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FIFTY YEARS PROGRESS IN SOIL PHYSICS

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ABSTRACT

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Despite a lengthy period of non-progress, soil physics has, over the last half-century, gained enormously in its self-confidence, its intellectual power, and its relevance to the practical problems of the real world.

INTRODUCTION

Activity in the natural sciences, whether measured by the number of scientific journals, the number of abstract journals, the number of scientific papers, or the number of scientists, has grown roughly exponentially over the last three centuries, with a doubling time of about fifteen years or, equivalently, by a factor of ten every fifty years (Price, 1961).

There seems no reason to suppose that the growth-rate of activity in soil physics has differed significantly from that of the natural sciences in general. In Western countries, at least, enthusiasm for support of the natural sciences has waned somewhat over the last five years or so (e.g. Ashby, 1971), and the growth-rate may well have fallen to half the Price value over this recent period. On that assumption, the intensity of activity in soil physics in 1974 is still *nine times* that in 1924 when the I.S.S.S. was founded. Further implications of this pattern of growth include the following: about 90% of all the work ever done in soil physics has been done in the period 1924—1974; and the soil physicists living today constitute rather more than 80% of those who have ever lived.

In these circumstances, an adequate account of the last fifty years' progress would demand no less than a full monographic treatment of the whole substance of soil physics. There is clearly no possibility that a single paper can meet that need. This article should therefore be considered as simply my personal view of how the character and content of soil physics has changed over the last half-century.

Before we turn to developments over the last fifty years, however, let us first consider briefly the early history of man's involvement with physical aspects of the soil.

HISTORICAL

Men have tilled the soil and irrigated it and drained it for at least six millennia. These enterprises have been (and remain) basic to civilization. They were an essential element in the first known civilizations: those of Mesopotamia (Kramer, 1958; Whyte, 1961), of pre-dynastic Egypt (Hamden, 1961; Whyte, 1961), of Syria and Palestine (Whyte, 1961), of Iran (Whyte, 1961), of the Indus Valley (Bharadwaj, 1961; Whyte, 1961), of Siberia (Kovda, 1961), of North Africa (Despois, 1961), of Mediterranean Europe, of China and Japan, and of pre-Columbian America (Armillas, 1961).

The use of physics in men's works with the soil had, necessarily, to await the emergence of physics itself. Precepts had been abroad, of course, which make good physical sense. The earliest known farmer's almanac, the "Sumerian Georgica", composed more than 4000 years ago and set down in cuneiform on clay tablets, includes instruction to the farmer to keep trampling oxen and other prowlers off newly irrigated soil (Kramer, 1958). And 2000 years ago, Vergil ("Georgics", Book 1, lines 89—90) suggested that, on some soils, the benefit of burning stubble is that "the heat opens up fresh ducts and hidden pores through which the juices of the soil may move to the growing plants".

Keen (1931, Ch.1) gives an account of early, pre-scientific, treatises on cultivation, of which Fitzherbert's "Boke of Husbandry" of 1523 is a notable example. The natural sciences, including physics, essentially began in the latter half of the 17th century, and it is of interest to see what the philosophers (as those first scientists called themselves) made of the soil. John Evelyn, the diarist, was an Original Fellow (and an early Secretary) of the Royal Society of London. During 1675 Evelyn gave two lectures to the Society entitled "A Philosophical Discourse of Earth, relating to the Improvement of it for Vegetation and the Propagation of Plants". The Society ordered the discourse printed and it appeared the following year (Evelyn, 1676).

This substantial treatise reports, inter alia, his observations "with an indifferent Microscope" on various soils, unwashed and washed, unground and ground. Its content of physics is slight, though Evelyn, like Vergil, recognizes the vital role of pores and pore geometry. He writes: "Clay is . . . a cursed step-dame to almost all Vegetation, as having few or no Meatuses for the percolation of the alimental Showers, or expansion of the Roots." Leibniz's "Dissertatio de Arte Combinatoria" had been published at Leipzig in 1666, and Evelyn reports the consequential first essay into numerical soil classification: "Those who have written *de Arte Combinatoria*, reckon up no fewer than one hundred seventy-nine millions one thousand and sixty different sorts of Earths." (Can modern pedology refute the calculation?)

