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Random river: Luna Leopold and the promise of chance in fluvial geomorphology

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A B S T R A C T

Luna B. Leopold was a hydrologist and fluvial geomorphologist who headed the Water Resource Division of the U.S. Geological Survey from 1957 to 1966 and was one of the leaders of the postwar quantitative revolution in geomorphology. Like other quantitative geomorphologists of his generation, he turned to numbers partly to strengthen his professional authority in policy debates, particularly those concerning a looming 'water crisis' in the United States. This paper traces the traces the evolution of Leopold's thought from the 1950s to the 1970s, when he turned from seeking universal empirical regularities in river processes to arguing that fluvial systems were inherently indeterminate and unpredictable. While this position was largely rejected by Leopold's scientific colleagues, it was embraced by members of the emerging environmental movement, for whom Leopold's stochastic understanding of rivers provided a seemingly authoritative rationale for the position that complete control and prediction of river systems would always remain out of reach. More broadly, this case shows how quantitative imprecision and uncertainty can sometimes be advantageous for experts seeking to establish authority in a contested policy domain.

This paper examines the promise and limits of quantification as a path to professional authority in the context of fluvial geomorphology as it developed in the United States in the 1950s and 1960s. It focuses on the career and thought of Luna B. Leopold, head of the U.S. Geological Survey's (USGS) Water Resource Division (WRD) from 1957 to 1966, who sought to influence water policy by transforming the WRD from a provider of basic data into the nation's preeminent site for the production of quantitative, mathematical, physical explanations of the interactions between water and land (Fig. 1).[1] Leopold's case is particularly interesting because after his initial efforts at quantification failed to produce generalizable results capable of predicting the responses of rivers to changes in key parameters, he did not abandon his search for universalizable mathematical explanations of river form and function. Rather, he embraced stochastic models that allowed him to lay claim to a kind of 'estimated truth' even when certainty and precision remained beyond reach. This new approach won few adherents among his fellow geomorphologists, but it gave him a powerful voice among environmentalists. His career thus offers an illustration of how the rejection of precision and certainty can under some circumstances actually bolster the authority of the scientist.

More specifically, this case helps to complicate claims about the relationship between quantification and authority in the history of science. Since the 1980s, a series of groundbreaking works on the history of probability, statistics, and quantification have shown how scientists in positions of contested authority have adopted and deployed various forms of quantification from the simplest enumeration to the most complex mathematical modeling to strengthen their positions via à vis competing forms of expertise.[2] In the twentieth-century United States, in particular, Ted Porter has argued that an unusually welcome terrain for 'trust in numbers' was produced by a political and judicial system that relied on adversarial processes and from which some traditional sources of authority, such as the elevated social position of hereditary aristocrats or the divine inspiration of the clergy, were largely excluded.[3] In this context it is no surprise that an expert such as Leopold would turn to quantification to strengthen his professional authority. What is surprising is that when that time tested strategy

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[1] S.P. Wainwright, Science studies in physical geography: an idea whose time has come? *Progress in Physical Geography: Earth and Environment* 36 (2012) 786–812.

[2] L. Daston and P. Galison, *Objectivity*, New York and Cambridge, MA, 2007.

[3] T. Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*, Princeton, N.J., 1995.

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Fig. 1. Luna B. Leopold photographed in the field in the 1970s. Source: U.S. Geological Service.

faltered, he embraced chance, imprecision, and uncertainty as alternative paths to the same end.

This study also contributes to the history of quantification in geography. As in the history of science more generally, the history of geography has generally depicted quantification as a strategy for achieving professional authority, as well as a method of producing more rigorous — if, in the view of many authors, also less complete or capacious — understandings of nature and society. As Tim Cresswell has argued, the rise of quantitative methods in the postwar period reflected a desire to make physical geography more authoritative by making it more 'scientific' according to the standards of the time.[4] In the case of phenomena that vary widely from place to place, however, the authority of cosmopolitan experts wielding sophisticated instruments and reams of quantitative data may pale next to the more embodied, multidimensional, qualitative expertise of local experts.[5] It was precisely such a dilemma that Leopold faced in the 1950s and 1960s, when the kinds of universalizable quantitative claims that were privileged in the policy sphere proved impossible to sustain in the face of geographical variation. His turn toward chance, imprecision, and uncertainty did not resolve this dilemma but did provide him a language for asserting his authority in another domain, that of the emerging environmental movement. More broadly, it provides an example of the kinds of forces, pressures, and contexts that could make quantification in geographical sciences into a disadvantage rather than an advantage, even at a historical moment of intense interest in universal models and quantitative methods.

The quantitative turn in fluvial geomorphology

Leopold's path to the leadership of the WRD had little in common with those followed by his immediate predecessors, who had slowly risen through the ranks after decades working within the division. Rather, he came at the position sideways, with a varied background in both pure and applied science and a boost from his famous father, the wildlife conservationist Aldo Leopold. After completing a degree in civil engineering at the University of Wisconsin in 1936, he followed his father's recommendation to take a position with the Soil Conservation Service, where he worked on problems of erosion in the arid Southwest, a subject that attained national prominence during the Dust Bowl years and that continued to preoccupy him throughout his career. With the outbreak of World War II, he joined the Army Weather Service and moved to Los Angeles to be trained by Jacob Bjerknes, one of the meteorologists responsible for bringing the numerical forecasting methods of the Bergen School of meteorology to the United States.[6]

After the war, following a brief stint with the Bureau of Reclamation, Leopold took a position as chief meteorologist for the Pineapple Research Institute in Hawaii, where among other things he investigated the possibility of weather control. His formal training culminated in a doctoral degree in geology under Kirk Bryan at Harvard University, a specialist in the geology and geomorphology of the arid U.S. West and the teacher of many of the leading U.S. geomorphologists of the postwar period. Leopold completed his dissertation on climate and erosion under Bryan in 1950 and almost immediately thereafter was recruited to join the USGS, where he rapidly rose through the ranks, becoming head of the WRD in 1957, a position he held until 1966. In 1972, he left USGS to take a professorship at the University of California, Berkeley, where he remained until the end of his career.

Leopold's enthusiasm for quantitative and mathematical methods is traceable to his atypical pathway to the leadership of the WRD. At Harvard, for example, he picked up Bryan's skepticism toward the organic, holistic, and evolutionary approaches to the study of landscape development that were commonly associated with physical geographer William Morris Davis, whose methods and models had dominated the field in the first half of the twentieth century. As early as the 1930s, in a direct attack on Davis, Bryan had begun to call for a quantitative revolution in geomorphology.[7] Only by quantifying physical attributes of landforms such as erosion rates, channel form, stream velocity, and sediment transport rates, he argued, would geomorphologists be able to generate genuinely new insights into the ways that flows of water shaped the land and vice versa. Although Bryan's own use of quantitative methods was quite limited, he succeeded in convincing Leopold and many of his other students, including Leopold's future collaborators M. Gordon Wolman and John P. Miller, of their importance.

