New Year Address from the President

Akira Asaoka
President, Japanese Geotechnical Society

A Happy New Year to all members of the Society. My sincere best wishes for the coming year.

In November, I was in Hiroshima for the 50th anniversary celebrations of the Chugoku Regional Branch where I heard a stimulating special address by Society member Professor Osamu Kusakabe. And a day before that, at the 50th anniversary event of the Kansai Branch, there were exhilarating speeches from Nobel prizewinner Masatoshi Koshiba and the Society's past President Toshihisa Adachi. My affiliation is to the Chubu Branch, and if I had not been serving as president, I would probably not have had the chance of hearing these talks. I warmly recommend any members interested to ask for details from the branch offices.

In my address today, I would like to follow up Professor Kusakabe's address with an assortment of my own habitual sentiments on the same theme. In one place, Professor Kusakabe remarked how scholarship in general, but especially in science and technology, has pursued what can surely be called an extreme course of divisions into specialty fields. Sure enough, taking Tokyo Institute of Technology as an example, we see that from a spectrum of 10 departments that existed in 1949, from Mathematics and Physics to Engineering Management, the number of specialties rose to 22 by 1967, and has since reached 45. But the fact is that specialization brings both merits and penalties; the main merit appears in the rapid advances in science and technology, but on the penalty side, there is a real danger of discrepancies opening between fields, of a break up into specialties even in educational programs, and of a loss of general vision and overview that specialization can provoke among researchers and engineers.

Reversing my order of presentation a little, let me think first about the penalty side of specialization, the loss of general vision and overview. I am reminded by this of something I read 40 years ago in Matao Noda: "Anything too detailed will perish." What this means is that ten thousand monographs of the sort that tell you precisely what conversation Michelangelo was having with which Hussite follower on what street corner in Florence on which day in December 1485,"will never say half as much in their ensemble as a single work (in this case, Burckhardt's "Civilization of the Renaissance in Italy") composed from the original perspective of one historian. As a civil engineer and a scientist of soil mechanics, I want to expand on this idea, in a way more fitted to civil/geotechnical engineering. Taking the leading French civil engineers of the 19th century as an example, and even leaving aside the outstanding case of Dupuit, known for his seminal work in cost-benefit analysis and public economy as well as for his creation of seepage analysis, these technicians as a group were brilliantly versatile, full of creativity, and capable from the start of turning their hand to the most varied problems of "structure," "water," "soil" and "planning." The Japanese engineers who contributed to postwar reconstruction were of exactly the same stamp, able to take on projects of the greatest diversity -- the bullet-train and the highway network, the complete surfacing of all the local road systems, urban improvements, water and sewage schemes, hydro- and thermoelectric plants — and to accomplish all of these things at the same time. There were obviously no manuals available for works like these in the late 1950s and early 1960s; these men must simply have been all-round generalists. The path of civic virtue is said to consist in putting concern for others before your own comforts, but another thing commonly said is that individuals of virtue come in no mold: they are not such specialists that they cannot do other things. This was the path, I think, that our engineering predecessors were so intently following.

Specialization is the wisdom of the day, however, and since the expansion policy in scientific disciplines in the mid 1960s, civil engineering research, especially in universities, also has pursued its course of extreme segmentation right up to the present. Thanks to the increasing divisions, there are few academics left in the lecture halls now who still have an overview of civil engineering as a whole, and this explains the total confusion that breaks out the moment someone comes out with a "declaration to get out of a dam" or whatever. But is it any wonder? In this 21st century, when science and technology are so specialized and divisions are growing deeper, can the civic virtues of the previous century still be expected from people who are supposed from the start to be specialists? Part of this problem may be that history and economics are originally humanity subjects. Also, new issues are being supplied by life science, human rights and environment studies, space research, and other disciplines which were still immature in the 20th century even with respect as to whether they were humanities or sciences. For a specialized research society like ours, it will be a more urgent task in the future than we can at present imagine to provide information actively and accurately for other specialist societies and for civic society in general. But it is also a fact that there are engineers and administrators making truly statesmanlike efforts in this area already. We welcome inquiries from them, and assure them of our support when wanted.

