribute to diminishing confusion, since the difference between fans and sandurs is still not commonly recognized. We regret that Marren apparently overlooks this aspect.

It is interesting in this context that Marren seems of the opinion that valley sandurs cannot be regarded as outwash fans, but that unconfined sandur settings can. We found no indication at all during our research of the Pleistocene deposits in NE Poland that either sandur plains or valley sandurs are present that show characteristics that would justify to identify them as fans. This is only logical, because Pleistocene outwashes formed particularly in lowland areas: the Sandinavian ice sheet reached The Netherlands, Denmark, southern Finland, northern and central Germany, northern and central Poland, Lithuania and Estonia in regions that can hardly be considered as piedmont areas. The same holds for the North American regions reached by the Laurentide ice sheet (Wojtek Nemec, personal communication, 2002).

It is, obviously, possible to discuss the sandur/fan problem at a semantic level: one might consider sandurs as a kind of megafans, in the sense that they resemble fans in plan view, but extend in a longitudinal direction tens to hundreds of kilometres. The deposits forming such landforms do, however, not display the sedimentary features that Blair and McPherson, (1994a,b) consider as characteristic of fans. Stanistreet and McCarthy (1993) had an interesting and sensible standpoint in this matter: megafans represent river deposits, and fossilized megafans cannot be identified as fan deposits. This is confirmed by recent studies of megafans (e.g., Shukla et al., 2001).

3. Sandur dynamics

According to Marren, the sandur models developed in the 1970s overlook that the sandur environment is highly dynamic and that deposition is largely controlled by catastrophic floods (jökulhlaups). He seems to suggest that this inevitably implies that fan-type deposits are formed. He is certainly correct with respect to the large role played by jökulhlaups as far as accumulation is concerned: the last decade has yielded a fairly continuous flow of publications that prove this. And some deposits formed by jökulhlaups are, indeed, fans rather than sandurs. An interesting example was provided by Russell and Knudsen (2002), but the sediments that they investigated build relatively small bodies (up to 2 km long) in an ice-contact environment, and they show precisely the characteristics that we attribute to fans (Zielinski and Van Loon, 1999a,b, 2000). We cannot agree with a reasoning stating that jökulhlaups contribute to fans, and that all sedimentary bodies containing deposits of jökulhlaups are *therefore* fans! Some of them are 'classical' sandurs, indeed.

Marren suggests that the Pleistocene sandurs on which our model is based are the exception rather than the rule (where we state the contrary). He argues (without providing any evidence) that the sites where meltwater left the glaciers or ice-sheet margins in NE Poland were highly variable, so that insufficient time was available to form sandurs. It is not clear to us why the same reasoning would not be applicable to present-day situations in Iceland or elsewhere: one of the characteristics of our time is that, for one reason or another, ice fronts move (in the majority of cases backwards). Nevertheless sandurs do exist nowadays, and Marren does not provide any real argument why they could not have existed in NE Poland during the Weichselian.

4. Form versus sediment

Marren emphasizes geomorphological aspects. We do not consider them unimportant, but we looked primarily at the sedimentary characteristics and found that two main groups of lithofacies could be distinguished on this basis. It then appeared that the sedimentary bodies of each of these two groups must have had different forms (fans and braidplains, respectively). This underlines that there were different conditions, leading to both different lithologies and shapes. The shape was, however, not the main aspect that our research focused on. This is why we submitted our manuscript on the sandur model to Sedimentary Geology, and not to a geomorphological journal.

We can understand very well that the short note presenting our ideas about the sandur model (Zielinski and Van Loon, 2002) is not convincing in every respect, because detailed sedimentological field data are lack-

ing. This is not due to lacking data, but because we considered a short note most suitable for raising a discussion on fundamental criteria. The wealth of sedimentological field data we gathered, including vertical and lateral facies distributions, have been used for a much more detailed contribution that has recently been published in Boreas, a journal which we consider more appropriate for such details (Zielinski and Van Loon, 2003). Discussions about the precise sedimentological data can be held then on a firmer basis. We will welcome such a discussion, just like we welcome the remarks by Marren, which show that the currently used sandur models need revision, indeed.

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