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CLOSING REMARKS

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HERE we are at the end of the Symposium—a bunch of engineers and scientists trying to establish some sort of common cause. On the one hand we have got research scientists—people who have been described as folks learning more and more about less and less until finally they know everything about nothing. On the other hand we have engineers—in the north of England an engineer used to be described as a chap who can do for five bob what any bloody fool can do for a quid, but this doesn't translate very well these days. Anyway, a new goal of the Society is to bring together these kinds of people for their mutual professional benefit, and for more effective pursuit of knowledge in glaciology. This has been, I think, the main intent behind the Symposium. The people who are here this week hardly need any convincing or converting in this direction; most of us are already somewhat captivated by the material we study and by the places in which it occurs. We know each other, and we have experience in working together. As one old friend said the other day: "This is the finest club in the world". But there is a lot going on outside this particular club, and I would like to consider the implications of the Symposium in that broader frame of reference. The subject is altogether too broad for a review of the technical and scientific details, as we have just seen from this discussion period, so I am going to confine my observations to broad generalities.

My impression is that there are three potential problem areas in the general sense. The first concern is the matter of achieving working relationships between engineers and scientists, not only on an individual basis, but also at the level of the establishment in science and engineering. The second problem area is the communication gap that seems to exist between science and technology where snow and ice are concerned. The third of these potential problem areas is the possibility of inadequate communication between a special research area for ice or snow and the corresponding research area for other kinds of materials. I will try to explain what I mean by these things, but before going on to that, I think we can probably establish some common ground by agreeing on the desirability of blending together science and technology in the snow and ice game, as the Society is now trying to do.

At the present time, financial support for research is dwindling in some areas, and it seems very appropriate to be striving for increased relevance and motivation on the science side and for more rigorous and frugal procedures in engineering research. Treatment of glaciology as an applied science seems likely to bring benefits to both ends of the research spectrum. A long time ago I went to work for an organization that was doing some pretty remarkable things in applied glaciology. The people who worked there were a cosmopolitan and really very highly innovative group, and I do not think there was much distinction drawn between scientists and engineers. Perhaps because of this, it seemed to function rather well. There were challenging engineering problems that provided the stimulation, the direction and the support for good basic research, and in turn the competence in science and research provided sound underpinnings for engineering and also a springboard for innovation. Of course, this place did not excite universal admiration. The name of the organization was the Snow, Ice and Permafrost Research Establishment, otherwise known as SIPRE, but its detractors among the fraternity of handbook engineers translated these initials as Stupid Individuals Performing Ridiculous Experiments—and this brings me to the first of those three points which I mentioned: the avoidance of polarization between science and engineering. A disdain for the lower orders is, of course, not restricted to handbook engineers—there are plenty of pseudo-scientists who see engineers in general as some sort of hairy-handed barbarians, but I think they too are of a certain type. One of my erstwhile colleagues, who shall remain nameless, refused to use graph paper that had twenty divisions to the inch because in his words "we scientists use millimetre graph paper". We have all come across the sort of folk who combat insecurity by aggressively waving their union cards, but we really need not take them too seriously. There is actually rather little difference between creative scientists and creative engineers. They share the same sort of basic training, they have the same sort of breadth in vision and understanding, both use judgement and intuition to identify the important factors of a problem, and they really both have an instinct for what you might call elegant simplicity. To take an example from this University, I suggest that Isaak Newton could well have been a superb engineer. The mathematical bridge that was built to his design at Queens' is a very clever piece of structural engineering, and the measurement of sound velocity by clapping hands in time to the echoes is truly an example of elegant simplicity—and of course it is completely modern by today's standards. So much for the sermon. We can probably all agree that while healthy rivalry is all to the good, divisive attitudes should be avoided like the plague.

