

M. King Hubbert and "The Theory of Ground-Water Motion"

by R. Allan Freeze

Introduction

In 1940, the November-December issue of the *Journal of Geology* was published in two parts, with Part I being a kind of special issue that contained just one paper. This paper was *The Theory of Ground-Water Motion* and it was written by the geophysicist M. King Hubbert. The paper was 159 pages long, with 47 figures, and 307 numbered mathematical equations (Hubbert 1940). Hubbert was little known in the groundwater community at the time. In fact he had never written a paper on groundwater before, and he wrote only one further meaningful paper on the subject and that was some 16 years later. At the time of publication of *The Theory of Ground-Water Motion*, Hubbert (Figure 1) was 37 years old and between jobs, having just parted ways with Columbia University where he had been an instructor for 9 years without ever having been offered a tenure-track position.

The Theory of Ground-Water Motion created considerable controversy at the time, but it has since come to be recognized as one of the seminal papers in the history of hydrogeology. The paper has come to be admired for its scientific rigor, its originality, and the clarity of its exposition. It is quoted at length in every groundwater text that has since been written. It is the goal of this biographical essay to examine how such a remarkable burst of intellectual energy came to pass, to review the influences on Hubbert's life that may have helped bring it about, and to assess the legacy of his paper in the historical development of the science of hydrogeology.

After his foray into groundwater science, Hubbert went on to make paradigm-shifting contributions in several other subfields of geology, notably his explanation of the hydrodynamic entrapment of petroleum (Hubbert 1953), his study of the mechanisms of hydraulic fracturing (Hubbert and Willis 1957), and his solution to the longstanding mysteries around the mechanisms of overthrust faulting (Hubbert and Rubey 1959). Hubbert's

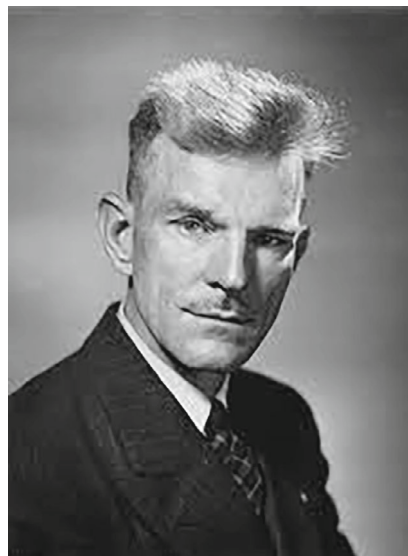


Figure 1. M. King Hubbert, circa 1940.

understanding of subsurface fluids certainly informed these later works but they are not topics that are usually considered to be within the bounds of groundwater science. Furthermore, in the public eye, he is undoubtedly best known for his predictions of the limited capacity of the world's oil and gas reserves and his controversial concept of "peak oil." All these nonhydrogeological contributions are hugely important to the history of the earth sciences, but this article does not discuss any of them at length. It is in no sense a full biography of Hubbert. Rather, it is a celebration of one influential paper, the one for which he is most revered in the hydrogeological community.

Table 1 is an attempt to summarize Hubbert's life on a single page. It is a compressed biography from his birth in San Saba, Texas, in 1903, to his death in Bethesda, Maryland, in 1989. It allows the reader to place *The Theory of Ground-Water Motion* onto the timeline of his personal life, his career path, his other seminal

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Table 1
A Timeline of Hubbert's Life

Decade	Personal	Career	Seminal Publications	World at Large
1900 1910	1903 Born, San Saba TX			1905 Einstein Relativity 1912 Titanic 1914-1918 World War I 1925 Scopes Trial
1920	1924-1931 University of Chicago, B.Sc. 1926, M.Sc. 1928	1926-1928 Field geophysicist, Amerada Petroleum Co, OK, TX, NM 1928-1929 Instructor, University of Chicago		
1930	1931 Married Yelena Pavlinova 1930s Founding member, Technocracy movement 1936 Divorced Yelena 1937 Awarded Ph.D. by University of Chicago 1938 Partnered with Miriam Berry	1931-1939 Instructor, Columbia University	1930 Isostasy: A critical review 1937 Theory of scale models as applied to the study of geologic structures	1929-1939 Great Depression 1933-1937 FDR New Deal 1939-1945 World War II
1940		1941-1943 Senior analyst, Board of Economic Warfare, Washington, DC	1940 The theory of groundwater motion	
1950	1950 Formally married Miriam 1954 GSA Arthur L. Day Medal	1943-1964 Shell Oil Laboratories, Houston TX	1953 Entrapment of petroleum under hydrodynamic conditions 1956 Darcy's law and the field equations of the flow of underground fluids 1957 Mechanics of hydraulic fracturing (with David Willis) 1959 Role of fluid pressure in mechanics of overthrust faulting (with William Rubey)	1950-1953 Korean War 1952 H-bomb 1953 DNA 1955 Start of Vietnam War 1957 Sputnik
1960		1964-1976 Research geophysicist, USGS		1960s Civil Rights Movement 1963 JFK Assassination 1975 End of Vietnam War
1970	1971 SPE Anthony F. Lucas Medal 1972 Honorary degree, Syracuse University 1973 GSA Penrose Medal 1978 Geol. Soc. London William Smith Medal	1976-1989 Consultant, USGS		
1980	1980 Honorary degree, Indiana State University 1981 Columbia University Vetlesen Prize 1981 Franklin Institute Elliot Cresson Medal 1989 Died, Bethesda MD			1983 Internet 1989 Fall of Berlin Wall

publications, his many awards, and the events in the world at large.

The photograph of Hubbert is from around the time of his groundbreaking publication on groundwater flow (Wilson 2007).

Sources

M. King Hubbert is perhaps the most celebrated earth scientist of his generation and much has been written about him. These writings are a rich source of material for this article. They include memorial articles published after his death (Doan 1989; Doel 2007); citations for his major awards (Donnay 1955; Prucha 1974; Raleigh 1982); biographical articles prepared during his lifetime (Clark 1983; Wilson 2007; Wilson 2008); and a full-length biography entitled *The Oracle of Oil: A Maverick Geologist's Quest for a Sustainable Future* (Inman 2016). Inman's book and most of the other biographical writings place their emphasis on Hubbert's role in the peak-oil controversies, and while his groundwater work is always covered to some degree, it has not been appraised before in the full hydrogeological context that is attempted here.

