

General Discussion

L. F. Winterkorn, Chairman:

We have gone a long way trying to find solutions for our macroscopic soil-water problems by passing to systems of ever smaller dimensions and ending up with the proton spin. Now, we have to ask: What good is all this for working engineers? Or rather, which available concepts and facts are directly useful to us and what is the best way of using them?

First of all, the laws of material behavior on a certain level of structure in order to be useful must be derived or at least expressed on the level of application. To gain a better understanding of such laws and obtain a mastery of their use, it is often necessary and always desirable to underpin them with knowledge derived from the study of a lower level of structure. Thus, we underpin the laws for the macroscopic level by those obtained at the microscopic, and so on, until we arrive at the nuclear or even intranuclear level. In this procedure, two important points must be realized:

1. Even if we understood perfectly the properties and behavior of the components of a system, the behavior of the system is not a simple summarization but contains an organizational or structural factor that may not be obvious a priori. Even in the relatively simple case of gases, one may have to use Maxwell-Boltzmann statistics under one set of conditions, Bose-Einstein statistics under another, and Dirac statistics under still another set.
2. There exists no a priori reason why the laws derived and valid on a particular dimensional level should be of a greater intrinsic certainty than those derived on a level of larger dimensions. As a matter of fact, the most exact physical laws were derived from the behavior of heavenly bodies whereas, on the other end of the size scale, one of the most important findings of modern science is the uncertainty principle of Heisenberg.

The lesson to be drawn from this situation is apparent. With all the justified eagerness to learn from the knowledge of material behavior on the levels of smaller dimensions, with all the acknowledged need for such knowledge to serve as underpinning for a better understanding and optimum use of soil on the dimensional level on which we work, it must be kept in mind that all this knowledge is ancillary to the job of coordinating this knowledge into practical phenomenological laws that are valid for engineering purposes. Engineers, especially the workers in the purer sciences, cannot be expected to do this for themselves. Much of the details furnished by them will, of course, have to fall by the wayside. To do this job, there is need for your cooperation if only in the form of questions, but any other form will also be appreciated.

I was somewhat taken aback when I heard the review of my paper entitled "Mass Transport Phenomena in Moist Porous Systems as Viewed from the Thermodynamics of Reversible Processes." This title may have been somewhat presumptuous and not entirely fitting, but it was intended to be provocative. Here is a good and promising method but some people have blown it up too big and promised too much without laying down the strict conditions necessary if the promises are to be realized. I tried to show the use of this methodology in the larger field of thermodynamics and to indicate the type of experimental data that has to be accumulated before this methodology can be honestly and sensibly applied for our purpose. I am happy to be able to state that Dr. Taylor of the Agricultural Experiment Station of Utah is already busy along these lines and that the proceedings of the Seventh International Soil Science Congress will contain a paper by him on this subject. Probably, additional papers by him and his co-workers will be published in the Proceedings of the Soil Science Society of America.

L. H. Fink, Philadelphia Electric Co.:

Our company has been very much concerned with the movement of moisture in soil under the influence of thermal gradients because of the influence of such movement on the thermal resistivity of soils and consequently on the heating of our underground cable systems.

I would like to ask for at least a qualitative statement of opinion on a question on the lack of agreement concerning the relative importance of movement in the liquid and vapor phases. Reading the papers in this symposium and related papers in the literature, I have gotten the impression that the evidence, at least that presented by the proponents of movement in the vapor phase, tends to indicate that it is impossible for there to be much movement in the liquid phase in open systems—at least for there to be enough to cause any appreciable results. On the other hand, the evidence presented to show that movement in the liquid phase is possible, could perhaps be interpreted as also supporting the Smith-Philip-De Vries hypothesis that movement is stepwise in vapor and liquid, alternately. At the same time, this hypothesis could also be consistent for instance, with the results obtained by Mr. Flood, referred to by Prof. Low.

The question is, is it correct that the Philip hypothesis presents a consistent explanation for the otherwise apparently conflicting data cited by the proponents of the vapor and liquid movement hypotheses, respectively?

P. F. Low, Department of Agronomy, Purdue University:

My own profession is largely the physical chemistry of soils more than the transfer of water by mass flow; therefore, I do not regard myself as an expert in this area. It is a possibility in Mr. Flood's work, of course, that part of the movement was in the vapor phase. The question arises whether in the voids of his carbon rod he had a vapor transfer between necks joining the carbon particles or between water necks at the point of contact between the carbon particles. I can only answer honestly that I do not know. Dr. Rollins has reviewed some of this work.