Systematic scientific study of agriculture did not really get under way until the first half of the 19th century, and physical studies of the soil were a large part of the early work. Despite their titles, Sir Humphrey Davy's "Elements of Agricultural Chemistry" (1813) and, later and more particularly, G. Schübler's "Grundsätze der Agrikultur-Chemie" (1838) had a substantial content of physics.

The early impetus in soil physics was, however, soon spent. The promise of a totally chemical theory of soil fertility emerged in the period 1834—50 through the work of Boussingault in France, Liebig in Germany, and Lawes and Gilbert in England; and soil studies became almost exclusively chemical. Toward the end of the century, however, the limitations of a purely chemical approach were clear to some. In the U.S.A., especially in connexion with soils of the arid and semi-arid regions, physical studies were pioneered by Hilgard, King, and Whitney; and the journal edited by E. Wollny "Forschungen auf dem Gebiete der Agrikultur-Physik" was a focus of soil-physical work in Continental Europe over the period 1878—98. Warrington (1900) gave an excellent account of the state of soil physics at the end of the century. His book consists of lectures given in 1896 and so makes no reference to Briggs (1897), which I consider later.

SOIL PHYSICS FIFTY YEARS AGO

There was significant progress in soil physics in the period between 1900 and the foundation of the I.S.S.S. in 1924. Its importance was not appreciated at the time, however, and it had no impact on international grasp of the issues in soil physics (let alone its practice) by the end of the 1920's. I shall return to this development later, but it has no place in this section, which is an attempt to look at the state of soil physics fifty years ago, as revealed in the early deliberations of the I.S.S.S. and its Commission I. The basic source material includes the Proceedings of the First International Congress, held in Washington in 1927; and the commentary on the papers of Commission I at that Congress by Keen (1928) is useful.

The place assigned to physics within the I.S.S.S. fifty years ago will appear odd to some present-day soil physicists, but it is readily understood when we recall the origins of the Society. The primary thrust for international organization had come from the pedologists, stimulated as they were by the prospect of a very general scheme of soil classification transcending national boundaries and spanning all continents. It should not occasion surprise, therefore, that the primary demand on physics was for methods of analysis and observation which would fit in a simple and tidy way into the pedologists' procedures for surveying, naming, and classifying soils.

"*Mechanical analysis*"

One thus finds that the greater part of Keen's (1928) "survey of the present position of soil physics" is devoted to the discussion of "mechanical analysis", the term then used for particle-size analysis. Mechanical analysis, in various forms, had been the major systematic means for the physical characterization of soils from the time of Sir Humphrey Davy onward. It took no account, of course, of the viewpoint hinted at long before in the writings of Vergil and John Evelyn, amongst others, that it was the size and configuration of the *pores* rather than of the *particles* which were the prime physical determinants of soil behaviour. In the event, it had been so much more obvious (and had seemed so much more simple) to find something quantitative to say about the particles than about the pores. With commendable frankness Keen (1928), however, concluded his comments on mechanical analysis with the sentence: "Nevertheless, the correlation of mechanical analysis results with field behaviour appears destined to remain indefinitely in the qualitative stage." For all that, mechanical analysis remained an obsession of Commission I right up to World War 2.

Gradually, however, there came recognition that the preoccupation with mechanical analysis represented a dead-end. Schofield and Russell (1947) wrote: "The particle size distribution of a soil has only limited significance for agricultural research work. It helps to place soils in categories such as sands, loams or clays, but this can be done almost equally well by the feel of the soil between the fingers and under foot . . . The physical properties of soils that are of importance in soil work are mainly concerned with the pore size distribution in the soil." And Childs and George (1948) observed that "Physically a soil may be regarded as completely specified by the geometry of the interface between the solid component and the void space together with that of the air—water interface within the voids." These were not, of course, isolated revelations; but I must defer reference to related developments until their proper place later in this article.

"Single-value" physical characterization

Awareness that mechanical analysis provided, at best, a qualitative indication of the field behaviour of soils had led, by the time of the First Congress, to the search for a "single-valued" physical characterization of soils. Potential candidates included the "hygroscopic coefficient", the "wilting coefficient", the "moisture equivalent" and the "sticky point". The hope, presumably, was that a single number might be found which would be simpler than the set of numbers obtained from the mechanical analysis and yet at the same time possessed of greater physical meaning. Keen (1928) expressed some scepticism at this search for a chimera and accepted the view that "no single method serves adequately to distinguish a soil. We may ascribe this to two reasons: firstly, the complex nature of the material; and secondly, the empirical nature of many of the single-value determinations. The time is ripe for a thorough examination of these methods to ascertain what physical property — or more generally, combinations of properties — are really involved in each method."