While these written contributions are valuable, the real mother lode of Hubbert lore comes from a series of face-to-face interviews carried out over nine multihour sessions in Bethesda, Maryland, by the science historian Ronald E. Doel in 1989, just before Hubbert's sudden death later that year. The interviews were put together under the auspices of the American Institute of Physics through the Niels Bohr Library, Center for the History of Physics. A full transcript of the Doel-Hubbert conversations is available online on the AIP website (Doel 1989). A perusal of these transcripts leaves one with a feeling of knowing Hubbert at a personal level. One can sense his confidence, his drive, and the crystal clarity of his thinking, but also his cantankerousness, his anti-authoritarian streak, and his dismissiveness of the works of others which do not meet his rigorous standards.

There are also two caches of Hubbert's papers, one at the University of Wyoming, and the other at the Illinois State Geological Survey. It is likely that there is much information of value in these collections but I have not examined them for this article.

Influences on Hubbert's Life

Family

Hubbert was born on October 5, 1903, in San Saba, Texas, in the hill country northwest of Austin. Hubbert's mother, Cora Virginia Hubbert (nee Lee) was a schoolteacher, and all the evidence suggests that it was she, rather than his father, who encouraged Hubbert's interest in getting a good education. She recognized his precocity and arranged for him to study for his high school equivalency at a local private junior college, and then to begin his college training away from home at Weatherford Junior College near Fort Worth. Other than this early

maternal influence, there is little evidence that family played much of a role in Hubbert's later life. He had five siblings, three older sisters and two younger brothers, but they receive scant mention in the Doel interviews. In fact, the most noteworthy is Hubbert's confession that he once turned down an appointment at UCLA because there were too many family members in the area. "They do an awful lot of family socializing there," he noted, "and I don't like to do that kind of thing; I mean once a year or something of the sort, but not every few weeks" (Doel 1989, Session VIII).

Hubbert's mother, Cora, is also probably responsible for his unusual given names. The M stands for "Marion," a name that he reduced to an initial in all his professional writings. Both the "Marion" and the "King" are named after men who played a role in Cora's own educational pilgrimage from farmhouse to schoolteacher.

Personality

When it comes to descriptions of Hubbert by his memorialists, the adjectives are all of a piece. He was acerbic, opinionated, sharp-tongued, stubborn, irascible, abrasive and cantankerous. He thrived on intellectual battles and was not averse to making them personal. As David Doan puts it in his memorial: "King appreciated argument as the knife welcomes the whetstone" (Doan 1989). He reveled in proving others wrong, especially if those others were recognized authorities, and he recognized this trait in himself. "I seem to have an aptitude," he said, "for arriving at conclusions which are contrary to the authoritative opinions of the moment" (Inman 2016). Barry Raleigh in his Vetlesen Prize citation puts a more positive spin on Hubbert's maverick streak: "Being outspokenly correct when the conventional wisdom would have it otherwise may not win popularity contests, but the vitality and intellectual integrity of men such as King Hubbert are rare and precious qualities" (Raleigh 1982).

Perhaps because of this tendency to abrasive behavior, Hubbert often worked alone. He was self-directed, hard-working, and confident in his own abilities. Francis Pettijohn describes Hubbert's arrival at the University of Chicago for graduate work in 1924. He was "an iconoclast," he says, "an independent individual," who "neither needed nor accepted advice and supervision." He was "a student of nobody" (Doan 1989).

In fact, his rebelliousness asserted itself at quite an early age. Despite being raised in a milieu of Methodist fundamentalism, and having participated in many church-based activities as a youth, Hubbert had "turned heretic" by the time he reached his late teens, and he fully rejected his earlier religious upbringing (Doel 2007). For the rest of his life he relied on clear thinking and rational thought as his guideposts, and had little use for those who used faith-based reasoning to solve the world's problems.

As we shall see, both these prominent features of Hubbert's personality, his desire to set things right, and his confidence in his abilities to do so himself, played a

role in his writing *The Theory of Ground-Water Motion*. He was jogged into action by his conviction that some of the then-current concepts of groundwater flow were simply wrong, and he did the research and writing of the paper almost entirely alone.

Physics and Geology

Hubbert's first interest in geology apparently arose as part of his loss of faith. He wanted to counter the biblical chronology of the earth that he had learned as a child with a more scientifically based chronology (Doel 2007). Furthermore, very early in his studies he saw the need for greater attention in geology to the fundamental laws of physics. It is not really clear when Hubbert developed this strong desire to couple physics and geology, but it was certainly present by the time he arrived at the University of Chicago where he self-directed his study program to include courses in mathematics, physics, and chemistry as well as a broad suite of the earth sciences. The marriage of geology and physics became a lifelong fixation. He wrote several papers emphasizing these needs in geological education, and expressed his views strongly as a member of the education committees of several professional organizations.

It does not seem surprising that this preoccupation might lead him eventually to groundwater. Clearly, it is one of the branches of the earth sciences that is amenable to mathematical treatment based on physical principles.

University of Chicago

Hubbert's years at the University of Chicago saw the flowering of his fertile mind. However, much to the chagrin of his mentors, he cared little about degrees and worried not at all about meeting the necessary requirements. As Hubbert put it: "I insisted upon obtaining as broad a knowledge of fundamental principles as time and circumstances would allow, and I was thus enabled to avoid the trap of specialization" (Raleigh 1982). Providentially, the faculty recognized his uniqueness and bent over backward to cater to his independent spirit. They found a little-used interdisciplinary program on the books that allowed them to give him his B.Sc. in 1926. He was then awarded his M.Sc. in 1928 and his Ph.D. in 1937 (long after he had left the university), not on the basis of met requirements but rather in recognition of the originality of two of his early published papers.

His time at Chicago also cemented his relationship with the *Journal of Geology* which later became the outlet for his groundwater paper. The journal was not the flagstaff of any of the extant geological associations. Rather it was a highly regarded independent publication, controlled by the University of Chicago geology faculty, and published by the University of Chicago Press. The editor was Rollin T. Chamberlin, and fellow UC faculty J. Harlen Bretz and Francis J. Pettijon were on the editorial board. All three of them knew of Hubbert's precociousness and encouraged him to publish three of his early publications in the *Journal of Geology*. The third of these was his critical review (with Frank Melton) of

the then-widely-held concept of isostasy. In a conclusion that presaged many of his later papers, Hubbert found that "the theory of isostasy rests upon a much more insecure foundation than has heretofore been supposed" (Hubbert 1930).