A number of other developments in the 1930s and 1940s bolstered this quantitative turn in geomorphology. They included the publication of the 1941 book *The Physics of Blown Sand and Desert Dunes* by the British engineer and desert explorer Ralph Alger Bagnold, with whom Leopold later entered into a highly productive collaboration, as well as the influential work of the hydraulic engineer Robert E. Horton, whose 1945 paper on the 'Erosional Development of Streams and Their Drainage Basins', published just before his death, served as a model of the 'rational' study of geomorphological phenomena for Leopold's generation.[8] As Horton put it, the 'physical and mathematical treatment of the subject establishes rational quantitative relationships between the interpretation of observed phenomena accurately and with confidence in the correctness of the results'.[9] The prospect of such quantitatively grounded 'confidence' proved highly alluring to

[4] T. Cresswell, *Geographic Thought: A Critical Introduction*, Chichester, 79–102. See also D.N. Livingstone, *The Geographical Tradition: Episodes in the History of a Contested Enterprise*, Malden, MA, 1992, 304–346.

[5] J. Vetter, *Field Life: Science in the American West During the Railroad Era*, Pittsburgh, 2016.

[6] On the Bergen School, see J.R. Fleming, *Inventing Atmospheric Science: Bjerknes, Rossby, Wexler, and the Foundations of Modern Meteorology*, Cambridge, MA, 2016.

[7] K. Bryan, William Morris Davis — leader in geomorphology and geography, *Annals of the Association of American Geographers* 25 (1935) 23–31.

[8] R.A. Bagnold, *The Physics of Blown Sand and Desert Dunes*, London, 1954; R.E. Horton, Erosional development of streams and their drainage basins: hydrophysical approach to quantitative morphology, *Geological Society of America Bulletin* 56 (1945) 275–370.

[9] Horton, Erosional development of streams and their drainage basins, 369.

postwar geomorphologists.

Geomorphologists could also look to the neighboring discipline of meteorology for a model of the successful use of quantitative, physics based approaches to complex natural phenomena. Leopold's training with Bjerknes during the war exposed him to the Bergen School's thermodynamic approach, which sought to analyze and predict weather systems in terms of transfers of energy from one part of the atmosphere to another.[10] Because the necessary calculations were too complex to be performed by hand, it was only after digital computers became widely available following World War II that numerical weather prediction became feasible. Its success helped generate widespread optimism that similar approaches would benefit other areas of the earth sciences, including the study of landforms.[11] In some cases, the links between the fields were direct. In a 1960 paper on river meanders, for example, Leopold and Wolman noted the similarity between helical flows in the atmosphere as analyzed by meteorologists and similar flows in rivers.[12]

Finally, in the years during and after the war, a general interest in ‘systems’ provided a common language and set of methods for applying quantitative, mechanistic methods to the study of complex phenomena.[13] Leading theorists of process geomorphology such as Arthur Strahler, Richard Chorley, and John T. Hack — the latter one of Leopold's colleagues at the USGS — used the idea of systems to explain how simple physical processes, when linked together through circuits of feedback and control, could produce the appearance of complex goal directed behavior.[14] In geomorphology, such work drew on ideas from information theory and statistical thermodynamics to explain how open and closed ‘systems’ — rather than ‘landscapes’ — developed over time. Deeply functionalist in orientation, it was a useful tool for scientist-engineers such as Leopold who sought to apply basic geomorphological principles to practical problems of land and water management.

In sum, quantitative methods for studying land forming processes had been overshadowed in the early twentieth century by Davisian qualitative geomorphology, which sought to explain long term landscape development in organic and evolutionary terms. From the 1930s onward, however, Bryan and others encouraged a shift toward quantitative methods for studying physical processes. This shift was strengthened by contributions from researchers outside of geomorphology proper, such as Horton and Bagnold. At the same time, quantitative meteorologists such as Bjerknes were using reductive, mechanistic, mathematical methods to study complex natural phenomena like weather at a time when digital and analog computers were making the necessary calculations feasible for the first time. In the 1940s and 1950s, as scientists across disciplines turned to theories of ‘systems’, a new generation of geomorphologists drew on these influences and methods to craft a new approach to the study of landforms that came to be known as ‘process geomorphology’. At the WRD, Leopold sought to use these new methods to bolster his and his colleagues' professional authority at a time of growing national concern about the ‘water crisis’.

Process geomorphology in the Water Resources Division

At the time of Leopold's arrival at the U.S. Geological Survey in 1950, however, these developments were still in their infancy. Quantitative, mathematical, and mechanistic approaches to landforms were still uncommon within the WRD as an institution and in fluvial geomorphology as a discipline. This is not to say that such techniques were completely unknown. Hydraulic engineers in British India, for example, had developed a sophisticated set of techniques for understanding processes of deposition and silting in canals and dams decades earlier. While these methods were sometimes applied to nonengineered rivers, however, most hydrologists and geomorphologists assumed that rivers were too complex and too variable to be analyzed effectively in such terms. What Leopold brought to the WRD as someone who had trained as a civil engineer before turning to hydrology and geomorphology was an unshakable confidence that these concerns were unfounded — that is, that the ‘regime theory’ of canals could be extended and applied to both modified and unmodified rivers.[15]

To do so successfully required more than just conviction however; it also required transforming the WRD as an institution. The division's historical roots lay in the late nineteenth century, when the USGS had established its first permanent stream gauge station.[16] By the 1950s, the work of the Surface Water Branch, the WRD's largest component, was dominated by the maintenance and monitoring of a network of thousands of measurement stations. Concentrated in the U.S. West and often operated in partnership with state and local water agencies, these stations provided a dynamic view of the United States' water resources. Like the mapping and surveying for which the USGS was best known, this was important but routine work, and few hydrologists or geomorphologists thought of the WRD as a site for fundamental research. Rather, the division was mainly known for providing data on groundwater reserves, stream discharge, flood frequency, and other aspects of the nation's water resources that local, state, and federal agencies could use in their decision making.[17]

Leopold's ambition was to transform the WRD into an organization that provided authoritative explanations of the fundamental processes that determined the interactions between land and water. Convinced that the division had lost its way in the previous decades and was desperately in need of reform, however painful, Leopold made many enemies among the WRD's rank and file employees during his nine years as the division's chief.[18] He later recalled that when he arrived at the WRD in 1950, ‘[t]here wasn't a research man in the whole organization’ — a characterization that, coming from someone who saw science as the sole legitimate basis of sound water policy, was both scathing and somewhat exaggerated. Not shy about expressing such opinions during his time at the head of the WRD, Leopold alienated many of the WRD's employees, particularly those stationed at regional offices far from Washington, whose careers had been centered on precisely the provision of basic data that Leopold was now criticizing as insufficiently ambitious

10 On the Bergen School, see K.C. Harper, *Weather by the Numbers: The Genesis of Modern Meteorology*, Cambridge, MA, 2008; Fleming, *Inventing Atmospheric Science*.

11 Harper, *Weather by the Numbers*.

12 L.B. Leopold and M.G. Wolman, River meanders, *Bulletin of the Geological Society of America* 71 (1960) 779. They cited C.G. Rossby, The scientific basis of meteorology, *U.S. Department of Agriculture Yearbook* (1941) 599–656.

