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The main stimulus and support of ground-water hydrology has always been the human need for water supplies, and the glory of ground-water work has been that human betterment, through the development of more abundant, convenient, and wholesome water supplies, has followed closely in the wake of our work. However, this utilitarian urge has become so extreme that at present in this country practically all funds available for ground-water work must be used in applying our knowledge to specific ground-water surveys or water-supply problems, with virtually no opportunity for research work except as it is carried on inadequately and almost surreptitiously in connection with these utilitarian projects. We are constantly compelled to follow the wasteful course of applying the little that we now know instead of being able to devote a reasonable part of our efforts to the fundamental task of developing the basic principles of the science so that in the future we will have something more worth while to apply. What is primarily needed at the present is not more money for ground-water work but a more rational use of the money that is spent.

With these words, O. E. Meinzer (1934, p. 30–31) summarized the status of ground-water research three decades ago. What has happened since then? How do we fare today, and what is the promise of tomorrow? These subjects I will discuss. But first, let us review the early history of ground-water research in the United States, extending it back to the end of Meinzer's day shortly after World War II. For background I am indebted to Meinzer's own writings and to those of my colleagues in the U.S. Geological Survey. For detailed summaries of the development of ground-water hydrology, including research, you are referred to papers by Meinzer (1934, 1942) and by Ferris and Sayre (1955). The latter paper includes an extensive bibliography of the principal developments before 1955.

There is very little record of scientific ground-water work in the United States before 1877, when T. C. Chamberlin's first report on artesian conditions was published. This was followed by a more comprehensive report in 1885. These two papers may be said to mark the start of ground-water research in the United States.

Beginning about 1890, the interest in ground water as a resource quickened. Consequently, and with stimulus from John Wesley Powell, the second Director of the Geological Survey, ground-water studies were begun in this agency on a small scale. Early studies, made by geologists of the Geologic Branch as a part of their geologic fieldwork, were largely descriptive and were concerned principally with where water was to be found. Included among the first studies were investigations in the Arkansas Valley by G. K. Gilbert, in the central Great Plains by N. H. Darton, and in the High Plains by W. D. Johnson. In the tradition of the geologists of that day, these early investigators were keen observers and masters at drawing a maximum of sound conclusions by inference from sparse data. They established geology as the cornerstone of ground-water hydrology, and the Geological Survey as the principal Federal agency concerned with studying the ground-water resources.

Meanwhile, work by other investigators, notably Allen Hazen, Franklin King, and Charles Slichter, was directing interest in the United States for the first time in any substantial degree to the quantitative aspects of ground-water hydrology. A milestone was passed in the early 1900's when Slichter devised methods of measuring rates of ground-water movement. His studies of underflow in the South Platte River, Arkansas River, and Rio Grande Valleys marked the beginning of quantitative ground-water studies.

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From these early beginnings ground-water work in the United States progressed slowly until the mid-twenties. Nevertheless, events during this period were to lay the foundation for much of the ground-water research to come later. Most of the effort, by a handful of investigators in the Geological Survey, was concerned with geology as it controlled the occurrence and movement of ground water. From this work stemmed Meinzer's (1923) classic Water-Supply Paper 489, which remains today a basic ground-water reference. But more important than published reports were two men and their ideas. The men were W. C. Mendenhall and Oscar Meinzer.

Mendenhall, later Director of the U.S. Geological Survey, was to kindle and direct Meinzer's early interest in ground water; and Meinzer, in leading ground-water activities in the Geological Survey, was to set a new pattern from which ground-water hydrology in the United States would evolve as a distinct phase of science. Mendenhall's contribution can be summed up best in terms of Meinzer's recollections:

> I wish to mention two lessons that I learned from Mendenhall. The first was to take ground-water work seriously. There was a tendency at that time for geologists assigned to the ground-water investigation of an area to devote themselves to the geology of an area for its own sake and to give only scant attention to the ground water. In some cases the geologic data obtained in a season of field work would be supplemented with information in regard to the wells obtained by sending circular letters to the postmasters in the area covered. Mendenhall was bitterly critical of this frivolous attitude toward ground water, and I was wholly in accord with him.
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> Another lesson that I learned from Mendenhall concerned the importance of the quantitative problems in ground-water work. He worked in southern California, where the major problems even at that time related to the quantities of ground water permanently available, and his thinking was always in quantitative terms. Thus it happened that all through the lean years we were concerned with quantitative methods. Although we did not have funds or personnel to make adequate tests of the methods we devised, we were nevertheless laying the foundation for important results in later years.