Columbia University

In 1931, Hubbert accepted an offer to become an instructor of geophysics at Columbia University. It was the beginning of nine stormy years which Hubbert retrospectively described as "stultifying and sterile" (Inman 2016) and "lacking in intellectual companionship" (Doel 1989, Session III). He felt that his ideas were not welcome, and that he was viewed by the other faculty members as a crank.

The source of the conflict was a difference of opinion as to the scope of geophysics in the earth sciences. Hubbert saw it as the broad integration of geology, physics, chemistry, and mathematics in the study of the earth, and he wanted to teach it that way. Columbia wanted him to focus on the narrower application of recognized geophysical tools in mineral and petroleum exploration. In Hubbert's inimitable view, they "didn't have any more idea than a jack rabbit of what geophysics was about" (Doel 1989, Session IV). In 1938, he requested support to set up a comprehensive geophysics program at Columbia, but his proposal was rejected. It was the beginning of the end. Their parting the following year was by mutual consent. Hubbert was sanguine: "They had enough of me," he said, "and I had enough of them" (Clark 1983).

However, it was during the Columbia years that Hubbert published his first truly seminal paper: *Theory of scale models as applied to the study of geologic structures* (Hubbert 1937). It was this paper that the University of Chicago cited in awarding Hubbert his Ph.D. in 1937. In the paper he demonstrated mathematically that rocks can deform plastically at depth. As J.H. Donnay put it in his Day Medal citation: "It once and for all solved the old paradox: how can the earth be at the same time strong as steel and soft as putty" (Donnay 1955). It brought Hubbert his first measure of fame, and ensured that people would pay attention when his groundwater tome appeared 3 years later.

The Great Depression

Hubbert's years in New York City coincided with those of the Great Depression. He had his job at Columbia so probably did not suffer personally during this period. However in earlier years, money had always been tight. During the years before and during his tenure as a student in Chicago, he worked as a waiter, gas station assistant, telephone installer, postal clerk, farm hand, and railroad track layer in order to keep the dog from the door (Inman 2016). It was also necessary for him to take an 18-month leave of absence from his studies in Chicago, during which time he worked for Amerada Petroleum on one of their field crews, to make enough money to continue his academic program. This penchant to follow

through on what one started, and to work hard to achieve one's goals, remained with Hubbert throughout his life. It undoubtedly played a role in the periods of intensive self-directed study that led to many of his most original contributions.

Technocracy

The Depression also gave rise to much discussion in intellectual circles as to how better to organize society to avoid these recurring periods of financial ruin. One such muse was Howard Scott, a charismatic and energetic utopian reformer who proposed a new social order that he called Technocracy, in which the means of production and the distribution of goods would be recast from a price-based market system to one that was operated and controlled by a technological elite consisting of scientists and engineers. He claimed that such an approach would put an end to partisan politics, create greater economic efficiency, reduce graft and corruption, and lead to more rational and objective decision-making (Akin 1977). Hubbert became enamored of this unconventional proposal after meeting Scott during his Columbia years (Doel 2007; Inman 2016). He was one of the founding members of the Technocracy movement, and he authored the official Technocracy Study Guide that was used by the many Technocracy chapters that grew up around the country. It is possible that his flirtations with this somewhat offbeat way of thinking were at least partially responsible for his lack of acceptance by his colleagues at Columbia. In any case, critics were quick to label the movement as anti-democratic and the Technocracy flame was a relatively brief one. Hubbert never spoke publicly about his involvement in it after public interest waned around 1942.

It is not clear whether technocratic ideas had any explicit influence on his professional work, but it is certainly not surprising that the movement's emphasis on rationality and objectivity would appeal to him. As Ronald Doel has described it, "Understanding societal dynamics and uncovering the physical laws governing geological phenomena were, for Hubbert, interwoven and mutually dependent concerns" (Doel 2007).

Technocracy was not the only evidence of Hubbert's nonconformist mind. His marital history also indicates that Hubbert was not averse to the unconventional mores of New York City in the thirties. Hubbert was first married in 1931, to a Latvian émigré named Yelena Pavlinova, but it appears to have been a marriage of convenience that was entered into mainly to allow Yelena to obtain her U.S. citizenship (Inman 2016). They did not really live as man and wife, and the marriage was dissolved in 1936 when Hubbert met the love of his life, Miriam Grady Berry, who was also a divorcee. He and Miriam became lifelong partners, but due in part to the restrictive laws of the time with respect to the rights of divorced parties, they did not officially marry until 1950. It was a long and satisfying union, and while Hubbert does not credit Miriam with a significant role in his scientific work in the Doel interviews, he certainly speaks of her with fondness and respect. They did not have children.

And so we come to the fall of 1939. The Great Depression was essentially over. Roosevelt's New Deal was up and humming. War had been declared in Europe but the United States had not yet become involved. Hubbert now had Miriam at his side. His interest in Technocracy had waned, but his willingness to embrace unconventional wisdom remained. He had just parted ways with Columbia and had no immediate job prospects. So what did he do? He hunkered down and wrote *The Theory of Ground Water Motion*.

The Theory of Ground-Water Motion

Genesis

Hubbert himself stated that: "Two characteristics have been common to most of my investigations: the first, a desire to understand the mechanism of some puzzling phenomenon, and the second, an almost total initial ignorance of the problem to be investigated (Wilson 2007)." Such was the case with his study of groundwater motion. Furthermore, he had time on his hands. "I decided to write a little paper on the subject," he told Doel years later, "lifting myself by my own bootstraps and starting from zero" (Doel 1989, Session III).

In actual fact, Hubbert was not quite starting from zero. He had broached the subject of groundwater flow with the engineering students in his classes at Columbia, and had come to recognize that Darcy's law for the flow of water through porous media was a linear law just like Ohms law of electrical conductivity or Fourier's law of heat flow through solids, and that groundwater flow must therefore take place in a potential field just as these other manifestations of the flow of mass and energy.