I would like to make one general comment in this regard: The path by which water moves through a porous system, whether it be in the films along the particle surface or in the vapor phase in the voids, will depend on the energy necessary for that movement. It can be visualized that a moving molecule has to surmount a potential energy barrier in going from one equilibrium position along the path of movement to the next. If movement along the liquid surface requires movement over a smaller potential energy barrier than movement in the gas phase, then movement will occur in the liquid phase. The path that is taken will largely depend on the energy required for movement to occur. As the liquid films get thicker, the outermost layer of the water molecule should move with less and less resistance because the forces of adsorption would decrease. It is conceivable that both mechanisms might occur simultaneously.

One other factor, and Prof. Flood made this point, when there is an adsorption potential so that the potential energy of the water is less next to the clay surface or the carbon surface than out in the middle of the pore, if there is a vapor pressure gradient in the center of the pores, this gradient may be a million times less than that along the surface, because the pressure build-up toward the surface might be a thousand times and when there is one vapor pressure, for example, at the inlet face and another at the outlet face and a given vapor pressure gradient in the voids, the fact that the vapor is adsorbed on the surface and its pressure can build up on the surface by reason of the attractive forces can lead to a gradient along the surface that might be a million times the gradient in the vapor. Even though the viscosity of the water on the surface is maybe a hundred times higher than that of normal water, according to Prof. Flood there is so much larger gradient present that water can still flow in the films.

The problem may be looked at in terms of Darcy's Law for unsaturated systems. In the films there is a conductivity, a transmission constant, that in the saturated state is called the permeability or conductivity. That constant may decrease a great deal as the surface of the clay particles is approached. Thinking of Darcy's Law, the flow would be expected to go down. But as the surface is approached, the pressure gradient could go up far more than the transmission constant could go down, or might go up

more than the transmission constant would go down, with the net effect that the transfer along the surface might be a lot higher than would be normally anticipated. I hope that Prof. Rollins will care to comment because he has worked in this area.

R. L. Rollins, Brigham Young University:

In addition to what Prof. Low has said, I would like to make a couple of comments. The type of system must be considered when we examine any research that has been performed in this particular field. After reviewing the research of the workers who have been proponents of the vapor transport theory, it seems that they have been dealing with soils that are primarily nonplastic or of very low plasticity. Under these conditions it would be expected that vapor transport would be the predominant mechanism of flow. On the other hand, the work of Dr. Winterkorn, and perhaps of one or two other individuals, was performed on systems that were highly plastic or bordering on the montmorillonite-type clays. Under these conditions, it would seem that the predominant mechanism would be flow in the liquid phase.

A few years ago, I performed some research on the movement of soil moisture due to a thermal gradient which attempted to measure quantitatively in an open system the amount of vapor flowing. The soils used were of low plasticity. Comparing the observed flow rates with the theory advanced by Philip and De Vries, there appears to be a fairly good correlation between theory and experiment, except below air-filled voids of 5 percent. For systems below this percentage, the correlation was not good. Above this figure, there is a fairly good correlation. The type of system dealt with in attempting to make any general conclusion as to the type of flow mechanism must be considered. I agree with Prof. Low that it is possible to have systems where both of these mechanisms would occur to some degree simultaneously, with liquid flow in highly plastic systems and vapor flow predominating in the less plastic systems.

Sereda, Division of Building Research, National Research Council of Canada:

We must realize that Prof. Flood's work, discussed by Prof. Low, and thermomigration of moisture in soils, discussed by Mr. Penner, are two different cases. Dr. Flood worked under isothermal conditions. He established a high vapor pressure at one end of a carbon rod and a low vapor pressure at the other. By condensation, a thicker film was formed on the internal carbon surface at the high pressure end than at the low pressure end. This resulted in a suction potential under which the condensed water tended to redistribute itself. This procedure is analogous to putting together at the same temperature two samples of a material, one of which has a higher moisture content than the other.

The situation about which Mr. Penner was speaking is quite different. In systems where the large, thermally activated flows have been observed, it is a matter of dealing with near-saturated conditions in which the relative humidities approach 100 percent. In the experiment, a homogeneous sample of clay or sand at a given, uniformly distributed moisture content, which usually corresponds to the high pressure end of the isotherm, is taken and then different temperatures are applied to the ends of the sample. Under these conditions, the high spreading suction obtained under isothermal conditions for different moisture contents of the samples cannot be expected, as in Dr. Flood's experiments.