13 M. Church, The trajectory of geomorphology, *Progress in Physical Geography: Earth and Environment* 34 (2010) 265–286. On the turn to systems theory during this period even beyond the natural sciences, see H. Heyck, *Age of System: Understanding the Development of Modern Social Science*, Baltimore, 2015.

14 See for example R.J. Chorley, *Geomorphology and General Systems Theory*, Geological Survey Professional Paper 500-B, Washington, D.C., 1962.

15 L.B. Leopold, M.G. Wolman and J.P. Miller, *Fluvial Processes in Geomorphology*, San Francisco, 1964, 266.

16 The U.S. Geological Survey has published an eight-volume administrative history of the WRD and its predecessors from the late nineteenth century through 1994. The first volume is R. Follansbee, *A History of the Water Resources Branch, U.S. Geological Survey: Volume I, From Predecessor Surveys to June 30, 1919*, Denver, 1994.

17 See also the case described in G. Parrinello, Charting the flow: water science and state hydrography in the Po Watershed, 1872–1917, *Environment and History* 23 (2017) 65–96.

18 H.H. Hudson et al., *A History of the Water Resources Division, U.S. Geological Survey: Volume VI, May 1, 1957, to June 30, 1966: The Years of Change*, Denver, 1996, 2.

and ‘scientific’.[19]

Despite the resistance and recalcitraince of much of the WRD's staff, Leopold was able to force a number of research oriented reforms through due to the steadfast support of the USGS director at the time, Thomas Nolan. In 1957, for the first time in its history, the annual USGS budget appropriation included a line item specifically devoted to research. (That is, it included $607,000 for ‘new responsibilities in hydrology’ that was specifically devoted to research in the WRD.)[20] One of his first actions as leader of the division was to ask his team to draw up plans for a ten year research program in sediment transport that would make it possible for the WRD to ‘carve a name again in the field of river hydraulics’.[21] A few years later, he succeeded in having his title changed from Chief Hydraulic Engineer to Chief Hydrologist to emphasize the importance of research over engineering.[22]

In these and many other ways, scientific research became the overriding priority during Leopold's time as head of the WRD. Although he did not shy away from criticizing the WRD as he found it in the 1950s, Leopold also sought to ground his program of reform in the USGS's own traditions, as the ‘again’ in the call for research in river hydraulics quoted above suggests. In particular, he and other process geomorphologists of the era looked for inspiration to the USGS geologist Grove Karl Gilbert, whose early twentieth-century studies of the physical mechanisms of erosion and deposition in rivers served as a counterpoint to Davis's contemporaneous studies of landscape evolution — one that, as later commentators have noted, exaggerated the contrast between Gilbert's and Davis's work.[23] For postwar process geomorphologists, however, the historical reality of the Gilbert-Davis split was not decisive. Rather, references to Gilbert served a dual rhetorical function, simultaneously rooting their approach in the discipline's traditions and identifying it with a supposedly neglected strand of those traditions.[24]

By shifting the WRD's focus from routine data collection to research and from practical questions of hydraulic engineering to the basic science of hydrology and geomorphology, Leopold hoped not only to improve its understanding of the interactions between water and land but also to strengthen its voice in high level policy discussions about the nation's water resources. Expertise in the relationship between flows of water and the shape of the land was in high demand in the years after World War II, when American policymakers became increasingly concerned about the looming threat of widespread water shortages, particularly in the rapidly developing but arid and drought afflicted U.S. Southwest. In the wake of the Great Depression and World War II, the size and scope of the U.S. federal government had dramatically expanded, while the economy of the United States had become increasingly integrated. Many policymakers therefore believed that a national policy was required even if the water crisis was regionally specific.[25] Water shortages, they thought, threatened not only the development of the arid Southwest but also the national economy that depended upon its factories, workers, and consumers.[26]

Precisely what that national water policy should be was the subject of a series of congressional commissions and hearings organized between the late 1940s and the early 1960s. The most influential of these, in terms of both public visibility and legislative consequences, was the Senate Select Committee on National Water Resources that operated from 1959 to 1961. The committee was strongly influenced by industrial, agricultural, and residential water users and the state and federal agencies that served them, and it was particularly attentive to the concerns of Western states such as Colorado, Utah, Arizona, and California. Despite a variety of internecine disputes, the message presented by these water users and agencies was unified — namely, that demand for water was inevitably bound to increase, and that irrigation channels and other large scale engineering works were the best way to meet it.

This position was supported by most federal agencies engaged in water resources issues, particularly the Bureau of Reclamation and the Army Corps of Engineers, who dominated testimony before the Senate Select Committee. Recognizing that regional water users and the federal agencies that supported them were biased in favor of large scale engineering works, however, the committee also invited what it considered to be disinterested experts — that is, experts capable of providing hard facts and realistic predictions regardless of the hopes and wishes of water users. Within the federal government, the WRD was the natural place to look for such expertise, and Leopold was consequently invited to present on ‘Water Facts and Problems’ at the committee's first meeting.[27] His report drew on recent work within the WRD that predicted an imminent water crisis linked to the nation's rapid growth in population and economic activity.[28]

Testimony to Congress was just one of the ways that Leopold and his colleagues at the WRD sought to wield influence over public policy. They also launched direct appeals to the public through newspapers, magazines, and popular books and pamphlets that helped promote the idea that the nation faced a ‘water crisis’ and that the WRD's expertise was essential to resolving it. Among them was a 1960 *Primer on Water* coauthored by Leopold and one of the WRD's senior scientists, Walter Langbein, which stressed the importance of monitoring and studying the nation's water resources as a whole — in other words, the importance of precisely the kind of work performed by the WRD. Because ‘water is a complex resource that varies from time to time and differs from place to place’, and one that moreover has the capacity both to benefit and to harm, they wrote, the ‘Nation must therefore be ever watchful of its water, and the citizen well informed on its changing state’.[29]

In this context, Leopold's advocacy of quantitative, mathematical, physical, and reductionist methods took on a particular political significance. Quantitative measures of the nation's water resources and mathematical models of how those resources varied over time and space promised to make the nation's water problems solvable, while also positioning the WRD and its scientists as the

[19] Leopold Oral History, Berkeley, 1990–1991, 96.

[20] Hudson et al., *Years of Change*, 102.

[21] Chief Hydraulic Engineer [Luna B. Leopold] to Messrs. R.W. Carter, P.C. Benedict and E.L. Hendricks, 18 November 1957, Re: ‘RESEARCH–Extension of the Gilbert experiments on grain transport, and related field investigations’, in Folder: Research: March 28, 1956 thru Dec. 31, 1958: No correspondence for 1953, ‘54, and 195, Box 190, Water Resources Division, General Records, Central Classified Files, 1945–1976, Record Group 57, National Archives, College Park, Maryland.

[22] For a quantitative assessment of the scientific prominence of the WRD during this period, see M.W. Doyle and J.P. Julian, The most-cited works in *Geomorphology*, *Geomorphology* 72 (2005) 238–249.

[23] S.J. Pyne, *Grove Karl Gilbert: A Great Engine of Research*, Austin, 1980.

[24] D. Sack, New wine in old bottles: the historiography of a paradigm change, *Geomorphology* 5 (1992) 251–263.