Meinzer learned his lessons well. In subsequent years he emphasized the study of ground water as a worthwhile goal in itself and broadened ground-water investigations to include all facets of the hydrologic cycle that impinged thereon. To help provide the quantitative input he drew in the engineer and physicist, concerned principally with fluid mechanics, to team with the geologist, concerned principally with describing the earth medium. With V. C. Fishel, a physicist (Meinzer and Fishel, 1934), he investigated experimentally in the laboratory and proved that Darcy's law is valid for the extremely low gradients common in ground-water flow. He translated the doctoral dissertation of G. Thiem, which he found by chance in the Survey Library. This gave him the steady-state mathematical equation for determining permeability and transmissibility by a field pumping test. Slichter (1899) had published an identical equation, except for form, but apparently its potentialities were not recognized. Meinzer then arranged for Wenzel (1942) to make an extensive field test of the Thiem method at Grand Island, Nebr., and from this test a better understanding was obtained of its potential application.

The broadened interest and outlook toward ground water is reflected by the wide variety of subject matter that received attention during the period extending from the midtwenties to World War II. This included the water budget, the relation of salt water to fresh water, the relation of ground water to surface water, recharge, ground-water discharge by phreatophytes, the compressibility and elasticity of artesian aquifers (Meinzer, 1928, in part on the basis of ideas suggested by D. G. Thompson), and the chemistry of ground water. In particular, attention was given to the theory of ground-water motion. The storage and transmission characteristics of porous media were studied in both laboratory and field, and methods for determining these characteristics quantitatively were derived and tested. The conceptual explanation of the functional characteristics of aquifer systems by Theis (1938, 1940) was an outstanding advance. The development of the nonsteady-state equation by Theis (1935) provided the means for quantifying those characteristics and showed clearly that time can be an important boundary in hydrologic analysis. This work, which since has been extended by colleagues of Theis to cover variations from ideal conditions, represented a major breakthrough in the search for quantitative principles that might be applied to investigational procedures. Notable also was the development at about the same time of the theory of fluid motion through porous media by Muskat (1937), and a comprehensive treatise on the theory of ground-water motion by Hubbert (1940). Together, these gave the ground-water hydrologist new analytical tools and concepts for solving ground-water problems.

The end of World War II approximately marks the end of the formative period of ground-water hydrology. Ferris and Sayre (1955, p. 716–717) aptly describe the early part of this period as one of rugged individualism. "Geologist, engineer, and physicist, each motivated by some immediate problem, attacked only that phase of ground-water hydrology which related to his specific problem, and only those aspects that fell within his own field of competence." They note also, speaking of the theory of ground-water motion, that in the early days there was little interchange of information among workers in Europe and the United States; consequently, many parallel developments were formulated contemporaneously. In the United States the same was true with respect to the interchange of information among workers in ground water and workers in related fields such as soil physics, soil mechanics, drainage engineering, and petroleum engineering. The close of this early period was marked by a great improvement both in applying the coordinated effort of workers from different disciplines and in the interchange of information.

Throughout this time there was no financial support for a ground-water research program in an organized sense. Research had to be carried out in the U.S. Geological Survey, as Meinzer noted, almost surreptitiously in connection with utilitarian projects; it was made possible because of the encouragement of interested State geologists and engineers who helped support the work and contributed some of the ideas. There was no formal training for ground-water hydrologists—at best here and there a geology department would offer a course in ground-water geology or a civil engineering department would offer a course labeled "hydrology." As a consequence, research was carried out by men whose training initially had oriented them toward other (though related) careers. There was little research or research interest in ground water by universities and colleges; thus the potential in the academic manpower pool was untapped.