The follow-up effort for cooperative work within the framework of Commission I (Keen, 1930) seems to have involved more labour than illumination; and general insight into the meaning and the limitations of various "single-values" had necessarily to await wider appreciation of the physics of soil water, a matter treated at a later point of this paper.

Soil colour

Another early preoccupation of Commission I of the I.S.S.S. was soil colour (Arkangelskaya, 1930; Shaw, 1934). Here also the goal was to provide the surveyors and classifiers with a technique (and in this case a nomenclature as well).

Soil colour studies are, of course, quite peripheral to the central problems of the physical processes of the soil in nature. The effect of natural soil colour on these processes is minimal and indirect, amounting to a possible small effect on albedo and thus on the nett radiation balance at the surface of bare soil.

PHYSICS OF SOIL WATER: THE FOUNDATIONS

Many of the peculiarities and difficulties of soil physics, as it is represented in the early publications of the I.S.S.S., seem to have stemmed from a fundamental perplexity as to how to proceed with the proper scientific study of the physical processes involving soil water. A powerful conceptual basis for such progress had, in fact, been laid down in the literature as early as 1907; but it so happened that in the succeeding two decades few soil physicists grasped its significance and none succeeded in building on it creatively. Such eras of non-progress doubtless befall many fields of natural science and should occasion little surprise. In general, it is probably idle to speculate on the causes. In this instance, however, two men played major parts in the sequence of events; and it may be instructive to look at them in some detail. Over and beyond its factual significance, their story represents a parable for soil physicists and, indeed, for scientists in general.

L.J. Briggs and the influence of his work

In 1896 Lyman James Briggs joined the newly constituted Division (later Bureau) of Soils of the U.S. Department of Agriculture as Physicist-in-Charge of its Physical Laboratory. He was only 22 years old, but within a year Briggs (1897) had published the bulletin "The mechanics of soil moisture", which was to determine the generally accepted approach to the physics of soil water for at least four decades. Briggs wrote: "The water contained in a soil may be considered to be of three kinds — gravitation water, capillary water, and hygroscopic water. Gravitation water is that portion which is in excess of the amount which the soil is able to retain under existing conditions, and is consequently free to drain away. The capillary water is that part which would be retained in the capillary spaces under these conditions, and which is capable of movement under capillary action. The hygroscopic water is that found on the surface of the grains, which is not capable of movement through the action of gravity or capillary pores." Briggs ornamented these assertions with general qualitative discussions of gravitation, capillarity (including surface tension and the properties of soap films), hygroscopicity, and viscosity.

This type of classification scarcely originated with Briggs (cf. Warrington, 1900), but he provided it with an aura of completeness and of respectable physical content. For its time Briggs' paper had much merit, but his categories were arbitrary and artificial. His descriptive and qualitative approach represented a cul-de-sac from which there could be no path to a quantitative physical theory of soil water. The great misfortune was that what might have been useful as a temporary aid to thought was a barrier to progress once it became enshrined as doctrine.

How did this enshrinement come about? As we shall see, it is possible to suggest that this was, in part, an accident of persons and institutions. It should not be overlooked, however, that the Briggsian picture of soil water was attractive to the type of mind which is, by inclination and by training, more at ease with descriptive classifications painted with a broad brush than with efforts towards a quantitative and predictive science.

A natural development within the Briggsian schema was the invention of various empirical "soil constants" (= single values), the basic physical meaning of which was at the time obscure, but which were supposed to occupy certain points within Briggs' three classes. Briggs' own contributions to the array of soil constants were the moisture equivalent (Briggs and McLane, 1907) and the wilting coefficient (Briggs and Shantz, 1912).

Briggs became Physicist-in-Charge of the Biophysical Laboratory of the Bureau of Plant Industry of the U.S. Department of Agriculture in 1906 and he directed the biophysical work until 1920. He was thus well-placed to establish his concepts, and procedures based on them, in the many U.S.D.A. laboratories concerned with agronomy and soil problems.