[25] D. Worster, *Rivers of Empire: Water, Aridity, and the Growth of the American West*, New York, 1985; M. Reisner, *Cadillac Desert: The American West and Its Disappearing Water*, New York, 1986.

[26] A. Needham, *Power Lines: Phoenix and the Making of the Modern Southwest*, Princeton, N.J., 2014.

[27] Leopold's presentation is described in T.M. Schad, An analysis of the work of the Senate Select Committee on National Water Resources, *Natural Resources Journal* 2 (1962) 236. See also Select Committee on National Water Resources, *Report of the Select Committee on National Water Resources*, Washington, D.C., 1961, 78.

[28] W.B. Langbein and W. Hoyt, *Water Facts for the Nation's Future: Uses and Benefits of Hydrologic Data Programs*, New York, 1959.

[29] L.B. Leopold and W.B. Langbein, *A Primer on Water*, Washington, D.C., 1960, 49.

most authoritative and disinterested voices in contentious debates over water policy. By embracing and advancing such methods, Leopold hoped, the division would be transformed from a provider of data and 'water facts', which could all too easily be appropriated by water users and state and federal agencies for their own purposes, into the nation's leading source of water expertise.[30]

The universalizing aims of hydraulic geometry

The influence of these scientific and policy concerns can be seen in the details of Leopold's research program at the WRD, particularly in the program of hydraulic geometry that he and his colleagues developed in the 1950s and 1960s. One of his most important collaborators in this effort was Thomas Maddock, Jr., a hydraulic engineer with whom Leopold had worked at the Soil Conservation Service in the late 1930s. In a landmark 1953 paper on 'The Hydraulic Geometry of Stream Channels and Some Physiographic Implications', Leopold and Maddock echoed Bryan's earlier skepticism about the value of qualitative studies of landforms and landscape development. While such studies remained useful, they argued, they were no longer sufficient. 'Geomorphology cannot move ahead if we remain content to describe processes of land sculpture only in qualitative terms', they wrote. 'When viewed quantitatively, the interactions of the various hydraulic factors are found to be more complex than the qualitative analyses have led us to believe, but the unraveling of their complexities constitutes advance of knowledge'.[31]

Leopold and Maddock's hydraulic geometry represented more than just a turn toward numerical measurements and mathematical equations. It also represented a new optimism about the possibility of identifying empirical regularities, expressed in the form of mathematical equations, that would hold true for landforms and land forming processes under any and all circumstances. Specifically, they argued that various quantifiable characteristics of rivers — including their mean velocity, total discharge, width, depth, slope, channel roughness, and the quantity and grain size of sediment load — could be related to each other in terms of simple power laws, which turned out to be 'greatly similar even for river systems very different in physiographic setting'.[32] The apparent variability of river forms was an illusion, in other words, behind which lay timeless, placeless natural laws that could be expressed in quantitative and mathematical terms and understood without reference to the historical development of a particular landscape or the peculiarities of a particular location. This universality, they thought, was what would make geomorphology into a genuine science.

The appeal of hydraulic geometry rested partly on its practical utility. In contrast to qualitative descriptions of landscape evolution — that is, Davisian geomorphology or 'physiography' — it offered a means of quantitatively predicting a river's discharge rate on the basis of channel characteristics and vice versa (Fig. 2). This was especially useful for extrapolating from instrumented rivers to noninstrumented rivers, an important task given the WRD's mandate to cover the entire nation's water resources and its limited resources for establishing new instrument stations. Moreover, by establishing in advance which characteristics were essential and how they should be measured, hydraulic geometry provided an interdisciplinary language for describing diverse rivers in quantitative terms. As one geomorphologist later reflected, it 'established both the means and the rationale for a working relationship among geomorphologists, hydrologists, and engineers'.[33] Leopold and Maddock's 1953 hydraulic geometry paper consequently set the agenda for much of the research in the WRD's Surface Water Branch in the 1950s and 1960s. It also served as the methodological foundation of *Fluvial Processes in Geomorphology*, a 1964 textbook by Leopold, Wolman, and Miller that served as the standard reference on the subject for decades to come.[34]

Beyond its appeal as a program of research, hydraulic geometry was also useful in the policy domain. It helped the WRD's scientists make the argument that their expertise was both more disinterested and more effective than that of the Bureau of Reclamation or the Army Corps of Engineers. In particular, it capitalized on a scientific resource to which the WRD had privileged access — namely, a vast storehouse of data on stream channels and flow rates produced by its stream gauge network, which could be used to derive the coefficients for hydraulic geometry's power laws and to test their predictive power. Other agencies had their own sources of data, of course, as well as the experts needed to interpret them; the Army Corps of Engineers had hydraulic laboratories and elaborate physical models, while the Bureau of Reclamation had extensive data on the dams and irrigation canals it had built. In both cases, however, the data and models in question were largely specific to particular kinds of construction, such as concrete arch dams, or particular sites that had been heavily modified by human activity, such as the Lower Mississippi River. When it came to natural rivers, neither of these competing agencies could match the range, diversity, and quantity of data collected by the WRD's stream gauge network. By drawing on a dataset to which the WRD had privileged access and offering a set of portable methods for studying and modeling any river under any circumstances, hydraulic geometry therefore promised to provide precisely the kind of expertise that would allow the WRD to position itself as the leading provider of disinterested expertise on the nation's water resources.

The limits of quantification and the promise of chance

Despite the multifaceted appeal of hydraulic geometry, its limits soon became apparent. In particular, the equations that Leopold and Maddock had suggested governed the relationships between key hydraulic factors — equations that resembled those used by hydraulic engineers to evaluate canal designs but were intended to be much broader in scope — proved to be both less universal and less complete than they first appeared. As Leopold and Wolman noted in 1957, studies subsequent to Leopold and Maddock's 1953 hydraulic geometry paper mostly confirmed that discharge and width were related by a simple power law for virtually all rivers. The same studies had also shown, however, that other characteristics of a river, including its velocity, slope, and depth, were 'mutually adjusted' in ways that could not be predicted by the hydraulic geometry equations alone.[35] Under any given set of conditions, as a result, '[t]here is a continuum of natural stream channels having different characteristics that are reflected in combinations of values of the hydraulic factors'.[36] Precisely which combination of values obtained in any particular case was, they admitted, beyond

[30] L.B. Leopold, Geomorphology: a sliver off the corpus of science, *Annual Reviews in Earth and Planetary Sciences* 32 (2004) 6.

[31] L.B. Leopold and T. Maddock, Jr., *The Hydraulic Geometry of Stream Channels and Some Physiographic Implications*, Geological Survey Professional Paper 252, Washington, D.C., 1952, 52.

[32] Leopold and Maddock, *The Hydraulic Geometry of Stream Channels*, 1.

[33] N.J. Clifford, River channel processes and forms, in: T.P. Burt, R.J. Chorley, D. Brunsden, N.J. Cox, and A.S. Goudie (Eds), *The History of the Study of Landforms*, Volume 4*: Quaternary and Recent Processes and Forms (1890–1965) and the Mid-Century Revolution*, London, 2008, 287.

[34] Leopold, Wolman and Miller, *Fluvial Processes in Geomorphology*.