His interest at looking at things further was initially piqued by a discussion he had at the 1939 meeting of the International Union of Geodesy and Geophysics in Washington with an old officemate from Chicago, who informed him that many of the papers and books of the day stated that the potential quantity for groundwater flow was the fluid pressure. Hubbert knew from fundamental physical principles that this could not be the case. As he stated to Doel: "It is easy to show that that was impossible" (Doel 1989, Session III). This issue of the incorrect usage of pressure as the potential quantity for groundwater flow became an obsession for Hubbert that stayed with him for his entire life.

The Status of Groundwater Studies, Circa 1940

Of course, by 1940 there was already a reasonably long history of research into groundwater flow, and Hubbert was presumably aware of most of it. Many of the generally recognized classic papers that preceded Hubbert's work are sprinkled throughout the reference list at the end of this paper. Excerpts from several of these historical papers can be found in the *Physical Hydrogeology* volume of the *Benchmark Papers in Geology* series (Freeze and Back 1983).

The earliest work came from Europe, beginning with the classic experimental study of Henry Darcy that led

to the law that bears his name (Darcy 1856). Darcy's colleague, Jules Dupuit, was apparently the first to make widely available published use of Darcy's law in his analysis of horizontal flow to a well in an unconfined aquifer (Dupuit 1863), although his first development of these ideas may have come as early as 1857 (Ritzi and Bobeck 2008). The analysis of unconfined flow to wells is also the topic of subsequent studies by Joseph Boussinesq (1904) and Philipp Forchheimer (1901). (Note that the references given for these two men are not for their original papers, but for their later better-known works where their ideas are more fully expounded.) The recognition that groundwater flow is analogous to heat flow was apparently first made by Forchheimer, and on this basis he made the case for the appropriateness of Laplace's equation for steady-state groundwater flow. All of the European authors work with hydraulic head as their dependent variable (or head squared when the Dupuit assumptions are invoked) but none of them further address the issue of fluid potential.

There were three textbooks on subsurface flow published in the late thirties in the years leading up to Hubbert's work. Cyrus Tolman's book *Ground Water* (Tolman 1937, with some chapters co-authored with Joseph Poland and others) is a very detailed look at groundwater in all its many facets, but it is a highly descriptive work and does not address any of Hubbert's theoretical concerns. The book by Robert Dachler *Grundwasserströmung* (Dachler 1936, in German) is quoted briefly by Hubbert, and probably deserves more attention than he gave to it. It is a short book but it contains a strong theoretical component and includes many quantitatively correct flow nets for flow toward wells and flow beneath dams. The book by Morris Muskat *Flow of Homogeneous Fluids through Porous Media* (Muskat 1937) was a particular *bête noire* for Hubbert. Muskat used fluid pressure as his dependent variable throughout, and in Hubbert's opinion, "The fundamental equation of the entire book was wrong" with no "vestige of understanding of the physics of it" (Doel 1989, Session III).

Hubbert and the U.S. Geological Survey

The center of groundwater activity in America from a very early date was the U.S. Geological Survey. The papers by Chamberlin (1885), Darton (1897), King (1898), and Slichter (1898) presented some of the earliest descriptions of the principles of groundwater movement. Of these, Slichter's paper is the only one quoted by Hubbert, presumably because it is the only one that contained a detailed mathematical treatment based on Darcy's law. However, from Hubbert's perspective, Slichter's analysis was just as flawed as Muskat's because he too used fluid pressure as the potential quantity for groundwater flow. In Hubbert's opinion, the repeated quoting of Slichter's paper by later USGS researchers colored almost all of their subsequent work.

It is unclear how much interaction Hubbert had with the USGS during the period when he was preparing *The*

Theory of Ground-Water Motion. In the paper, he gives some recognition to Oscar Meinzer's report on *The Occurrence of Ground Water in the United States with a Discussion of Principles* (Meinzer 1923), but only for "the geological background of ground-water hydrology." In this he was correct, in that there is no discussion of Darcy's law or the physics of flow in Meinzer's report, and the great majority of it deals with specific hydrogeological environments found in various parts of the United States. There is evidence that some interaction did take place, because in his acknowledgements Hubbert thanked Meinzer and C.E. Jacob for "useful information pertaining to the coefficient of permeability used by the Ground Water Division." However, elsewhere he described the version of Darcy's law most widely used in USGS reports as "incomplete" (Hubbert 1940, 920). There is also evidence that Hubbert found the USGS group closed to his ideas. In the Doel interviews, Hubbert describes the USGS groundwater team as a "self-contained enclave," and Meinzer's leadership of it as "autocratic" (Doel 1989, Session V).

In defense of the USGS, it must be noted that in 1940 their hydrogeologists were almost totally engaged in finding water supplies for U.S. military bases, an assignment that was deemed critical to the budding war effort. They may not have had time for Hubbert's esoteric concerns. Furthermore, Hubbert's paper deals only with steady-state flow. He did not address transient flow to wells which had become a staple of USGS publications. He did not introduce the concept of aquifer compressibility (Meinzer 1928) or any of the other concepts related to aquifer storativity. He quoted Theis' classic paper (Theis 1935) only in passing, noting that the Theis analysis is based on an analogy with heat flow. The later paper by Jacob (1940) that provides a more theoretical foundation for Theis' work appeared in the same year as Hubbert's so it is unlikely he had access to it.

Contributions of Hubbert's Paper

The right-hand column of Table 2 is a listing of the headings that appear in *The Theory of Ground-Water Motion*. The bold-faced titles in the left-hand column are an attempt on my part to group the headings by topic. They do not appear in Hubbert's paper. In its length and coverage and detail, the paper has much more in common with a textbook than it does with the length and style of the usual research contribution.

There are many original insights in the paper, but if we are to try to parse out those that have had an impact on the history of hydrogeology, I would identify the following:

1. Development of the potential quantity for groundwater flow from first principles.
2. Clarification of the constant of proportionality in Darcy's law as a function of both fluid and porous media.
3. Summary of the relationships between flow lines, equipotential lines, stream tubes, sources, and sinks, in a groundwater flow net.