What can be said, though, of the merits of field specialization? Is it really true that specialization has brought progress to the science of soil mechanics? In his talk, Professor Kusakabe sought the origins of field specialization in the 17th century reductionism of Descartes. The actual events and phenomena of the world are exceedingly complex, and the minuteness of facts reflects this complexity. As there is no way of dealing with these things as they are, events and phenomena needed to be divided up into distinct "elements," which we can then examine closely in their ramified details. This procedure is called analysis. By way of analysis, we come to a view of something resembling "truth." This is reductionism. However, Descartes also says that it is necessary to verify whether this picture of truth is correct or not by means of integration. It is not enough merely to explain the behavior of elements as they are found; it is just as essential to see whether relations to behaviors in other elements can be explained, and whether things previously regarded as separate facts turn out in the end to be explicable as the obverse and converse sides of the same coin; in these ways, Descartes argues, it is possible to confirm whether a truth holds correctly or not. And it is only by means of this kind of truth, or "theory," that human beings can hope to attain a whole understanding of the complex phenomena of reality. A theory is not just a model of elements, therefore. Professor Kusakabe went on to say that only a true theory can enable human beings to predict phenomena that are not yet known, and that exact experimental science is the means for verifying predictions of this kind.

What are the implications of this for soil mechanics? Taking the example of sand, one sand soil is not like another; an unlimited number of different states can be easily created in which the sand ranges from extremely loose, through medium loose and medium dense, to dense. But when sand in each of these different states is subjected to shearing without any volume change, surprisingly different outcomes are produced in the same sand material. Up until recently, soil mechanics simply capitulated in the face of this complexity, and either constructed a special "constitutive equation for loose sand" or else assumed distinct "parameters for dense sand," although in fact it was the same sand in both cases. In drained shearing conditions, under which there is a change in volume, this meant that no adequate account of the process could be offered. Dense sand is produced from loose sand by means of compaction. But as this is the same in effect as drained shearing, researchers were inclined to leave processes like compaction outside of their scope of inquiry. Putting this pointedly, it means that up until practically yesterday it was considered permissible to describe the liquefaction of loose sand using a constitutive equation that was not properly capable of accounting for compaction in the very same sand. And in fact this amounts to saying that the argument that a sand loose enough to be easily compacted is then/therefore liable to liquefaction in the event of an earthquake is really no more than an explanation of convenience to offer to the public. A further point to make is that after an earthquake has occurred, liquefied sand undergoes considerable consolidation under its own weight. Yet the constitutive equation employed to calculate the liquefaction effect is wholly incapable of accounting for consolidation subsidence, making it necessary either to change the model details, or to replace the parameters. In other words, for the stages after the earthquake, the special model for the liquefaction of sand needs to be supplemented by a different special model for consolidation subsidence. In this way, led by a blind faith in ready expedients, more and more divisions in procedure are introduced, and special models are multiplied indefinitely. But in fact the whole ensemble of these models will never be a match for the unknown “single work” of the future, which in this case has to be a theory about the certain something that is distinctive of sand. Remaining satisfied with models for elements while hanging back from the endless task of disentangling the complex relations of facts – what other name can be given to this, if not bad habit?

I have so far talked about sand, but the same points can be made about clay. It is still commonplace at the moment for a constitutive equation valid only for the loaded state of remolded normally consolidated clay to be used for calculating consolidation deformation in naturally deposited clay soils, the ordinary state of which is one of overconsolidation. While there may have been some questioning as to whether this practice is apt or not, there has little theoretical discussion of problems such as disturbance and sensitivity ratios. No standards exist yet for sensitivity ratios in measurement in our societies. Another obvious point to make is that soils are not limited to sands and clays alone. Between the two, there is a dense variety of other soils. Concerning these intermediate soils, however, all that has been published so far is in one special issue of “*Soils and Foundations*”; thus, while a mechanics of sand and a mechanics of clay can be said to exist already, a more general science of soil mechanics still remains to be set up. It is time that we pulled down the hedges between special models, and approached general theory in a spirit of “where sand responses are understood, clay responses can be understood too.” While it may be true that specialization has been a conspicuous part of science and technology since the 19th century, it is quite wrong to overlook the fact that knowledge has been kept general and universal at the same time by dint of theory.