[35] L.B. Leopold and M.G. Wolman, *River Channel Patterns: Braided, Meandering, and Straight*, Geological Survey Professional Paper 282-B, Washington, D.C., 1957, 71.

[36] Leopold and Wolman, *River Channel Patterns*, 73.

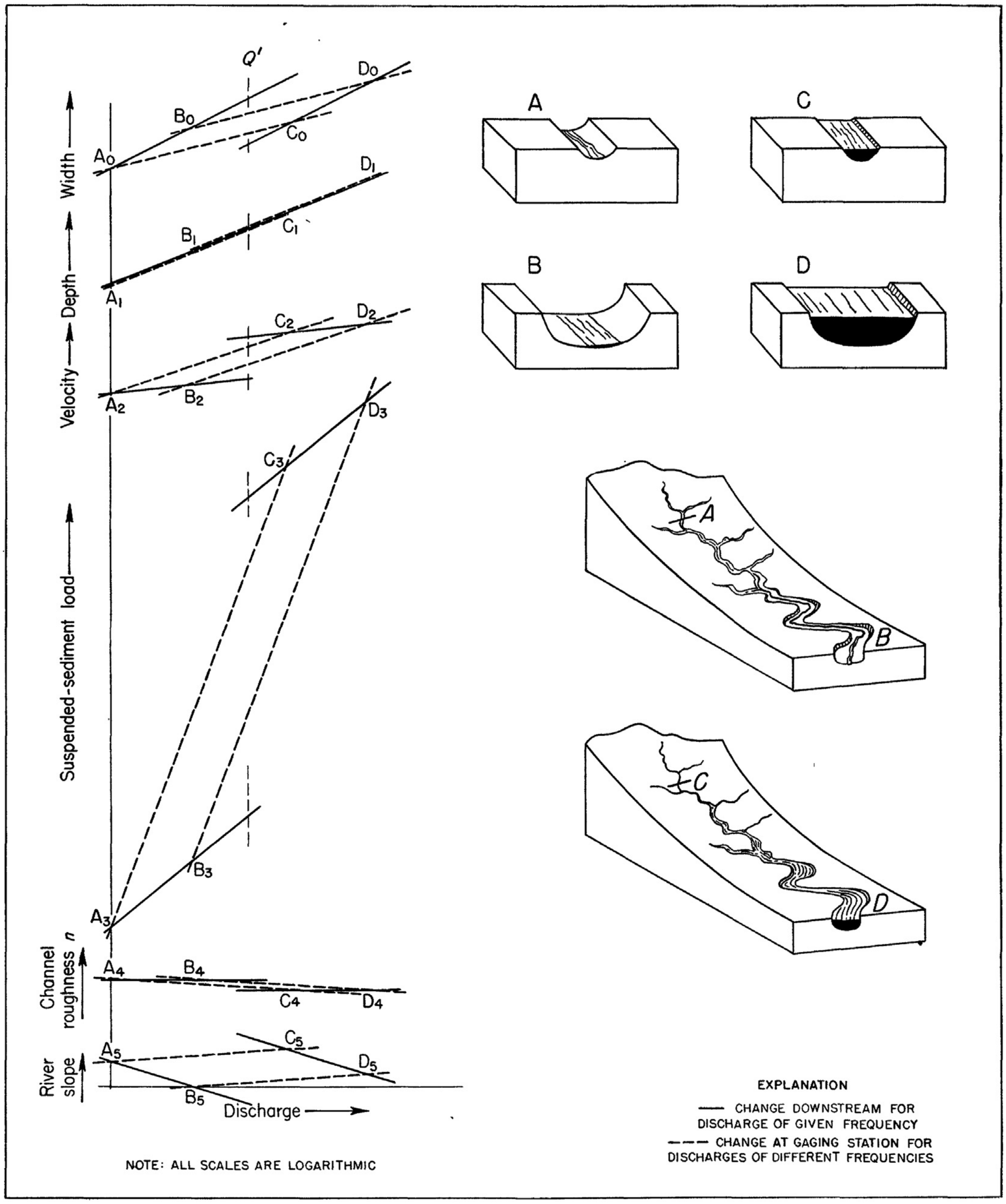

Fig. 2. An illustration of the hydraulic equations describing relationships between discharge and various physical parameters. Source: L.B. Leopold and T. Maddock, Jr., *The Hydraulic Geometry of Stream Channels and Some Physiographic Implications,* Geological Survey Professional Paper 252, Washington, D.C., 1952, 27.

the power of hydraulic geometry to predict. This was a long way from the kind of universally applicable model that Leopold and Maddock had called for in 1953.

The shifting geography and environmental conditions of knowledge production played a role in this wavering of confidence in the potential of hydraulic geometry.[37] Leopold and Maddock's initial study, while including examples from across the United States, had been largely based on rivers in arid and semiarid regions of the U.S. West, where the WRD had traditionally focused its data gathering efforts, where Leopold and Maddock themselves had conducted the majority of their fieldwork, and where national water policy debates were the most intense. This geographical limitation was a weakness in the study that they themselves recognized and later sought to rectify. Once further studies were carried out in other settings, however, the exceptions began to multiply. Tidal estuaries behaved differently than terrestrial rivers, for example, as did seasonal or ephemeral streams, while rivers in the humid eastern half of the United States behaved differently than those in the arid West.[38]

These variations did not invalidate hydraulic geometry, but they did require its claims to be tailored to specific places and

[37] See Parrinello, Charting the flow.

[38] R.M. Myrick and L.B. Leopold, *Hydraulic Geometry of a Small Tidal Estuary*, Geological Survey Professional Paper 422–B, Washington, D.C., 1963; L.B. Leopold and J.P. Miller, *Ephemeral Streams — Hydraulic Factors and Their Relation to the Drainage Net*, Geological Survey Professional Paper 282–A, Washington, D.C., 1956.

conditions. Moreover, understood as self-regulating systems, rivers simply seemed to have too many options in response to any given set of conditions to make precise prediction possible without the inclusion of arbitrary constraints, which often had to be based on scientists' field experience. (In the terms of a later assessment, ‘hydraulic geometry was diagnostic only in particular conditions of a constant load-slope relationships, and, too, where other aspects of channel (plan)form could be neglected’ — that is, where the geological characteristics of particular places could be ignored.)[39] Ironically, by showing how a supposedly universal model needed to be tailored to specific local conditions, the turn toward quantification in the quest for objectivity and authority had made it increasingly evident how dependent precise results were on the scientist's trained judgment and situated field experience.[40]

At the same time, and partly because of these increasingly evident limitations, skeptics of hydraulic geometry were becoming more vocal. Among them was J. Hoover Mackin, a geomorphologist of the old guard — indeed, one of Davis's last students — at the University of Texas at Austin. Mackin had conducted highly respected work on ‘graded’ streams in which erosion and deposition appeared to be in balance, and he was no opponent of using quantification or mathematics in the study of rivers. Nonetheless, he was deeply skeptical of Leopold and Maddock's attempt to develop a universal model of all rivers. As he pointed out, even when the equations of hydraulic geometry were a good fit to empirical measurements, which was sometimes but not always the case, they failed to explain anything fundamental about river mechanics.[41] However quantitative or mathematical they might be, Mackin noted, they remained purely ‘empirical’ rather than ‘rational’ in Horton's sense of the term — that is, they identified regularities in the relationships between various parameters without producing any insight into the physical mechanisms that generated those regularities.[42]

Influenced both by the admissions of failure by the advocates of hydraulic geometry and by the criticisms of outsiders such as Mackin, it was around this time — that is, in the late 1950s and early 1960s — that many fluvial geomorphologists, including some within the WRD, began to admit that the promise of hydraulic geometry had been oversold. Understandings of particular rivers, they asserted, would continue to rely not only on quantitative measurements and mathematical models, the value of which virtually no one was contesting, but also on the scientist's field experience and trained judgment in relation to particular landscapes. Interestingly, however, this was not how Leopold responded. Instead of retreating to trained judgment, firsthand experience, or narrowly tailored studies of specific rivers, Leopold dove headfirst into the murky waters of what he called ‘indeterminacy’ and ‘irreducible uncertainty’.