But this does not mean I am arguing that there is no liquid flow involved when a thermal gradient is applied to a moist soil sample. Prof. Winterkorn has already pointed out that this isotherm changes at different temperatures, as in fact it moves up with a decrease in temperature. He has published his experiments and his reasons for thinking that there should be some liquid flow.¹ His pertinent statement in HRB Special Report 40 says that the water "shifts" in the sample because of the change of isotherms from one temperature to the other.

¹ Soil Science, 38:No. 4 (1934); 40:No. 5 (1935); 41:No. 1 (1936). HRB Proc., 27:443-445 (1947).

H. F. Winterkorn:

I would still like to have some more discussion about the problem brought up by Mr Fink, for what may appear to be fine points of theory are actually decisive factors with respect to proper design of soil structures for various purposes. Mr. Fink's problem and that of the electric power industry is how to prevent the drying out of soil around buried power cable conduits. Such drying lowers the thermal conductivity of the soil, which in turn leads to overheating and eventual burning out of the power cables with great direct and often greater indirect economic loss.

C. M. Johnston, Bureau of Public Roads, U.S. Department of Commerce:

There seems to be an argument between one group and another as to whether thermally activated water movement went through the vapor or liquid phase. Possibly, both groups are right. Now, one speaker said that the adsorbed water was held so tightly that water movement had to take place in the vapor phase. In such tightly held films, would it be possible for a molecule at one end, possessing a large amount of free energy, to bump all the way through so that this molecule would take the place of its next neighbor and this, that of its neighbor, and so on until finally on the other end there is a free molecule?

H. F. Winterkorn:

The possibility of such a mechanism has been discussed in the literature as far back as the times of Grotthus, but there are some serious objections if the hydrogen linkage of the water molecules is taken into account. There are, however, a large number of possible and actually occurring types of flow in these systems. As a reference, the extremes of "plug" flow as in certain electro-osmotic systems and of molecular diffusion in zeolites have been studied by M. H. Hey in England and results published during the early thirties in the Mineralogical Magazine. Hey's work and conclusions might be pertinent with respect to moisture movement in hardened cement pastes.

E. Penner, Division of Building Research, National Research Council of Canada:

To add one further note on the confusion between liquid and vapor phase, it seems that at the time when Gurr, Marshall, and Hutton² published their work, they showed fairly definitely that if salt is added to a moist soil and then a temperature gradient applied to this soil, all the salt accumulates at the hot end. What this means is that there cannot be any continuous liquid flowing from the hot end to the cold end. It must all have gone through a vapor phase. Now there is certainly "back-wicking," which is flow from the cold side to the hot side. In the case of Gurr, Marshall, and Hutton, this was done with closed systems. It is true that we did not use material—in our open system—which would be akin to the clays referred to, but rather to silts and sands. What Gurr, Marshall and Hutton used was a loam.

H. F. Winterkorn:

A loam and washed fine sand.

E. Penner:

There was no evidence at all that the complete movement is in the vapor phase from the hot to the cold end under temperature gradients. This means that there can be segments of liquid but they are always separated by a vapor phase. These gaps are in fact barriers to salt movement from the hot side to the cold side.

H. F. Winterkorn:³

The results of the experiments by the Australian workers are not as clear cut as they appear to be and lend themselves to interpretations that are at variance with those that have been offered. Also, as has been stated, they were performed on loams and sands.

² Soil Science, 74:No. 5, pp. 335-345 (1952).

³ The following comments are an expanded version of the oral discussion in order to avoid low specific reference to the actual experimental data of Gurr, Marshall and Hutton.

not on clay soils. The physical characteristics of the materials used by the Australian workers were as presented in Table 1.

Enclosed cylindrical soil samples 10 cm long and 14 cm in diameter were submitted to temperature gradients of about 1.6 C per cm. One set of tests with the loam soil involved 10 different moisture contents ranging from 1.7 to 24.4 percent subjected to the temperature gradient for 5 days. Maximum differences in water content at the end of the tests were observed in the samples of the original uniform moisture contents of 5.3 and 7.9 percent, respectively. This, according to the authors, placed the maximum

TABLE 1

Properties	Loam Soil	Washed Fine Soil
Particle size distribution (percent):		
2-0.2 mm	1.9	20
0.2-0.02 mm	50.2	80
0.02-0.002 mm	31.3	0.0
<0.002 mm	16.6	0.0
Total soluble salts (percent)	0.077	—
Chloride content/100 g of soil in me	0.14	—
Moisture equivalent (percent)	20.0	2.5
Permanent wilting (percent)	6.3	—
Plastic limit	15.8	—

or water transmission at about one-third of the moisture equivalent or about 6.7 percent of water. The permanent wilting point is at 6.3 percent.

