

# Letters

## Glacial Till

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Johnson's (1976) review of the volume *Glacial Till* edited by R. F. Leggett (1976), and his comments on the conference under the same title held in Ottawa in 1975, states in its second sentence that "the opportunity to develop some new approach to the subject through the conference and volume of proceedings seems to have been lost to a large extent". This comment suggests that the reviewer had misunderstood the purpose of the conference: to bring together the workers of all those disciplines who deal with till, mainly from a practical viewpoint (geotechnical, pedologic, prospecting), to present up-to-date reviews on the present status of till investigations, and to create inter-disciplinary discussions in particular on those matters which are not sufficiently clear to workers in other disciplines, or where more cooperation is desired. This was stated in advance in the first announcement of the conference, widely distributed in March, 1974. The four invited speakers were asked to present general reviews on the present "state of art" in their particular fields. Anybody interested was asked to submit proposals on more specific topics, preferably in the form of reviews on the present stage of knowledge on their topics. In summary, the main purpose of the conference was to review and discuss the present status of till investigations in an inter-disciplinary forum, rather than to "develop some new approach to the subject" as expected by Johnson (1976).

A few comments may be made on the reviewer's complaint that insufficient attention was paid to the study of present day glaciers. The organizing committee of the conference was equally anxious to include papers on such studies,

therefore listing "the mechanism of till formation and deposition" as the very first topic among the 'Proposals for Papers' in the first announcement. Disappointingly, one proposal only (out of 68 received) offered to discuss such data, and this proposal (by G. S. Boulton) was accepted. It was unfortunate that, because of a sudden illness in Boulton's family, he could not arrive at the conference, but his paper was included in the volume. Two other proposals also mentioned modern glacial deposits, besides Pleistocene materials, but it was not clearly stated whether present glacial activity was also investigated.

In my oral presentation of the paper on "Tills: their origin . . ." at the conference, I stressed that the increasing number of investigations of present day glaciers have brought us closer to a better understanding of the genesis of till; also, my genetic classification of tills, except for the last right-side column (Dreimanis, 1976, Fig. 5) is based more upon investigations at present day glaciers, than upon inferences from Pleistocene tills. The examples of various genetic types of tills had to be given from Pleistocene tills, to prove, particularly to the engineers and pedologists that genetic classification of tills is not merely a theoretical concept.

There was still a chance, considering the ample time (25% of the total, plus the time reserved for the Boulton's paper) available and used for discussions, but presently active glaciers were hardly mentioned in these discussions, even though several of the 275 participants were familiar with them. There must be some reason why the transport of glacial debris and deposition of till by present day glaciers are discussed less commonly than, for instance, the present day formation of beaches, nearshore and fluvial sediments, or mass movements. The studies of glacial transport and sedimentation are more

difficult, more costly and more restricted by logistics.

The question has also been raised frequently, how much do the present day glaciers resemble the Pleistocene ice sheets. "Modelling" is usually based upon a few available cases, and their generalization occasionally runs into difficulties, when interpreting Pleistocene till sections. This was quite evident, for instance, during a recent (1976) field conference on tills in Sweden, where those who had studied different presently active glaciers, often disagreed among themselves on interpreting the same Pleistocene sections, or were reluctant to draw definite conclusions.

The international project on genesis of till (at the INQUA Commission on genesis and lithology of Quaternary deposits that was one of the co-sponsors of the 'Glacial Till' Conference) in which I have been fortunate to participate for a number of years, indicates that we are gradually approaching the time when a common denominator will be established for observations on the genesis of tills at various presently active or stagnant glaciers, glaciological models, and the extreme variety of the Pleistocene and pre-Pleistocene tills. Still, more "cross-fertilization" of ideas and interchange in the application of research methods is required among various disciplines of the earth sciences and the basic sciences, to speed up this process. The "new avenues of approach" urged by Johnson (1976) have not been forgotten, nor neglected, as one may conclude from some of his (ibid.) remarks, but they were not the main objective of the review papers at 'Glacial Till' conference.

Though the emphasis in the reviews at the 'Glacial Till' conference was on the present and past work done in North America, a considerable amount of references from Europe were included,

particularly from publications which are insufficiently known on this continent, particularly because they have been published in languages other than English. On the other hand, many European workers are unaware of a number of North-American publications. It is hoped that the *Glacial Till* volume will help in exchange of information about the work done by the researchers of at least these two continents; no response has yet been received, unfortunately, from other continents.

### References

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## The Great Flood

Both authors and readers of geoscience literature will share many of the concerns expressed by Jim Aitken (*Publish and Perish*) in the *Pyroclasts* column of this issue. While not being an advocate of the Publish or Perish philosophy, I do feel that Aitken's article contains several naive statements that require some comment.

The inflation of geological literature is to be predicted considering the expansion of universities, government agencies and certain parts of industry over the past two decades. The expansion is not only in numbers of professionals but also a broadening of the disciplines now covered by the earth sciences and the wider spectrum of problems tackled by geoscientists.

Aitken's application of a Richter Scale for publications may be useful in the long term with the benefit of hindsight. In the short term, how many of us can truly recognize a new breakthrough when announced at a GAC or GSA meeting? To what extent are the smaller, less earth-shattering studies necessary to generate the rare breakthroughs? Scientific communication takes many forms at meetings and cannot be restricted to a few lengthy papers on new radical thoughts; I doubt that there is an adequate mechanism for the appropriate selection of these few contributions although perhaps L. W. Morley and J. T. Wilson could form a selection panel.