Leopold's work in this area was critically dependent on the assistance of Langbein, whose mathematical skills far outstripped his and whose interest in quantitative studies of the relationship between topography, rainfall, and flooding dated back to the late 1930s.[43] In 1962, Leopold and Langbein authored a paper on ‘The Concept of Entropy in Landscape Evolution’ in which they admitted that ‘[h]ydraulic equations are insufficient to determine the velocity, depths, and slopes of rivers that are themselves authors of their own hydraulic geometries’ but also proposed a path forward that would continue the search for universal mathematical models of geomorphological processes.[44] That path lay in the adoption of techniques from statistical thermodynamics and in the idea of the inherent indeterminacy and irreducible uncertainty of complex natural systems. Specifically, Leopold and Langbein argued that river systems were constrained by a tendency toward the minimization of the total work done by the river, on one hand, and a tendency toward the uniform distribution of the energy expended across the length of the river, on the other.[45] Within these constraints there were multiple paths a river could take, with the consequence that scientists could at best provide estimates of the probability, not the certainty, of a river taking a particular form under given conditions. In relation to the universalizing ambitions of hydraulic geometry, Leopold and Langbein's 1962 paper on entropy might therefore be seen as an attempt to salvage a ‘rational’ approach to the study of rivers by replacing the deterministic claims of hydraulic geometry with stochastic ones — that is, substituting claims about what a river must do with claims about what it might do.

Leopold and Langbein's stochastic turn thus made it possible for them to continue to develop quantitative, mathematical models even while acknowledging that those models did not allow them to perfectly predict a river's response to any given change. As they argued in a philosophical essay on ‘Association and Indeterminacy in Geomorphology’ in 1963, apparent randomness was not simply an artifact of human ignorance or technical limitations, although both were contributing factors. Rather, it was an inherent property of geomorphological systems and indeed of nature as a whole.[46] ‘We think we see operating in landscape development’, they wrote, ‘a principle long recognized in physics but new to geomorphic thinking — a principle of indeterminacy’.[47]

This principle of indeterminacy rendered hydraulic geometry as it had been formulated in 1953 untenable, they went on to argue. Even with a perfect grasp of the relevant physical laws and complete knowledge of the relevant conditions, it would still be possible to find ‘a spectrum of different dimensions and positions of the otherwise identical aspects’ of a given landscape, such as the patterns of rills that formed when rain fell on an uneroded hillslope.[48] The scope of possible variation might narrow as scientists improved their knowledge of physical laws and their ability to measure initial conditions, but some irreducible uncertainty would always remain.[49] Hydraulic geometry had therefore failed not because it had adopted the wrong methods or models but because it had sought certainty in a realm where nature itself was uncertain.

At a time when the hydrologists and fluvial geomorphologists of the WRD were claiming authority in public debates on the basis of their quantitative understanding of water and rivers, the adoption of ideas of irreducible uncertainty and indeterminacy might seem counterproductive, to say the least. Leopold and Langbein, however, saw no reason for concern. On the contrary, it was precisely because they had embraced randomness and uncertainty that they believed they were uniquely qualified to speak with authority. Even

[39] Clifford, River channel processes and forms, 293–294.

[40] Daston and Galison, *Objectivity*; Vetter, *Field Life*.

[41] J.H. Mackin, Concept of the graded river, *GSA Bulletin* 59 (1948) 463–512.

[42] J.H. Mackin, Rational and empirical methods of investigation in geology, in: C.C. Albritton, Jr. (Ed), *The Fabric of Geology,* Reading, MA, 1963, 135–163. For a more recent summary of the legacies of Leopold and Maddock's work, see C.J. Gleason, Hydraulic geometry of natural rivers: A review and future directions, *Progress in Physical Geography* 39 (2015) 337–360.

[43] W. Langbein et al., *Topographic Characteristics of Drainage Basins*, Geological Survey Water-Supply Paper 968-C, Washington, D.C., 1948.

[44] L.B. Leopold and W.B. Langbein, *The Concept of Entropy in Landscape Evolution*, Geological Survey Professional Paper 500-A, Washington, D.C., 1962, A1.

[45] Leopold and Langbein, *The Concept of Entropy*, A1.

[46] L.B. Leopold and W.B. Langbein, Association and indeterminacy in geomorphology, in: Albritton, *The Fabric of Geology*, 184–192.

[47] Leopold and Langbein, Association and indeterminacy in geomorphology, 188.

[48] Leopold and Langbein, Association and indeterminacy in geomorphology, 191.

[49] Leopold and Langbein, Association and indeterminacy in geomorphology, 189.

if ‘in geomorphologic systems the ability to measure may always exceed ability to forecast or explain’, they argued, the search for ‘stochastic rather than physically deterministic’ relationships promised to lead to improved understanding and prediction, since ‘probabilistic relationships may provide better agreement with actual conditions than the direct physical relationships which have previously been used’.[50]

The turn to chance also had other implications for their contributions to the national water policy debate. In 1961, as Leopold and Langbein were beginning to grapple with the limitations of hydraulic geometry and to reformulate its claims in stochastic terms, Leopold delivered an address to the National Water Research Symposium in Washington, D.C., on the subject of ‘Philosophy for Water Development’, where he questioned the foundations of the presentation on ‘water facts’ that he had given to the Senate Select Committee just two years earlier. There was no such thing as a solution to the water problem, he told his audience, regardless of whether that problem was framed in terms of supply, distribution, or quality. The reasons were twofold. To begin with, since scientists would never have a perfect understanding of water systems, any decision about how to manage water resources would inevitably be based on a consideration of only some of the relevant factors. Secondly, since nature was fundamentally indeterminate and rivers were ‘authors’ of their own paths, any supposed solution to a given problem would inevitably unleash a series of unpredictable consequences. Thus, Leopold concluded, ‘any water policy must be more concerned with the decision-making principles of water management than in decision making for water development’.[51] In other words, developing the capacity to respond to unintended consequences was more likely to be productive than attempting to predict and prevent those consequences in the first place.