From these circumstances stemmed Meinzer's complaint that "We are constantly compelled to follow the wasteful course of applying the little that we now know instead of being able to devote a reasonable part of our efforts to the fundamental task of developing the basic principles of the science so that in the future we will have something more to apply."

Nevertheless, in retrospect, despite the disadvantages attending the circumstances of that day, the sum of these circumstances tended toward molding much that is good. It tended toward creating a research attitude among the people who were engaged in ground-water studies, even though these studies were utilitarian. The worker of that day had no handbook, nor was there an elite corps to whom he could turn for principles and for guidance in applying them. If he were to solve his problems, he had to engage in research personally. This sort of challenge led some high-caliber people into the field who might not have been attracted otherwise, and it yielded results that were characterized by relevance to water problems. For example, Theis was not conducting research into the theory of ground-water motion when he developed the nonequilibrium formula; rather, the original transient case was developed from a geological standpoint by Theis to enable him to solve a practical field problem. In his words (Theis, 1963, p. 28):

> In the days when this equation was being worked out, which was before publication of Muskat's book or King Hubbert's paper on ground-water flow, I was faced with the proposition of how to think about pumping from the High Plains, particularly from the Llano Estacado, or Staked Plain of Texas. Here, very evidently we could not think of a small withdrawal of water from a water body, which had several times the capacity of Lake Mead, in equilibrium terms, which had been the practice up to that time. So I was driven to find a new way to think about the problem and, of course, as any of you who have read the paper will know, I ended up with a mathematician friend who helped me in my quest to find the new way.

We turn now to developments during the last two decades. The first decade, from World War II to the middle fifties, was one of transition. The substantial increase in water use accompanying peacetime growth after World War II gave added impetus to the interest in water, and the resulting clamor for more and better basic data brought about a rapid expansion in programs of water-data collection and interpretation. Research, though carried on as always in conjunction with these programs, became progressively smaller in proportion to the total effort. The need for an increased emphasis on ground-water research had been recognized, however, and progress in this direction was to mark the last decade.

In the U.S. Geological Survey, a major step toward making a reality of Meinzer's dream for a program devoted specifically to ground-water research was taken in 1956 when R. R. Bennett drafted a statement identifying five major areas of research vital to an understanding of hydrologic systems. With stimulus from Luna Leopold and Nelson Sayre, a research section was established under Bennett's leadership, and with minor modifications Bennett's original statement of areas for research became the foundation of much of the Survey's ground-water research program. For a recent description of this program you are referred to Geological Survey Circular 492 by C. L. McGuinness (1964).

Perhaps the most significant event promising an impact on ground-water research was the passage in 1964 of the Water Resources Research Act (Public Law 88–379) and the establishment of the Office of Water Resources Research in response thereto. This office already supports 69 projects identified as ground-water research. In addition, it publishes annually a catalog of water-resources research which should promote the exchange of research information (Office of Water Resources Research, 1965a, b).

During the past two decades there have been no spectacular breakthroughs in ground-water research, but rather numerous advances along many fronts. Shortly after World War II Meinzer identified future needs for research in eight categories: ground-water hydraulics, the zone of aeration, ground water-surface water interrelationships, geophysical methods, the mode of ground-water occurrence in various terranes, geochemistry, salt-water encroachment, and ground-water contamination. From an earlier review (Hackett, 1964) of the advances made in these categories, I repeat the following summary:

In ground-water hydraulics, developments have taken four principal directions: refinements in aquifer-test methods such as those involving the use of image wells (Ferris and others, 1962); a closer look at the true nature of the storage coefficient, especially in strata that deform plastically rather than elastically; permeability distribution to enable economically possible yet reasonably accurate extrapolation of limited aquifer-test data in setting up electrical analog models of aquifer systems; and studies of diffusion and dispersion in relation to transport of contaminants (Ogata and Banks, 1961; Ogata, 1961 and 1963; Skibitzke and Robinson, 1963), such as radioactive wastes, and to encroachment of salt water in coastal aquifers (Cooper and others, 1964).