Table 2
The Table of Contents of *The Theory of Ground-Water Motion*

Introduction	Introduction
Theory: Darcy's Law	The Darcy Experiment The Physical Significance of Darcy's Law Analysis of the Parameter K Range of Validity of Darcy's Law Darcy's Law for Gases
Theory: Potential Field	Flow through Extensive Media Application to Fluid Flow Relation between the Potential Field and the Flow Field Refraction of Flow Lines across Boundaries between Different Media Flow from Point Sources and Line Sources
Theory: Multiple Immiscible Fluids	The Boundaries between Inhomogeneous Fluids Slope of the Interface The Elevation of the Fluid Interface Refraction of a Fluid Interface across a Plane Boundary between Media of Different Permeabilities Application to Ground-Water The Effect of Capillarity Influence of Capillarity upon a Two-Fluid Interface The Water Table
Theory: Effect of Temperature and Pressure	The Effect of Temperature and Pressure upon the Fluid Properties Rotational Field of Force
Implications of Theory on Practical Ground Water Issues	Comparison of Present Theory with Methods Used by Others Velocity Potential The Piezometric Surface The Condition for Artesian Water The Problem of Permeability The "Law" of Baden Ghijben and Herzberg
Application to Problems of Current Interest	Applications to Problems of Current Interest Flow Near the Water Table The Johnson Hypothesis of the Origin of Submarine Canyons The Florida Ship Canal
Conclusion	Conclusion

4. A full development of the tangent law of refraction across geologic boundaries.
5. Differentiation of the water table and the air-water interface.
6. Clarification of the limitations on the concept of a piezometric surface.
7. Presentation of his classic diagram of a flow net in a vertical cross-section with the water table as the upper boundary.
8. Definition of the necessary and sufficient conditions for artesian flow.

This list is biased toward issues of interest to groundwater professionals. It is not meant to undervalue the contributions in the paper that are related to multiphase fluids in porous media or the effects of temperature and pressure on fluid properties, both of which have considerable interest to chemical and petroleum engineers.

The Potential Quantity for Groundwater Flow

Hubbert posed the question of potential as follows: "What we seek to find is some physical quantity, capable

of measurement at every point in a flow system, whose properties are such that the flow always occurs from regions in which the quantity has higher values to those in which it has lower." He recognized that the manometer height h satisfies this condition formally, but warns that "to adopt it empirically without further investigation would be like reading the length of the mercury column of a thermometer without knowing that temperature was the physical quantity being indicated" (Hubbert 1940, 795). Then he got down to business. He first noted that viscous flow through porous media is a mechanical process controlled by the friction between fluid and media. He recognized that such a process would be described by physicists in terms of the work done as the fluid moved along a flow line. He also recognized that the work done in moving a unit mass of fluid between any two points in a flow system would be equal to the loss in mechanical energy of the unit mass. As such, flow must occur in a thermodynamically irreversible fashion from areas of higher mechanical energy to areas of lower. He concluded that the potential quantity for groundwater flow must therefore be the mechanical energy per unit mass.

No-one before him had ever thought about things in this way.

From the work of Bernoulli, Hubbert knew that his energy term would have a gravity component, a pressure component, and a kinetic component. For the sluggish velocities of groundwater flow, the kinetic term is negligible, and he was left with just gravity and pressure terms. By using the basic principles of physics he was able to determine the fluid potential at any point in a flow system by calculating the work done against gravity in raising a unit mass of fluid from some datum (usually taken as sea level) to the elevation in question, and the work done in raising the fluid pressure in the unit mass from some datum (usually taken as atmospheric pressure) to the pressure existing at the point in question. In this fashion he was able to show theoretically that the hydraulic head h is a suitable measure of potential, but that the actual potential quantity, physically derived and dimensionally correct, is equal to g times h where g is the acceleration due to gravity.

More to the point, at least in Hubbert's view, is that the pressure p is not the potential quantity for groundwater flow.

It is fair to ask whether Hubbert was making a mountain out of a molehill in his attacks on pressure-based versions of Darcy's law. The case can certainly be made that both Slichter and Muskat knew that there was both a pressure and a gravity component to hydraulic head. They would have argued that their side-stepping of the gravity component arose from their emphasis on the horizontal flow to wells in horizontal aquifers where gravity plays no role. For his part, Slichter did recognize the impact of gravity for vertical flow, at least in part. His version of Darcy's law is presented as a three-dimensional vector, and he first developed the three terms for the case with no "external attractive forces" (such as gravity) and used them to get to the Laplace equation in three dimensions with pressure as the dependent variable. He then developed a second case that includes the impact of gravity, arriving at the correct form of Darcy's law in the z -direction. However, he then incorrectly stated that the pressure-based version of the Laplace equation developed for the nongravity case is "unchanged by the new hypothesis" (Slichter 1898).

Muskat, too, invoked a gravity component in several places in his book. He clearly states that the pressure-based equation is "not sufficiently general to cover all the cases in which we shall be interested. For, when the fluid has a component along the vertical, it is clear that gravity must be taken into account either explicitly or implicitly" (Muskat 1937). Perhaps because of his background in the oil patch, Muskat seemed married to the preservation of pressure as his dependent variable, and then had to graft on a gravity term when it was necessary. For Hubbert, this approach was theoretically and mathematically indefensible. He felt that physics is physics; that formulae that apply to phenomena that occur in three-dimensional space ought to be valid for all three dimensions in all cases.

For those versed in fluid mechanics, Hubbert's use of a potential-based approach to groundwater flow could be viewed as less general than the force-based approach that would arise from consideration of the Navier-Stokes equation. Hubbert is careful to note the theoretical limitations of potential theory in his 1940 paper, and as will be noted later in this article, he returned to Navier-Stokes as a starting point many years later.