The river’s choice

Even as talk of a national ‘water crisis’ in the United States continued through the 1960s, the WRD suffered a series of setbacks in its position vis à vis other federal and state agencies involved with studying and managing national water resources.[52] Ironically, the biggest blow came from the Water Resources Research Act (WRRA) of 1964, which on its face was a direct response to the kinds of concerns expressed by Leopold and his colleagues over the preceding decade about the need for more federal support for water research. In his signing statement for the law, President Lyndon B. Johnson affirmed the Congress's conclusion that ‘acute water shortages are hampering our industries, our agriculture, our recreation, and our individual health and happiness’ and that more and better coordinated research would help solve the problem.[53]

So far, so good; this was precisely what Leopold and the WRD had been advocating. The problem, from the WRD's perspective, was the precise way in which the WRRA sought to promote water research. Rather than entrusting the WRD with the responsibility of coordinating national water research, it gave that responsibility to other agencies in the federal government and increased support for cooperative programs with the states. In comparison, the WRD received a relative meagre budget increase. In plain terms, the WRD's bid to become the preeminent national center for water research had failed. In subsequent years, as some of the WRD's leading scientists retired or moved into academia — including Leopold, who took a professorship at the University of California, Berkeley, in 1972 — the division largely abandoned its ambition of becoming a site for groundbreaking hydrological and geomorphological research and returned to its former role as provider of basic data.

The program of hydraulic geometry and the stochastic models that Leopold and Langbein had developed in the early 1960s also began to falter. Most fluvial geomorphologists remained committed to the quantitative, mathematical, physical, and reductive program of process geomorphology, but they rejected hydraulic geometry in its most ambitious form, including the stochastic variant that Leopold and Langbein had offered in the early 1960s. Instead they pursued a more modest aim that drew on the methods of hydraulic geometry without investing them with the importance they had initially been granted — namely, developing an understanding of the empirical correlations and basic physical mechanisms of river form and function that could be adapted to particular cases in ways that would provide a modicum of insight and control. In light of these more modest aims, the claims about inherent uncertainty and indeterminacy offered by Leopold and Langbein seemed both overdrawn and largely irrelevant.

Even Langbein, who continued to work on stochastic models of geomorphological phenomena, turned away from the more radical claims about indeterminacy and uncertainty that had appeared in his papers with Leopold in the early 1960s.[54] In the mid-1960s, he began collaborating with the physicist Adrian Scheidegger, who shared his enthusiasm for extending statistical thermodynamic models to geomorphology and whose mathematical skills and understanding of physics were far superior to Leopold's.[55] Leopold later remembered Scheidegger as ‘a very famous guy who was so theoretical that nobody could understand him’, a comment that says much about both Leopold's investment in applied research and the gap in physical and mathematical expertise that separated him from Scheidegger.[56]

In his publications with Scheidegger, Langbein rejected the idea that land forming processes were inherently indeterminate and endorsed the rather more modest and pragmatic claim that stochastic methods were useful for estimating processes that were, at root, fundamentally deterministic. As they wrote in 1966, ‘in geomorphology nothing is really random; when it rains, the fall of each raindrop is governed by Newton's law of motion and by atmospheric friction’. As a matter of convenience, a geomorphological process certainly could be studied ‘as if the process were random’, but the processes that led to river meanders, dune fields, hill slopes, and coastlines were ultimately just as deterministic as any other macroscopic natural process.[57] To believe otherwise, they thought, was to admit that geomorphologists would never be able to produce the kinds of certain knowledge that made their discipline worth pursuing in the first place. It was, in the words of a later

[50] Leopold and Langbein, Association and indeterminacy in geomorphology, 191.

[51] L.B. Leopold and E.L. Hendricks, Philosophy for water development, Address at the National Water Research Symposium, Washington, D.C., 28 March 1961, 5.

[52] For examples of ‘water crisis’ talk, see J. Wright, *The Coming Water Famine*, New York, 1966; F.E. Moss, *The Water Crisis*, New York, 1967.

[53] L.B. Johnson, Statement by the President upon signing the Water Resources Research Act., July 17, 1964, in: G. Peters and J.T. Woolley (Eds), *The American Presidency Project*, http://www.presidency.ucsb.edu/ws/?pid=26378. See discussion in T.M. Schad and E.M. Boswell, *History of the Implementation of the Recommendations of the Senate Select Committee on National Water Resources*, Washington, D.C., 1969.

[54] On Langbein's career, see J.C.I. Dooge, Walter Langbein and the emergence of scientific hydrology, *Water Resources Research* 32 (1996) 2969–2977.

[55] A.E. Scheidegger, Some implications of statistical mechanics in geomorphology, *Hydrological Sciences Journal* 9 (1964) 12–16.

[56] Leopold Oral History, 115.

[57] A.E. Scheidegger and W.B. Langbein, *Probability Concepts in Geomorphology*, Geological Survey Professional Paper 500-C, Washington, D.C., 1966, C1. Similar ’as if’ language, probably inserted at Langbein's insistence, appears in W.B. Langbein and L.B. Leopold, *River Meanders — Theory of Minimum Variance*, Geological Survey Professional Paper 422-H, Washington, D.C., 1966, H1.

critic, little more than a 'counsel of despair'.[58]

Unlike Langbein and these later critics, however, Leopold held fast to the notion of the inherent indeterminacy of geomorphological processes and the irreducible uncertainty of scientific claims about them. He included these ideas in the 1964 textbook *Fluvial Processes in Geomorphology* he coauthored with Wolman and Miller, for example, not as a challenge to the hydraulic geometry approach on which the book was based but rather as a necessary step forward that would allow the field to finally achieve its promise, even if it required abandoning certain ambitions.[59] 'When there are a large number of interrelated factors which must adjust among themselves in response to occurrences in the environment, such as storms or flows', they wrote, 'it should be expected that there will generally be an indeterminacy in the manner of this mutual adjustment'.[60]

Two decades later, in a series of oral history interviews conducted at the University of California, Berkeley, in 1990–1991, Leopold described the stochastic and thermodynamic approach to geomorphology as 'undoubtedly. . . the most important idea I ever had', but also one that was 'still so difficult to understand that it's not either very much read or much understood'.[61] As he told his interviewer:

> it turns out that in the river system, there's not just one answer. If you had all the equations that are needed, then you would say, 'If you're given these set of circumstances, the river must do thus and so.' And that's not so. If you're given certain circumstances, the river has several choices, and that choice can be dictated by chance, and indeed is dictated by chance. Now, that is a very important idea because it means that the river has a lot of internal flexibility, all governed by physical laws, but the laws don't make it clear that the river has to do certain things. It can get wider, it can get narrower, it can get faster or slower, it can carry more load or less load. . . . The physical characteristic does not dictate that it has to do only one thing.[62]

It might seem odd that a research program that began with the aim of placing fluvial geomorphology on the firm foundation of deterministic relationships among physical parameters in order to allow scientists to make more authoritative claims in a context of heated policy debates should have ended with the admission that a river has 'internal flexibility' and 'choices' (even if those choices are 'dictated by chance'). For Leopold, however, there seems to have been no tension between these positions. Admitting that rivers were able to act freely within certain probabilistic constraints did not invalidate the science that described those constraints, nor did it undermine fluvial geomorphologists' claims to have a better understanding of rivers than anyone else. It simply made it clear that no one, regardless of how large their datasets or refined their models might be, would be able to predict precisely how a river would respond to a given intervention. Perfect knowledge and perfect control were impossible and would remain so no matter how much geomorphologists improved their methods and models.