E. Buckingham and the foundations of modern soil physics

The second personage in this story is Edgar Buckingham. Beyond the fact that they were contemporaries, Americans, and physicists, Buckingham and Briggs seem to have had nothing in common. Buckingham was an Easterner, whereas Briggs was born and wholly educated in the mid-West. Buckingham studied physics at Harvard and went on to post-graduate research at Harvard, Strasbourg, and Leipzig, where he received his doctorate.

In 1902, at the age of 35, seven years older than Briggs, Buckingham joined the Bureau of Soils as an assistant physicist under Briggs' direction and supervision. This was a strangely lowly situation for a man with his prestigious training, who had taught physics for ten years at Harvard, Bryn Mawr, and the University of Wisconsin, and who had just published an excellent monograph on thermodynamics. "His book", we are told, "carried the reader from the simplest facts of temperature measurement through the equilibrium of heterogeneous systems. It was a powerful contribution toward clarifying the subject." In the absence of information on the point, one can only guess at the reasons for the great gap between Buckingham's evident intellectual distinction and the subordinate place he occupied, not only in Briggs' laboratory, but throughout his subsequent career.

Within two years Buckingham had completed an outstanding pioneering study of diffusion and mass-flow of CO_2 and oxygen in soils. This classical work, combining extensive experimentation and quantitative physical theory, was transmitted for publication by Briggs and appeared as Buckingham (1904). Buckingham's opus magnum in soil physics was, however, yet to come. Before he quit the Bureau of Soils in 1905 for the post of Physicist with the U.S. Bureau of Standards (later National Bureau of Standards = N.B.S.) which he was to occupy until his retirement, Buckingham had developed the conceptual basis for modern physical studies of water in unsaturated soils.

Buckingham's grasp of thermodynamics enabled him to appreciate that, regardless of any qualitative schema of discrete classes of soil water, a continuity of energy states was involved and the whole moisture range was amenable to a unified treatment.

He also had the sagacity to avoid getting bogged down in the too-literal contemplation of models based on packings of spheres or on bundles of capillary tubes. With sure touch and an unerring sense of realism, he went straight to the prerequisites for a quantitative predictive theory applicable on the scale of observations and of practical concern: that is, a formulation based on quantities defined on the Darcy scale, the scale large compared with that of the individual pores. Buckingham took this for granted, and specific discussions of the matter of scale only came many years later (e.g. Raats, 1965; Philip, 1973).

The first ingredient in Buckingham's formulation followed from his recognition that the forces governing the equilibrium and movement of soil water are conservative and therefore amenable to treatment through their associated scalar potentials. He worked specifically with a total potential consisting of the gravitational potential and the potential of the forces arising from local interactions between soil and water. Buckingham called the latter the "capillary potential": it is now commonly designated the "moisture potential", though some prefer "matric potential". Buckingham was quite clear that he was working with the mechanical components of the partial Gibbs free energy. He wrote as follows: "The simple conception of a mechanical potential will suffice for present purposes, though it is not impossible that with more comprehensive data available we should have to use thermodynamic potential or the free energy." Buckingham carried out the first measurements of Ψ, the moisture potential, and he presented data on the dependence of Ψ on moisture content for a number of soils.

The second ingredient in the formulation followed from Buckingham's recognition that the appropriate generalization of Darcy's law should hold for water movement in unsaturated soil. Through a perceptive and accurate discussion of the mechanics of flow in unsaturated soil on the Navier-Stokes scale (cf. Philip, 1973), he introduced the concept of a "conductivity" (known now as the "hydraulic conductivity" or the "unsaturated permeability") which is dependent on the moisture content. Buckingham went on, through a consideration of steady one-dimensional flow systems, to evaluate the "moisture diffusivity" (as it was to be known fifty years later) and its dependence on moisture content. In effect, he developed (in different symbols) the relation

$$D = K\,\mathrm{d}\Psi/\mathrm{d}\theta$$

where D is the moisture diffusivity, K is the hydraulic conductivity, and θ is the volumetric moisture content. He showed that his experimental determinations of D were at least consistent with his observations of $\Psi(\theta)$ and his conjectures on $K(\theta)$.

Buckingham was well aware of the profound implications of this work. He wrote that "it is possible, though not probable, that we could give a complete mathematical treatment of the subject", once the functional dependence of Ψ and K on θ were known. The qualification "though not probable" was a piece of decent (but superfluous) caution.