The Constant of Proportionality in Darcy's Law

Hubbert recognized that Darcy's law is an empirical law based on experiment, and he was determined to give it a more theoretical foundation. He was especially drawn to the constant of proportionality in the law, which Darcy had denoted with a lower-case k , but which many subsequent authors had capitalized to an upper-case K . Darcy called this constant "permeability" and stated that it was a property of the porous medium through which the water was flowing. First of all, Hubbert recognized that K was not a dimensionless constant, but had to have units of $[L/T]$ to balance the two sides of the equation. Second, he recognized, as others had before him, that K was not just a property of the porous media but also of the fluid, and that discharge through porous media ought to vary directly with the density of the fluid ρ , and inversely with the viscosity of the fluid μ . On this basis, most of his predecessors leapt immediately to the relationship $K = Nd^2\rho g/\mu$, where d is the grain size (or other representative length scale of the media). Hubbert was not satisfied with such a leap of faith, and he used 22 pages of careful physical analysis of the forces and stresses acting on the flow, at both microscopic and macroscopic scale, invoking along the way Newton's law of viscosity, Reynolds numbers, and the concept of kinematic similarity, to arrive at the same relationship. In this expanded version of K we are still left with a constant of proportionality N , but it is now a dimensionless one. If we parse out the Nd^2 term and give it the notation of lower-case k , we have a term with dimensions $[L^2]$ that is a function of the porous media alone.

In recent years it has become standard to call k , with units $[L^2]$ the *permeability*, and K , with units $[L/T]$ the *hydraulic conductivity*. However, Hubbert cannot be credited with this clarification of terminology. Things were quite confused on this score in 1940, and while Hubbert included a multipage discussion in his paper that was designed to clear up the confusion, it was not very successful in doing so, and he never introduced the term *conductivity* for K (which he might have been the most likely to do so, given his familiarity with the analogous usage of thermal conductivity and electrical conductivity in other branches of physics).

Flow Lines and Equipotential Lines

In his paper, Hubbert reviewed the concept of a flow net, and showed mathematically that the family of flow lines will be everywhere parallel to the potential gradient and will form an orthogonal system with the family of equipotential lines (at least, in isotropic media).

He further clarified that equipotential lines must intersect impermeable boundaries at right angles, and be parallel to constant-head boundaries, and he used the equation of continuity to illustrate the fact that for steady flow, the discharge along a given stream tube must be constant.

More importantly, he noted that “it follows that no stream tube of finite cross section can terminate except upon boundaries of the flow region, one end of each tube acting as an inlet and the other as an outlet” (Hubbert 1940, 836). He referred to these inlets and outlets as “sources” and “sinks” and gave an example of a source as “a lake from which water flows into the ground,” and a sink as “a well toward which the flow lines converge” (Hubbert 1940, 836). He did not introduce the terms “recharge area” or “discharge area” but they are certainly implied in discussions later in the paper where he refers to sources “distributed over the air-water interface being produced by downward percolating ground-water,” and sinks that are “limited to the bottom of valleys containing streams” (Hubbert 1940, 928).

All of these things will seem self-evident to the modern hydrogeologist who has been trained in flownet construction since his or her first introductory groundwater course, but in 1940, the whole idea of the groundwater regime consisting of a naturally driven, three-dimensional set of equipotential planes being pierced at right angles by a set of flow tubes delivering water from sources to sinks was a new way of looking at things. It was the birth of the idea of groundwater flow as a component of the hydrologic cycle, with groundwater basins as an alternative unit of study, rather than the studies of isolated aquifers that were then in vogue.

Tangent Law of Refraction

Every modern textbook presents the tangent law of refraction for flow lines and equipotential lines as they cross the boundaries that separate geologic formations of differing permeability. Few of them give reference to any sources earlier than Hubbert, but in fact the tangent relationship is covered in Dachler’s text (Dachler 1936) and he quotes earlier sources. Muskat was apparently aware of the relationship, but mentioned it only in a footnote. In his paper, Hubbert improves on earlier work by developing the tangent law from first principles using Darcy’s law and the conservation of mass. For permeabilities that are several orders of magnitude apart, the tangent law leads to the conclusion that flow in aquitards is often almost vertical, and flow in aquifers is often almost horizontal, a finding that had important implications for subsequent studies of regional groundwater flow, leaky-aquifer well hydraulics, and the analysis of land subsidence.

The Water Table

In his paper Hubbert stated that “it has become standard usage in ground-water hydrology” to define the water table as the “elevation below which water flows freely into an uncased hole and above which no such flow occurs.” Or more broadly, “the water table is conceived to

be a continuous surface ... whose elevation coincides at every point with the position the air-water interface would assume in an uncased open hole were one placed at that point.” He further noted the belief then in “wide currency” that the water table, so defined, “forms a bounding surface between a lower region which is completely saturated with water and an upper region in which the saturation is incomplete,” thus presuming that the water table and the air-water interface that separates the saturated and unsaturated zones are coincident (Hubbert 1940, 895).

Hubbert took on this topic by asking rhetorically whether such a surface has any physical significance, specifically “does it form any kind of a recognizable boundary that one could distinguish visually in a permeable region bounded by a vertical glass plate?” He then used the concepts developed in his earlier discussions on the physics of capillarity at the interface of two fluids in a porous medium to answer these questions, applying these concepts to the specific case of the two-fluid interface that exists between air and water in the vicinity of the water table. He first noted that the pressure at the air-water interface in an open hole must be equal to the atmospheric pressure, and that the water table between holes must therefore be defined as the isobaric surface on which the fluid pressure is atmospheric. He then invoked his capillarity arguments to show that the upper boundary of the zone of saturation will not be coincident with the atmospheric-pressure isobar but will lie above it. He defined the “capillary fringe” as the “region between the isobaric surface, $p = 1$ atmosphere, and the air-water interface, characterized by $p < 1$ atmosphere. He provides a table that shows how the thickness of the capillary fringe will depend on the grain size (or other representative length scale) of the porous media, being very thin for gravels and very thick for clays.

He noted further that the fluid flow will obey “precisely the same laws in the region where $p < 1$ atmosphere as in that characterized by $p > 1$ atmosphere.” He concluded this section of his paper with the advice to hydrogeologists that the conception of the water table as a surface of discontinuity between two zones having fundamentally different physical characteristics, be discarded as a “misleading fiction.”

The Piezometric Surface

Hubbert was not enamored of the use of the so-called piezometric surface to map the heads in horizontal aquifers. He cautioned that “it must be kept clearly in mind that the ground-water potential field and flow field is a three-dimensional field occupying all the space from the air-water interface down to infinite depths, and all that we have done in is to determine the value of this three-dimensional field upon an arbitrary two-dimensional surface through that field” (Hubbert 1940, 910). He further reminds the reader that use of a piezometric surface to make flow calculations requires that the aquifer be truly horizontal, and that it be bounded top and bottom by “rocks so impermeable that the normal components of the flow across these bounding surfaces are negligible.”