My second impression has to do with the communication problem, or to what is known in some management circles as information transfer. A major difficulty that faces practising engineers and planners, is that technical information on snow and ice is not readily accessible. Even if all the scattered journals, proceedings, research reports and other obscure documents can be dug out, it may still turn out that the relevant data are concealed by the way in which they are expressed. What can we do about this? I think this Symposium is one thing—and a very big thing. We have had a meeting of bodies if not of minds and the resulting publication ought to be a very good compact reference source, and one that will lead easily to other reference sources. From the research side of the business, we probably need more publications of the right sort—reviews, monographs and, though I shudder at the thought, perhaps even handbooks. We have to address the right problems and generally make our work sensible, comprehensible and credible. It might help to shed theological preferences and avoid mumbo-jumbo jargon that is sometimes brought from special fields of training. Also in the cause of common language, we can try to conform to the generally accepted conventions when we are preparing graphs and equations, taking a bit of care over the designation of variables, the properties of functions, physical dimensions, boundary conditions, and things like that. From the engineering side, it would be helpful to have problems well defined and also to have them exposed for broad consideration. It is very frustrating to work on an assigned aspect of a problem and then, later, when you are given access to all the facts, to find that the problem itself could be avoided entirely. I have got a very good example of this in mind but I had better refrain from using it here. Restrictions on proprietary information are another nuisance, but I think there are some very hopeful signs in that area, at least in North America. One blessing in disguise may be the strict government control of northern development, because industry is being forced into research and disclosures that probably will return ample dividends in the long run.

The third of these impressions is concerned with the relation of ice research in a particular area to closely related research on other materials. I can perhaps explain what I mean there by examples. In ice mechanics a variety of tests have been adopted and employed, sometimes without a critical evaluation. In some cases, we have had conflicting and puzzling results. These same tests have been widely used and intensively studied in other areas of applied mechanics. Critical experiments and analyses have been carried out, photo-elastic methods have been applied, relevant mathematics has been very thoroughly worked over. Some of these mathematical analyses, incidentally, are classical topics that date back to the last century. The point here is that it is wasteful to fumble around discovering effects and limitations that are already very well known in other fields. Another point is that an engineer's confidence in glaciological research can be undermined by an impression of professional isolation. For example, somebody looking into questions of blowing snow might wonder if he saw there was no reference to work on loose-boundary hydraulics, or sedimentation theory, or things of this kind that are done in the hydraulics field. I could keep going on for a long time with these kinds of examples, and we could also perhaps find cases where broader appraisal of a problem area would be illuminating. For example we have debated the question of the upper limit of compressive strength for ice as the strain-rate increases. In a case like this, maybe we should take a look at the impact stress and stress-wave fracture, and perhaps do some tests with a Hopkinson split bar. Again, the general point here is that we cannot afford to be too narrow in applied glaciology; we have to keep an eye on all the related ice work, and also on all the related work that is going on in other fields.

We have had a long meeting, and I think it has been a profitable one. We have certainly covered a lot of topics, but we still have plenty of others left for the next get-together. We have not given explicit consideration to things like design and operation of icebreaking ships, snow ploughs, or oversnow vehicles. We have largely passed over the problems of construction in frozen ground and construction on, and inside, glaciers and ice caps. We did not get into things like ice forecasting for shipping, aviation problems, or special problems of military engineering; and of course we did not get onto Ed LaChapelle's problem of snow making. So these are all things we have in reserve, and even if we do pick up these topics later there are still plenty more. Applied glaciology still might deal with ice in plant and animal tissues, or even ice in foodstuffs. I think we have probably got ice in beverages fairly well covered.

Before I get carried away by euphoria, I would like to refer back to this morning's session, where we heard of an idea to use ice for mine support in a sub-level caving operation and a most august questioner was concerned that the ice might descend into the workings too fast. I had previously had a look at the problem, and been a bit concerned for a completely opposite reason, wondering whether the ice would flow fast enough in the early stages of the operation, at least with the geometries that were envisaged. Whatever the truth may turn out to be in a problem like that, I would doubt that anyone could make a confident prediction without experimental evidence—and maybe that is the note to end this Symposium on. Glaciological research has already produced a great deal of knowledge that can be of tremendous value to practising engineers, but there is still a great deal to be learnt, and these practical problems are probably going to keep us all on our toes for a very long time to come.