Aitken charges that published research results are the prime criterion for advancement and/or monetary support in universities and government agencies. They are, and should be, one criterion. In this university, at least, teaching and service are other prime criteria. Peer evaluations are sought on the significance and quality of publications. I am aware of recent cases where one faculty member with fifteen publications was denied tenure and another was in serious question despite over forty publications. The NRC Grant Selection Committees also use similar external assessments of research quality as do most journals. Thus the claim that an evaluation committee "rarely gets beyond counting titles, or possibly pages" is untrue.

Aitken has an obvious admiration for Charles Darwin and *The Origin of Species*. It is noteworthy that Darwin published this book almost twenty years after conceiving, and largely documenting, the idea. He was forced into publication by the similar ideas that had occurred to Wallace and who submitted them quickly for presentation at a scientific meeting. Was the concept of evolution advanced or delayed by Darwin's reluctance to publish? Could Wallace and others have taken it further had Darwin written a briefer account in 1842 rather than 1859? Would Darwin have advanced his concepts further with the benefit of a decade or two of critical comment by other scientists of that time?

The answer must surely not be a return to the old system but a reform of the present: longer grant terms, promotion and salary committees meeting biannually rather than annually; more rigorous refereeing for journal articles; greater use of data banks and microfiche. The publication of "major works" is now almost impossible. In paleontology, for example, there is no publication medium in Canada for major monographic studies: the GSC provides a Memoir service to its officers but this may involve a four year publication time. The proposal for a periodical devoted entirely to brief progress reports is appropriate but it will be a predictably poor seller in the journal market if financed by a society. Where such a publication has been undertaken "in house", i.e., the Report of Activities by the GSC, a notable inflation has also occurred to judge by the last issue (Part C, 1976).

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I would like to comment on the *Pyroclasts* column "*Publish and Perish*" by J. D. Aitken.

I'm sure that everyone admits that there is more than a "clash" of truth in the column, although some of the facts are sufficiently inaccurate and suggested mechanisms for reform are so ill-advised that I believe it defeats its own purpose.

Publication is, of course, necessary if one is to become known to one's peers. The process of peer evaluation used by most promotion and granting committees ensures that a paper every one or two years, if outstanding, is more than adequate to generate a strong reputation and a high level of funding. To use one of Aitken's own examples, Darwin returned with the *Beagle* in 1836, was convinced of the mutability of species by 1837, and had formulated his ideas on natural selection by 1842. However, he waited until 1858, when faced with an essay by A. R. Wallace proposing similar views to his own, to publish his ideas jointly with Wallace. The "*Origin of the Species*" followed quickly in 1859 as a result of this stimulus. No one is quite sure why Darwin waited so long to publish. Perhaps a sense of perfectionism coupled with private means and a large dowry accompanying his marriage in 1839 had something to do with it. I'm sure that Aitken is not advocating that universities and granting agencies attempt to identify potential mini-Darwins and "endow" them for twenty or so years with little published work to show for it. Earth Science would not be where it is today if Hess, Dietz, Wilson, Vine and Mathews had sat on their ideas until they were perfected and beyond all criticism.

My point therefore is that there is a middle ground between that taken by Darwin with respect to evolution and the hectic publication race indulged in by some of us. It is extremely important that science advances as a result of dialogue in which any individual builds on the results, sometimes only the partial results, of others. We must have some pressure within our system which encourages everyone supported by it to publish his results and ideas, imperfect as they may be, within a reasonable time frame.

My strongest objection to the column is the suggestion that needed reforms in our system should be imposed from above by the "masters of research geologists" rather than through a grass roots movement. Any system in which established "masters" (Academics in the Russian sense?) are given the authority to regulate it or reform it soon becomes stagnant because the "masters" never recognize when they themselves have ceased to be masterly.

The authoritarian line taken in the column also appears in the suggestion that day-long sessions at scientific meetings normally occupied by twenty or so papers should be given over to four. Who will select these four? One of the "masters"?

Healthy reform comes from the grass roots, from groups and individuals who, through suggestions to and participation in bodies such as the NRC, EMR and provincial funding agencies, through pressure on editors of journals and through the discussion pages of journals can expose superficial science and inadequate granting procedures. We must be careful to maintain a system through which good scientists can receive wide exposure and rise to the top without becoming indebted to those currently at the top.

To conclude, reforms are certainly needed. The suggestion of a journal devoted to one- or two-page articles is a good one. Tighter editing and review are needed not just in Canada but in all journals - certain journals maintain high standards but unfortunately the recent proliferation undertaken largely for commercial reasons by certain large publishing houses means that almost anything can find a home - we must look for a way to stop this commercial exploitation (corruption?) of our discipline. All of us can help in this by being much tougher when acting as referees for papers.

Insofar as granting agencies are concerned, I believe that peer evaluation through the use of external referees is of the greatest help. The NRC Earth Science Committee has moved a long way in this direction and is moving still further. Unfortunately, significantly less than 75 per cent of those asked to referee proposals bother to respond and of these responses about 30 per cent are so superficial as to be worthless.

It is up to all of us to take the trouble to put our house in order and not leave it to our self-appointed (or GAC-appointed) "masters".

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