The experiments made at 5.3 percent moisture content showed a maximum in chloride content about 3 cm from the cold end, those made at 7.9 percent moisture showed chloride peak at the same distance and a maximum value at the hot end. At higher moisture contents, the chloride contents form a plateau lying between 3 and 7 cm from the cold side, the height of the plateau corresponding to from about 7 to 10 percent of the original uniform chloride content. The highest absolute values for chloride concentration at the hot end occurred in the samples with original uniform moisture contents of 14.3 and 16 percent, respectively. Significantly, practically no water shifting took place at these moisture contents.

An experiment in which this loam soil was submitted at a water content of 11.3 percent to the temperature gradient for 18 days resulted in a reduction of the chloride content to about 15 percent of its original content in the colder half of the specimen, to about 40 percent at 7 cm distance from the cold side, and increased it 7 times the original concentration at 1 cm from the warm side. The accompanying water shift was only one-half that obtained in the 5-day experiment at the optimum water content for water shifting.

Ten-day experiments with the washed sand to which NaCl was originally added at the cold end made at 5.8 and 7.9 percent of water, respectively, gave an irregular pattern of water shifting with differences between cold and warm side of 1 and 0.5 percent, respectively, and a chloride concentration peak at a distance of about 5 cm from the cold side and a maximum concentration at the warm side for an initial water content of 8 percent. For the sample with 7.8 percent water, there was an absolute chloride concentration maximum at about 3 cm from the cold side, and the absolute minimum concentration fell within the first 2 cm from the warm side.

It should be quite obvious that these experimental data have as yet not been completely evaluated; also, that the addition of NaCl has complicated rather than simplified the interpretation of the experiments.⁴ To the speaker, the most significant fact brought

The system may be further complicated by "Soret" effects and by related phenomena that are treated in "The Structure of Electrolyte Solutions," Walter J. Hamer, Ed., John Wiley (1959).

out in the Australian experiments is that maximum moisture transfer occurred at water contents at about the permanent wilting percentage. Up to this percentage, the water is in such a state of organization or restraint that it cannot be utilized by plants. Lowering the temperature will bring more water into this restraint and increasing the temperature will bring less. Organization of molecules into a restrained structure, which is akin to crystallization, results in decrease or loss of its solvent properties, a phenomenon utilized in purification of chemicals by zone-melting.

Taking a soil sample of uniform moisture content at about the wilting or similarly significant point (indicating that most of the water molecules are in a restrained condition at the particular temperature), make one end of the sample warmer and the other colder. Because the restraining power of the mineral surface increases in depth with decreasing temperature and vice versa, the water will shift from the warmer side having less restraining power, to the colder side. If more water is available than necessary to satisfy the mineral surfaces, then this will be distributed in uniform thickness above the restrained layer. If this free water is of a sufficiently large amount, its presence may hide the fact that larger amounts of the total water are under restraint on the cold than on the warm side and the distribution of the water substance will remain uniform. On the other hand, because the strongly restrained water may not serve as solvent, only the free water can contain the electrolyte and, as in the Australian experiments, a great shift in electrolyte content may occur without apparent shift in total water content. There will be, of course, special effects at the ends of the samples due to the abrupt change in condition and to "thermal shock" at the beginning of the experiment. This also is exemplified by the Australian experiments as well as by those made by Trejo for his 1946 master's thesis in the speaker's laboratory at Princeton.

I would like to state categorically that those of us who believe in the indicated water shift toward the cold end in the liquid phase do not deny contributions by vapor transport and other means. Rather, because evaporation and condensation phenomena are so apparent and are ever-present in our daily life, their very obviousness seemed to make it unnecessary to talk much about them, especially because they are well covered in the classical soil science literature. There is sufficient experimental evidence to permit the statement that every transport mechanism imaginable in accordance with established physical laws will actually participate in the total phenomenon to an extent that is controlled by its activation energy. It is the degree of participation that is still to be established for different materials and conditions. Of considerable pertinence in connection with the effect of thermal gradients on moist soils is the symposium paper by Kolyaso and Gupalo. With respect to thermal conductivity of moist soils, the speaker expects to show in a forthcoming paper⁵ the necessity of having several transportation mechanisms acting at the same time in order to account for the actual magnitude of the total effect.

⁵ "The Behavior of Moist Soils in a Thermal Energy Field." 9th Annual Clay Symposium, National Research Council (1961).