On the first point, he argued that calculations will lose their validity “for angles of dip greater than about 3 degrees.” On the second point, he noted the common occurrence of closed highs and lows in the piezometric contours on many aquifers, which offer prima facie evidence that water must be entering the aquifer across the “impermeable” upper or lower boundaries. Near the end of this section of the paper, Hubbert admits that “many of these difficulties are not unrecognized” by hydrogeologists of the day, but he still highlights the “caution that must be observed in drawing conclusions from a piezometric surface” (Hubbert 1940, 912).

It is worth noting that modern hydrogeologists have pretty much abandoned the term “piezometric surface” in favor of “potentiometric surface.” The prefix “piezo” means pressure, and clearly it is the groundwater potential that is being mapped and not the pressure. Given Hubbert’s strong opinions about pressure-based approaches to groundwater flow, it is surprising that he did not raise this issue of terminology in his paper, but he did not. Nevertheless, while he didn’t address the issue directly, it may still be that his emphasis on potential, and his disdain for all things “piezo” may have indirectly fostered the subsequent change in terminology.

The Classic Flow Net

For those of us who are Canadians of a certain age, our introduction to Hubbert came in the form of his classic flow net for groundwater flow in a vertical cross-section in hill-and-valley topography with the water table as the upper boundary (Hubbert 1940, 930). This diagram was the inspiration for the work on regional groundwater flow carried out in the Canadian prairies in the 1960s by Peter Meyboom and Joe Toth. There had been earlier highly schematic diagrams of groundwater flow toward valleys, such as the ones presented by King (1898) and Tolman (1937), but none that made use of the fact that hydraulic heads on the upper water table must be equal to their elevation, thus defining the endpoints of equipotential lines, and none that recognized the need to fill the flow field with a set of orthogonal flow lines and equipotential lines. Hubbert also used this diagram to prove that “flow lines at their point of origin on the air-water interface always dip more steeply than the interface itself,” and that “the region of most intense flow is not a zone parallel to the air-water interface but a region where the flow lines converge toward a sink” (Hubbert 1940, 930). Lastly, the hill-and-valley flow net clearly indicates that groundwater basins are contiguous with surface-water drainage basins, at least in homogeneous media.

Artesian Flow

Hubbert introduced the concept of flowing artesian wells by referring to the “familiar textbook illustration of the aquifer that outcrops in the highlands and then passes under the plains beneath an overburden of impermeable strata.” Such systems were first recognized by Dupuit in the Parisian Basin as early as 1857 (Ritzi and Bobeck 2008) and were further described in an American

context in the early USGS work (Chamberlin 1885; Darton 1897). Hubbert was quick to note, however, that while such a hydrogeologic environment is “a sufficient condition for artesian water, it is by no means a necessary one, and its extensive use has obscured a more complete understanding of artesian phenomena.” He goes on to note that the only requirement for artesian flow from a vertical well is that the fluid potential be greater than the land surface elevation at some point along the wellbore, and that the variety of hydrologic situations that can give rise to such a situation is very great. “As a matter of fact,” he writes, “an aquifer need not be overlain by impermeable materials, and it need not outcrop,” and even if it does outcrop, “the outcrop area may just as well be a region of discharge as of intake” (Hubbert 1940, 914). All of which is probably clear to every modern hydrogeologist, but which was not at all clear in 1940, except to Hubbert, with his unique ability to apply the laws of potential theory to groundwater flow.

Response to Hubbert’s Paper

As Hubbert tells it, his paper made both the petroleum engineering community and the hydrogeological community “fighting mad” (Doel 1989, Session III). “These guys hardly spoke to each other,” he told one interviewer, “but I managed to infuriate both camps. When I say furious, I mean furious. It was rather like having thrown rocks into two hornet’s nests simultaneously” (Clark 1983).

In 1941 Hubbert was invited to summarize his paper at the annual meeting of the American Institute of Mining Engineers (which included the petroleum community at that time) in Washington, DC. Morris Muskat was present and he strongly defended his work in the discussion period. According to Hubbert, Muskat acted patronizingly toward him, and eventually became “emotional and abusive and beyond that there was no further rational discussion” (Doel 1989, Session III).

There was only one published letter-to-the-editor on Hubbert’s paper (Krutter 1941). It came from an instructor at Pennsylvania State College who had used Muskat’s text in his classes and was offended by Hubbert’s attack on it. He did not address the salient point of disagreement about the potential quantity for groundwater flow, defending Muskat on the grounds that his developments were correct “for all cases of practical interest” and “for the particular cases treated.” Hubbert replied that his conclusion that Muskat’s expression for Darcy’s law is physically erroneous “cannot be invalidated by the insertion of escape clauses to the effect that gravity is being neglected” (Hubbert 1941).

Hubbert took one more swing at Muskat in his 1950 review of Muskat’s text *Physical Principles of Oil Production*, a revised and expanded version of his earlier book. In his review, Hubbert pillories Muskat for not correcting the pressure-based version of Darcy’s law, despite “the twelve years which have elapsed” between the two books, and sarcastically suggests that this incorrect form “should hereafter be known as ‘Muskat’s law’” (Hubbert 1950).

Hubbert's Further Work on the Theory of Subsurface Flow

Sixteen years after the publication of *The Theory of Ground-Water Motion* Hubbert published a second paper on subsurface flow, *Darcy's law and the field equations of the flow of underground fluids*, and it too has become a classic (Hubbert 1956). Hubbert had always recognized that Darcy's law, as originally developed, was based solely on experimental evidence. In this later paper, he takes on the task of showing that it can also be determined from first principles, by providing a direct derivation of Darcy's law from the Navier-Stokes equation for the motion of viscous fluids. While my emphasis in this article has been on Hubbert's 1940 paper, his 1956 paper can certainly be viewed as a companion piece, and to the degree that Hubbert is recognized as one of the most influential pioneers of quantitative hydrogeology, this recognition surely rests on the contributions of both his seminal papers on subsurface flow.