The zone of aeration is the "no man's land" of hydrology. It has received some attention from the ground-water hydrologist, who is interested in how water travels through it both downward to and upward from the water table; from the meteorologist and the surface-water engineer, who are interested in its effect on interception of precipitation and modification of runoff; from the soil scientist, who is interested in it mainly as a reservoir of moisture for plant growth; and from the sanitary engineer, who is interested in it as a transporter of and a temporary or permanent trap for various contaminants (Stallman, 1964). Much of our present knowledge is qualitative or empirical, however; we are only beginning to investigate the fundamental physics and chemistry of this movement and interaction of water and its dissolved and suspended constituents in the zone of aeration. The ground-water hydrologist will have the principal role here because of his understanding of fluid flow in porous media, and much of his effort in the immediate future will be aimed at this field.

Ground-water–surface-water relationships are receiving more attention as hydrologists increasingly adopt the "systems analysis" point of view in attempting to visualize the effects, throughout whole hydrologic systems, of various water development and control projects. Achievements since Meinzer's time include some fundamental work on induced infiltration from streams to wells (Rorabaugh, 1953), on filling and emptying of bank storage as a result of natural fluctuations in stream storage (Cooper and Rorabaugh, 1963), on analysis of water-level fluctuations in wells resulting from tidal and other changes in stream storage in terms of aquifer characteristics (Ferris, 1963), and on interchange of fresh and saline water between coastal aquifer and the ocean.

Geophysical methods are being used increasingly in ground-water studies. Prospecting by the surface electrical-resistivity and seismic methods, which were used on a considerable scale in Meinzer's day, have been supplemented on a small scale by magnetic and gravity surveys. Borehole geophysics, however, is the principal current tool. The chief advances have been in the application of neutron and gamma-ray logging, in the hydraulics of multiple-aquifer wells, and in textural and geochemical interpretations from resistivity and spontaneous-potential data. Borehole geophysics is expected to be a principal tool in getting more reliable information at practical cost from wells and test holes for use in the construction of analog models of hydrologic systems.

Certain geologic terranes have characteristic hydrologic features which differ markedly from those of other terranes and which can be profitable studied in terms of those characteristic features. Among these are carbonate-rock ("limestone"), basalt, crystalline-rock, and permafrost terranes. Certainly these and other terranes received attention in Meinzer's day and before, but only in recent years has a more systematic effort been made to identify the lithologic features responsible for their distinctive hydrologic behavior and to measure their effect quantitatively.

Progress has been made in measuring the effect of certain structural features in damming or channeling underground flow in limestone and basalt (Newcomb, 1961); in relating the occurrence of water in crystalline rocks to lithologic types, major structures, joint patterns, and topography; and in prospecting for liquid water in areas of permanently frozen ground. Work is also underway on certain features of sandstone-shale terranes, such as texture in relation to directional permeability as a guide to patterns of deposition (which would help in extrapolating data to fill in permeability-distribution maps), and "membrane" properties of shales in relation to cross-bed movement of water in structural basins.

Geochemistry is becoming increasingly important as it is realized that some phenomena of water movement can be explained only on a chemical basis—for example, the "membrane" properties of shales mentioned above. Many current problems are basically chemical, such as high iron content in relation to organic matter, control of acid mine water, disposal of radioactive wastes, origin of water precipitated in inland areas, and maintenance of proper salt balance in irrigated areas.

Salt-water encroachment is a potential threat wherever ground water is used along the coast or along a tidal stream or estuary. It is a threat inland too, wherever heavy pumping may induce lateral movement or upconing of brines. An example of recent research achievements in this field is an explanation of the difference between the classical Ghyben-Herzberg balance between fresh and salt water and actual situations where the interface was displaced seaward as a result of erosion of the salt-water front—the energy coming from the twice-daily tide-induced fluctuation of the interface. A related achievement is the development of mathematical solutions for the positions of the salt-water interface under a variety of shore characteristics (Cooper and others, 1964). Research also is underway to explain certain anomalous distributions of fresh and salt water as hangovers from periods of different sea level hundreds or thousands of years ago.