There was a curious delay in the publication of Buckingham's account of this highly original and important study. It seems possible that Briggs was either unable or unwilling to see the deep and far-reaching significance of Buckingham's work. He certainly took no account of it in his own further work, nor in the concepts and procedures he nurtured in the U.S.D.A. The paper was not published until 1907, two years after Buckingham had moved on and a year after Briggs had gone to his new post in the Bureau of Plant Industry. The letter of transmittal, and the preface, omitted the usual acknowledgement of the author by name; and there was no hint of approval from the author's superior (Briggs).

Briggs and Buckingham: the epilogue

Buckingham never returned to the physics of soil water, on which he had made such a brilliant start: and no-one took over where he had left off. As we see in the next section, his work remained unknown or was not understood until its slow and gradual rediscovery over the succeeding decades. Physicists remember Buckingham for the Π-theorem of dimensional analysis (Buckingham, 1914); and some rheologists remember his analysis of plastic flow (Buckingham, 1921), which anticipated later (and less general) work by the famous Reiner (1926). The rheological study seems to have marked the end of Buckingham's productivity. In 1920 he found himself once more subordinate to Briggs, who had taken a senior position in N.B.S. Briggs became Director of N.B.S. in 1933 and held this eminent post until his mandatory retirement in 1945. Briggs went on as Emeritus Director until his death in 1963 at age 89, laden with honorary doctorates and many other awards of his grateful country. The shadowy and enigmatic Buckingham had, meanwhile, retired in 1937 and died in 1940.

PHYSICS OF SOIL WATER: REDISCOVERY AND DEVELOPMENT

In a paper which suggests no awareness of Buckingham's work, Gardner (1919) proposed a model of soil water movement corresponding to the special case with $\Psi \propto -\theta^{-1/3}$, $K \propto \theta$, and hence $D \propto \theta^{-1/3}$. These relations are unnecessarily specific and, in fact, bear no resemblance to those of actual soils and porous media. Gardner did take the further step, however, of applying the continuity requirement and he was thus the first to arrive specifically at a partial differential equation of the diffusion type which aimed at describing horizontal one-dimensional unsaturated flow. A related paper (Gardner, 1920) contains an oblique reference to Buckingham, but neither this nor a later paper (Gardner and Widtsoe, 1921) served to advance the physical theory.

Gardner's most useful contribution came subsequently when he put aside models of packed spheres and special functional forms, and turned his attention to the direct measurement of $\Psi(\theta)$. He and his associates (Gardner et al., 1922) perceived that Ψ could be measured manometrically so long as the instrument was connected to the soil through a water-filled vessel of sufficiently fine-pored ceramic. Israelson (1926) presented details of this pioneering work on the instrument to be known later as the tensiometer. This was a significant practical advance: Buckingham had been limited to obtaining $\Psi(\theta)$ from the moisture distribution in vertical soil columns in equilibrium with free water.

An interesting independent development in Austria was the experimental and theoretical work of Terzaghi (1923), who arrived at a diffusion equation to describe the related, but separate, process of horizontal one-dimensional flow in saturated swelling soils. He recognized that the quantities in his analysis corresponding to Ψ and K were functions of θ, but he linearized the problem. The first connections between Terzaghi's pioneering work and that of Buckingham were not to be established until 45 years later (cf. reviews by Philip, 1971; 1973).

One year after the First Congress of the I.S.S.S. there appeared the first paper in soil physics whose author understood the full implications and importance of Buckingham's contribution — namely Richards (1928). Richards was a young man working in Gardner's Department of Physics at the Utah Agricultural Experiment Station. Richards moved on to Cornell University, where his Ph.D. thesis work, reported in Richards (1931), represented the first unequivocal progress beyond Buckingham. Richards applied the continuity requirement to Buckingham's extension of Darcy's law and so obtained a general partial differential equation describing water movement in unsaturated non-swelling soils. He recognized that, when Ψ and K are single-valued functions of θ, this equation may be expressed with either θ or Ψ as the single dependent variable. Richards chose to write the equation with Ψ as the dependent variable. This choice (despite certain physical virtues) was a little unfortunate, because the equation in that guise is marginally more complicated and its diffusion form (already emergent in Buckingham's work) is slightly obscured.