Closure

In time, the value of Hubbert's analyses became evident. The controversies died down, all of them more or less settled in Hubbert's favor. Later textbook authors used his papers as the definitive source on the fundamentals of groundwater flow. Then, as his contributions to so many other branches of geology mounted, the accolades began pouring in: the honorary degrees, the Lucas Medal from the Society of Petroleum Engineers in 1971, the Penrose Medal from the Geological Society of America in 1972, the William Smith Medal from the Geological Society of London in 1978, the Vetlesen Prize from Columbia University in 1981, and the Elliot Cresson Medal from the Franklin Institute in 1981.

M. King Hubbert died in Bethesda, MD, in 1989 from a pulmonary embolism.

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References

Akin, W.E. 1977. *Technocracy and the American Dream*. Univ. California Press.

Boussinesq, J. 1904. Recherches théorétiques sur l'écoulement des nappes d'eau infiltrées dans le sol et sur le débit des sources. *The Journal de Mathématiques Pures et Appliquées* 10: 5–78.

Chamberlin, T.C. 1885. Requisite and qualifying conditions of artesian wells. *Groundwater* 5: 131–175 USGS Annual Report.

Clark, R.D. 1983. King Hubbert: Science's Don Quixote. *Geophysics: The Leading Edge of Exploration, Society of Exploration Geophysicists* 2, no. 2: 15–24.

Dachler, R. 1936. *Grundwasserströmung*. Springer.

Darcy, H. 1856. *Les Fontaines Publiques de la Ville de Dijon*. Paris, France: Victor Dalmont.

Darton, N.H. 1897. Preliminary report on artesian waters of a portion of the Dakotas. USGS Annual Report 17 (Part 2): 609–617.

Doan, D.B. 1989. Memorial to M. King Hubbert. geosociety.org/gsa/pubs/memorials.aspx.

Doel, R.E. 2007. Hubbert, Marion King. *New Dictionary of Scientific Biography* 21: 395–399.

Doel, R. E. 1989. Interview with M. King Hubbert. Nine sessions. Center for the History of physics, Niels Bohr library, American Institute of Physics. aip.org/history-programs/niels-bohr-library/oral-histories/5031.

Donnay, J.D.H. 1955. Presentation of the day medal to M. King Hubbert. Proceedings Volume of the Geol. Soc. Am., Annual Report for 1954: 66–68.

Dupuit, J. 1863. *Etudes Théoriques et Pratiques sur le Mouvement des Eaux dans les Canaux Découverts et à Travers les Terrains Perméables*, 2nd ed. Paris: Dunod.

Forchheimer, P. 1901. Wasserbewegung durch boden. *Zeitschrift Vereines Deutscher Ingenieur* 45: 1782–1788.

Freeze, R.A., and W. Back (Eds). 1983. *Physical Hydrogeology, Benchmark Papers in Geology*, Vol. 72. Stroudsburg, Pennsylvania: Hutchison Ross Publishing Co.

Hubbert, M.K., and W.W. Rubey. 1959. Role of fluid pressure in mechanics of overthrust faulting. Part I, Mechanics of fluid-filled porous solids and its application to overthrust faulting. *Geological Society of America Bulletin* 70: 115–166.

Hubbert, M.K., and D.G. Willis. 1957. Mechanics of hydraulic fracturing. *American Institute of Mining Engineers Transaction* 210: 153–166.

Hubbert, M.K. 1956. Darcy's law and the field equations of the flow of underground fluids. *Transactions of AIME* 207, no. 1: 222–239.

Hubbert, M.K. 1953. Entrapment of petroleum under hydrodynamic conditions. *American Association of Petroleum Geologists Bulletin* 37: 1954–2026.

Hubbert, M.K. 1950. Review and discussion: Physical principles of oil production by Morris Muskat. *Journal of Geology* 58: 655–660.

Hubbert, M.K. 1941. The theory of ground-water motion: A reply. *Journal of Geology* 49: 327–330.

Hubbert, M.K. 1940. The theory of ground-water motion. *Journal of Geology* 48: 785–944.

Hubbert, M.K. 1937. Theory of scale models as applied to the study of geologic structures. *Geological Society of America Bulletin* 48: 1459–1520.

Hubbert, M.K. 1930. Isostasy: A critical review. *Journal of Geology* 38: 673–695.

Inman, M. 2016. *The Oracle of Oil: A Maverick Geologist's Quest for a Sustainable Future*. New York, NY: W.W. Norton.

Jacob, C.E. 1940. On the flow of water in an elastic artesian aquifer. *Transactions of the American Geophysical Union* 21: 574–586.

King, F.H. 1898. Principles and conditions of the movements of ground water. USGS Annual Report 19 (Part 2): 207–245.

Krutter, H. 1941. Discussion: The theory of ground-water motion. *Journal of Geology* 49: 324–326.

Meinzer, O.E. 1928. Compressibility and elasticity of artesian aquifers. *Economic Geology* 23: 263–291.

Meinzer, O.E. 1923. The occurrence of ground water in the United States with a discussion of principles. USGS Water Supply Paper 489, 336 pp.

Muskat, M. 1937. *Flow of homogeneous fluids through porous media*. McGraw-Hill.

- Prucha, J.J. 1974. Presentation of the Penrose medal to M. King Hubbert. *Geological Society of America Bulletin* 85, no. 8: 1341.
- Raleigh, C.B. 1982. Citation for the Vetlesen prize to M. King Hubbert. *Transactions of the American Geophysical Union* 63: 249–250.
- Ritzi, R.W. Jr., and P. Boeck. 2008. Comprehensive principles of quantitative hydrogeology established by Darcy (1856) and Dupuit (1857). *Water Resources Research* 44, no. 10: W10402.
- Slichter, C.S. 1898. Theoretical investigation of the motion of groundwaters. USGS Annual Report, no. 19 (Part 2): 295–384.
- Theis, C.V. 1935. The relation between the lowering of the piezometric surface and the rate and duration of discharge of a well using ground-water storage. *Transactions of the American Geophysical Union* 16: 519–524.
- Tolman, C.F. 1937. *Ground Water*. McGraw-Hill.
- Wilson, J.E. 2008. *Intellectual Battles Were King's Feast*. The American Association Petroleum Geologists Explorer January Issue.
- Wilson, J.E. 2007. *King Hubbert: An Irascible Iconoclast*. The American Association Petroleum Geologists Explorer December Issue.