Ground-water contamination is an increasingly important field of study as development becomes heavier and wastes are disposed of onto the land surface or into surface-water bodies from which they can enter the zone of saturation. That contamination has been mentioned in connection with several of the preceding subjects simply goes to show the broad ramifications and potential seriousness of the problem. Of the many studies underway in this field, a good example is the study of the effects of synthetic detergents on the soil, such as reduction of soil permeability by dispersing soil colloids, and of the rate of detergent decomposition in various environments. Other important studies include those intended to develop criteria for safe disposal of radioactive wastes.

In addition to the foregoing, new techniques and methodology have been given attention by researchers For example, much attention has been given to the use of analog and digital computer methods for analyzing ground-water systems. As a consequence, the use of the electrical analog model in ground-water investigations already is widespread. Remote-sensing techniques are another example of a new development that is being studied for possible application to ground-water hydrology.

We also see during the past two decades among those engaged in ground-water research a maturing of attitudes along the following lines:

1. An increased recognition of the significance of the interrelationship between ground water and water in other phases of the hydrologic cycle—especially surface water and water in the unsaturated zone.
2. An increased recognition that ground-water research extends beyond the purely hydrological considerations and encompasses economic, social, legal, and political factors that relate to water utilization and reservoir management.
3. An increased awareness that some elements of ground-water research require an interdisciplinary approach by specialists in the natural, agricultural, engineering, and social sciences.
4. An increased awareness of interests shared in common with specialists conducting research in other fields, such as petroleum geology and engineering, agriculture, drainage engineering, and soil physics.

This summary brings us to the present day. What of the future? In terms of work to be done it is clear that more effort must be given to all the research areas described above. Increased attention must be given to developing methods for analyzing regional flow patterns and superimposed chemical systems. But to make fruitful the application of existing or new methods, research must be devoted also to improving ways of numerically defining the characteristics of these systems. These are high-priority items to which the U.S. Geological Survey already is applying much effort.

You will note that nearly all the research reviewed above is in the realm of hydrology. Ground-water research should be concerned also with the realm of water planning and management—including social, economic, and legal problems connected with the use and development of the ground-water resources. These, too, are high-priority areas, and they have been relatively neglected.

In terms of support, the outlook for ground-water research is bright. In its report on the Federal water-resources research program for fiscal year 1966, the Federal Council for Science and Technology estimates the annual research investiment to be less than 1 percent of the annual construction cost of the total water-related public-works program (Federal, State, local, and private). In turn, effort identified specifically as ground-water research amounts to about 6 percent of the total Federal water-research program, or only a fraction of 1 percent of the national water-related public-works program. From these figures one might, and perhaps justifiably, draw the conclusion that funds for ground-water research are woefully inadequate. I am inclined to think otherwise. The increased interest and activity by State agencies and universities, the passage of the Water Resources Planning Act and the establishment of the Office of Water Resources Research, and the continued interest and concern of the Federal Council for Science and Technology suggest that funds for research, if not adequate now, will be soon. However, funds are likely to be adequate only if utilized wisely in the pursuit of Meinzer's basic principles and if the temptation of diverting them to water-resources appraisals and others of Meinzer's utilitarian studies is avoided.

In terms of trained manpower for ground-water research we are lagging, but even here there is cause for optimism. With stimulus from the Water Resources Research Act, our universities should provide within a few years a source of manpower oriented directly toward water-resources work and trained with this work in mind.

The future of ground-water research is bright. We are compelled no longer to follow the wasteful course of applying the little that we now know instead of being able to devote a reasonable part of our efforts to the fundamental task of developing the basic principles of the science so that in the future we will have something more worthwhile to apply.

But, more than ever, we need to exchange information and especially to coordinate our research activities. In the words of Thomas and Leopold (1964, p. 1006–1007):

The kaleidoscopic variety of environments in which ground water occurs, and of modifications made by man, provide magnificent opportunities for any research that results in additional basic data and interpretive achievements. Thus random research, widely dispersed, can add to our total fund of knowledge; but like reproduction by division, its products may be new individuals, in new places, but remarkably similar to what we already have. The interrelation of "unknowns" points to a need for central direction and coordination of all these research efforts. Like water itself, our major water problems tend to resist partitioning, and without coordination of effort they may well remain unsolved.

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