Beyond its theoretical precision and lucidity, Richards' paper included the first experimental measurement of $K(\theta)$ and the first observations of capillary hysteresis (i.e. hysteresis in $\Psi(\theta)$) in a real soil, as opposed to the studies by Haines (1927, 1930a,b) on packed spheres and a coarse sand. To Haines, however, goes the credit for first recognizing, primarily through his investigations on "ideal soils", the fact of capillary hysteresis. This was the first element of the modern theory of the physics of soil water which had been neither explicit nor implicit in Buckingham (1907).

Richards' 1931 paper set the definitive course for subsequent progress in basic physical studies of water equilibrium and movement in non-swelling soils; but twenty years were to pass before Richards' contribution was understood — in the sense that it was constructively built on.

Over the next fifteen years, the principal progress was that more soil scientists became accustomed to the notion of moisture potential. Its first appearance in transactions of the I.S.S.S. was in the influential paper of Schofield (1935a), delivered to a plenary session of the Third Congress. The development of new and improved methods of measuring Ψ was an important factor in helping soil scientists come to grips with this concept; and Richards led the way. His 1928 paper had laid down such a programme of development: Richards (1942) gave the definitive report on the tensiometer; and Richards (1941) described the pressure-membrane apparatus.

Childs and George (1948) recognized the diffusion form of the steady one-dimensional flow equation and presented data on $D(\theta)$ for a sand. Forty-one years had passed since Buckingham had made the only previous estimates of $D(\theta)$! Finally, Klute (1952) rewrote the Richards (1931) formulation for three-dimensional unsaturated flow in the diffusion form with θ as dependent variable. This equation is conveniently written in appropriate units as:

$$\frac{\partial \theta}{\partial t} = \nabla \cdot (D \nabla \theta) - \frac{\mathrm{d}K}{\mathrm{d}\theta} \cdot \frac{\partial \theta}{\partial z} \qquad (1)$$

Here t is time and z is the vertical ordinate, positive downward. The coefficients D and $\mathrm{d}K/\mathrm{d}\theta$ are both markedly dependent on θ. D may vary typically through three or more decades (and $\mathrm{d}K/\mathrm{d}\theta$ through several more) in the moisture range of interest. These nonlinearities are far too strong to be ignored. It follows that application of the quantitative physical theory of water movement in unsaturated soils has depended quite centrally on the availability of solutions of the nonlinear Fokker-Planck equation (1) and on the nonlinear diffusion equation:

$$\frac{\partial \theta}{\partial t} = \nabla \cdot (D \nabla \theta) \qquad (2)$$

which applies to systems (such as horizontal ones) where gravity may be neglected.

Klute (1952) gave the lead by presenting a solution of the one-dimensional form of eq.2 for the case of horizontal absorption. There has been much subsequent activity and progress in solving eqs.1 and 2 and relating them to soil-water phenomena. Philip (1969) gave a lengthy review and Philip (1974) summarizes subsequent developments.

I conclude this section by noting briefly several extensions of the foregoing basic theoretical structure. Firstly, the approach was extended (Philip, 1954, 1955, 1957) to apply to moisture transfer in the vapour and adsorbed phases with the same mathematical formulation retained. These extensions depended, essentially, on the chain rule for differentiation and use of the thermodynamic relation:

$$h = \exp g\Psi/RT \tag{3}$$

where h is the relative humidity, g is the acceleration due to gravity, R is the gas constant for water vapour, and T is the absolute temperature. Eq.3, of which Buckingham was clearly aware, is the essential link between the liquid and vapour phases. The wholly unsatisfactory nature of studies of the "hygroscopic coefficient" and of other early investigations involving the vapour phase, as recognized for example by Keen (1928), seems to have stemmed from failure to appreciate the significance of eq.3.

The foregoing analysis has also been extended to nonisothermal systems by Philip and De Vries (1957), De Vries (1958), and De Vries and Philip (1959). Recently Jury (1973) has provided a useful and incisive commentary on this work by reinterpreting it in terms of the formulation of the thermodynamics of irreversible processes.

In addition, some progress has been made with the study of capillary hysteresis and the problem of its mathematical representation. Miller and Miller (1956) gave a penetrating but qualitative discussion. Poulovassilis (1962) gave the first published account of capillary hysteresis in terms of the independent domain model. Childs (1964) showed how the flow equation may be applied to a hysteretic system of this type. Philip (1964) and Mualem (1973) have proposed similarity hypotheses which greatly simplify characterizing the hysteresis. There remains, however, some question as to the adequacy of the independent domain model (Topp and Miller, 1966; Poulovassilis and Childs, 1971; Topp, 1971).