Another thought about this same matter is that while there are all sorts of specialized tools now available, including tools for consolidation deformation, bearing capacity, liquefaction and so on, the decision whether to use a consolidation tool, for example, depends on knowing ahead by some other path of insight that consolidation is going to occur in a particular soil. But no one special tool exists for telling you what will take place in this or that soil. Will there be consolidation deformation or landslide failure? Will the soil undergo liquefaction or compaction? What will go on happening after the earthquake is over? It is only by a theoretical calculation of the history of soil change over time that one can begin to forecast the succession of events that will occur in a given soil. If this is what theory is, it has to be said that however excellent Terzaghi and Taylor’s textbook may have been when it came out, its way of treating seepage as seepage, consolidation as consolidation, and failure as failure is no more than a stage of pre-reductionism, and not a work of soil mechanics theory. A textbook that dates back to 1948 has reached its limit of use. 40 years have also gone by since the appearance of *Critical State Soil Mechanics* and *The Finite Element Method*, both in the same year 1968. It is time for a new challenge now.

Recently, there has been a good deal of talk about “cross-field integration.” But this too is only likely to bear much fruit when it is pursued in line with the intrinsic development rules of a theory. In the November issue of this journal, Society member Professor Ikuo Towhata published a penetrating and entertaining article on this subject. At around the same time, I attended the 4th International Conference on Scouring and Erosion in Tokyo, which was organized by Professor J. L. Briaud of Geo-Institute, ASCE. Numerous researchers in river and coastal engineering were also there, and the event was a great success. But even assuming one is conversant with critical/limiting hydraulic gradients, with so few geotechnical engineers closely engaged in the investigation of water induced “drag force”/“shear” on the surface of a soil as yet, all one can hope for is a favorable development of integration in this area in future.

This year, the Japanese Geotechnical Society is celebrating its 60th anniversary. The executive board has been busy over the past year with energetic arrangements for a packed program of events. The list below is an approximate roundup of what is planned.

(1) 60th anniversary Geotechnical Symposium, (2) 60th anniversary National Geotechnical Conference with commemoration ceremony, (3) 60th anniversary events to be held by regional branches, involving the public, (4) 60th anniversary commemorative issue of the Society journal,

and 60th anniversary round-table discussion, and (5) Other celebrations.

In connection with (1), it is planned to invite a large number of papers on the topic "The Strength of Geotechnical Engineering and its Contribution to Society"; prizes will be offered, and recommended papers will be chosen for inclusion in the journal. For (2) and (3), all regional branches have been asked to set up projects with public appeal. Regarding (4), this is only an example, but one idea would be to extend feelers outside the Society by choosing a topic such as an introduction to recent innovations in measurement science and technology, or integrated cross-field research. Planning is also under way for a round-table discussion on the problems of the new public-service corporations and the expansion of geotechnical engineering into new areas. As for (5), the IS-Tokyo, IS-Gifu and IS-Kyoto functions planned for this year will be held as 60th anniversary events. The study on *Foundation Accidents Due to Earthquakes, Floods and other Disasters*, in preparation since the presidency of Professor Tatsuoka, will be published as a commemorative volume. And finally, a DVD version of almost all past papers of the Japanese Geotechnical Society will be brought out. This DVD will be a little expensive, but it is sincerely hoped that not only institutions, but also a good many individual members will be able to buy it.

I have appealed in this issue for public comment, but I ought also to mention that the application is now in preparation for the Society to be re-registered as an organization of public benefit. "Public benefit" means that we in this Society are working for the good of this country and its people, but turning the relation around, it also means that society can count on us to create a more accurate and useful science of geotechnical engineering. This point is essential and must not be forgotten. It only remains now for me to extend my best wishes for the new year to all Society members, and to wish them health and prosperity in all their activities.

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