Extensions to take account of the effects due to soil air (Youngs and Peck, 1964; Peck, 1965a,b; Philip, 1969; Morel-Seytoux, 1973) and of hydrodynamic stability (Hill and Parlange, 1972; Philip, 1972; Raats, 1973) are also under way.

SWELLING SOILS—SOIL MECHANICS

The developments of the preceding section provide a fruitful framework for the study of the hydrology of nonswelling soils, but they do not take account of the effects of volume change, which can be important in soils of high colloid content. Philip (1971, 1973) has reviewed some modest steps towards the required generalization, and towards the integration of two generally divergent viewpoints: on the one hand, that of soil physics (preoccupied with soil water and its flow) and, on the other, that of soil mechanics (concerned with soil-volume change and the associated stresses). Keen (1928), in his report on Commission I at the First Congress, remarks on the related problem of the interactions between soil colloids and the soil solution. Schofield (1935b) pioneered the approach through double-layer theory and later contributors included Childs (1954) and Bolt (1956). It is a continuing challenge to relate behaviour of the colloid extract to the macroscopic behaviour of the soil in the field (Philip, 1972).

In its earliest days Commission I was for "Soil Mechanics and Physics". By the time of the Second Congress in Leningrad in 1930, however, the title of Commission I had been changed to "Soil Physics". The change seems to have been effected without comment: I can find neither reference to it nor explanation of it in the publications of the Society. It may have been decided, in view of the primary interests of the I.S.S.S. in pedology and agricultural aspects of soil science, that the Commission was not well-placed to give sufficient attention to Soil Mechanics, with its close connections with civil engineering. Be that as it may, Soil Mechanics went its own way. An International Conference on Soil Mechanics and Foundation Engineering took place in 1936 and led to the formation of the International Society of Soil Mechanics and Foundation Engineering. In my opinion the separation of Soil Physics and Soil Mechanics, whatever the historical and practical reasons for it, impedes progress in both fields.

CONCLUDING DISCUSSION

Let me reiterate that I have not attempted the impossible (and, I think, undesirable) task of providing in this paper an exhaustive catalogue of a half-century of activity in soil physics. I have limited myself to those sectors about which I know most. I have, if you like, studied only a sample, and doubtless a biassed sample. I believe, however, that the sample is large enough, and important enough, for me to be able to offer some general observations on the changes in the character and content of soil physics over these last fifty years. The text by Nerpin and Chudnovskii (1967), which reviews progress in soil physics in the Soviet Union and covers many topics not mentioned here, indicates that these changes in the complexion of soil physics have not been confined to the West.

In these fifty years soil physics has gained enormously in its self-confidence, its intellectual power, and its relevance to the practical problems of the real world. These three improvements are interrelated and it is somewhat artificial to discuss them separately. We can, however, identify certain interwoven themes. Firstly, there has been more general recognition that soil physics has a much wider and more vital contribution to make than to serve merely as a handmaiden of pedology. The dynamic physical processes of the soil are central to plant growth, to the life of soil flora and fauna, to various important problems of dryland and irrigated agronomy, and to the fundamental problems of ecology, hydrology, and environmental quality. For all these applications, an adequate quantitative predictive science is needed. In the effort to supply this need, the soil physicist has become more prepared to avail himself of the general intellectual resources of physical science: of fields, for example, such as thermodynamics and nonlinear mechanics. Furthermore, once he is willing to look at problems in terms of their basic physical content, the soil physicist discovers that his studies have much in common with those of physicists concerned with other porous media. Increasingly, concepts and techniques in soil physics interact with those in more than a score of fields of natural science and technology (Philip, 1970).

It must be conceded, however, that this new maturity has not resolved all our difficulties. Our greatest scientific success has been in the study of local processes taking place on and under a single small area of the ground surface. The economy of analytical scientific methods is soon lost in the large, complicated, and often ill-defined, problems which arise on the scale of the river basin or the ecological unit: and it is clear that soil physicists must address themselves more to the question as to how we may best use our understanding of small-scale processes in attacking these larger problems (cf. Philip, 1972).

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