One of the reasons Leopold remained satisfied by this position even as his colleagues rejected it was that it resonated with his growing engagement with the environmental movement. After the untimely death of Aldo Leopold in 1948, Leopold had shepherded his father's still unfinished *Sand County Almanac* into publication in 1949, as well as a collection of his essays and journal entries that appeared under the title *Round River* in 1953.[63] The titular metaphor of the latter collection was one of ecological interdependency, with the round river standing for 'the stream of energy which flows out of the soil into plants, thence into animals, thence back into the soil in a never-ending circuit of life'.[64] For Leopold, however, rivers were much more than metaphors; they were the focus of his entire professional career, as well as much of his leisure time. From the late 1950s onward, Leopold became increasingly involved in river conservation issues. In this context, his embrace of uncertainty and indeterminacy served as a means of marshalling science to resist the control of nature rather than to extend it.

The relevance of randomness to Leopold's engagement with the environmental movement can be seen in his advice to David Brower of the Sierra Club on how to challenge the Bureau of Reclamation's plans to build additional dams on the Colorado River from the mid-1950s onwards.[65] Some of the advice that Leopold offered Brower concerned the tradeoff between water storage and evaporation and the relative advantages of dams located at various places along the river — that is, the kinds of utilitarian considerations, framed in quantitative terms, that would lead Brower to accept the necessity of a dam at Glen Canyon, which he and other Sierra Club members later came to bitterly regret.[66] At the same time, however, Leopold recognized that when it came to individual dam projects, as opposed to knowledge about rivers in general, the Bureau of Reclamation had more than enough data and expertise to convince policymakers that its position was the correct one, even if it would ultimately be proven wrong. Leopold therefore advised Brower that it would be strategically unwise to make quantitative claims about the impact of specific dams.[67]

While Leopold was unable to help Brower compete against the Bureau of Reclamation on quantitative grounds, he was able to offer something else of value — namely, a view of rivers as dynamic, unpredictable, and vulnerable that he could claim was rooted in the best science of the day. More broadly, as Leopold's hopes of transforming the WRD into a center for advanced research in hydrology and geomorphology faded and as his engagement with the environmental movement grew over the course of the 1960s and 1970s, including through his involvement with the Sierra Club under Brower's leadership, he increasingly emphasized the unknowability and unpredictability of rivers rather than the power of quantitative data and physicomathematical models to reveal their secrets.[68] In a keynote address delivered to the Governor's Conference on the California Drought in Los Angeles in 1977 and published under the title of 'A Reverence for Rivers', for example, he

[58] A. Werritty, Chance and necessity in geomorphology, in: D. Stoddart (Ed), *Process and Form in Geomorphology,* New York, 1996, 325.

[59] Leopold, Wolman, and Miller, *Fluvial Processes in Geomorphology*, 6.

[60] Leopold, Wolman, and Miller, *Fluvial Processes in Geomorphology*, 274. For a similar, later claim, see L.B. Leopold, Trees and streams: the efficiency of branching patterns, *Journal of Theoretical Biology* 31 (1971) 339–354.

[61] Leopold Oral History, 245.

[62] Leopold Oral History, 246. Leopold's 1994 book *A View of the River*, a summary of research over his entire career written for a general audience, was similarly centered on ideas of chance and entropy as inherent to geomorphic forms rather than simply as artifacts of human ignorance; L.B. Leopold, *A View of the River*, Cambridge, MA, 1994.

[63] A. Leopold, *Sand County Almanac, and Sketches Here and There*, New York, 1949; A. Leopold, *Round River: From the Journals of Aldo Leopold*, edited by L.B. Leopold, New York, 1953.

[64] A. Leopold, The round river: a parable, in: Leopold, *Round River*, 158.

[65] M.W.T. Harvey, *Symbol of Wilderness: Echo Park and the American Conservation Movement*, Albuquerque, 1994; R. Wyss, *The Man Who Built the Sierra Club: A Life of David Brower*, New York, 2016, 16.

[66] Harvey, *Symbol of Wilderness*, 288–299.

[67] T. Turner, *David Brower: The Making of the Environmental Movement*, Berkeley, 2015, 70; J.L. Powell, *Dead Pool: Lake Powell, Global Warming, and the Future of Water in the West*, Berkeley, 2008, 116.

[68] Leopold's involvement with the Sierra Club is briefly described in J. McPhee, *Encounters with the Archdruid*, New York, 1977, 218.

told his audience that the ‘river is like an organism; it is internally self-adjusting. It is also resilient and can absorb changes imposed upon it, but not without limit’.[69] Scientific research might provide insights into such processes, he continued, but complete knowledge would never be possible. The lesson of modern river science was consequently one that resonated with popular environmentalism — namely, that what was needed most was not more precise data or better models but rather a ‘more humble view of our relation to the hydrologic system’.[70]

Conclusion: uncertainty and indeterminacy in context

In summary, the period immediately following World War II in the United States witnessed the emergence of a new project of rendering geomorphology and hydrology ‘scientific’ through the use of quantitative data and mathematical models of physical processes. Epitomized at the WRD in Leopold and Maddock's 1953 proposal for a research program in hydraulic geometry, this approach initially seemed like a promising pathway to professional authority for hydrologists and fluvial geomorphologists at a moment of intense political debates over the national ‘water crisis’. Relatively quickly, however, it became apparent that the relationships among parameters revealed by hydraulic geometry were less universal than initially supposed. Leopold and Langbein responded to this failure by proposing to complement the hydraulic equations with stochastic models derived from statistical thermodynamics — in essence, to account for variability by introducing chance. The appeal of this approach among geomorphologists proved limited, however, and even Langbein abandoned it within a few years. Leopold was one of the few who remained committed to it, for reasons that had to do both with his particular career trajectory and with his involvement in the emerging environmental movement.

One of the lessons we can draw from this history is the importance of the material and social context for determining whether and how quantification and precision are seen as adding to or subtracting from the authority of the expert. Within the context of the WRD and the national water policy debates it attempted to influence, it is clear that Leopold and Langbein's efforts to incorporate indeterminacy and irreducible uncertainty into hydrology and fluvial geomorphology were not a great success. Leopold did not deploy the language of indeterminacy in his congressional testimony, nor did it feature prominently in the factsheets and primers that he and his colleagues at the WRD produced for broader consumption. Moreover, as Leopold's advice to Brower suggests, the WRD's ability to compete with the Bureau of Reclamation and the Army Corps of Engineers in the production of authoritative quantitative predictions of river behavior was limited, particularly when it came to specific engineering projects, where those agencies' site-specific knowledge and ‘as nearly as may be’ ethos trumped the WRD's attempts at universal scientific models (whether ‘empirical’ or ‘rational’). In the context of environmental advocacy, however, Leopold's particular form of expertise did have value, since what he offered was not precise, objective, quantitative predictions but rather a scientific justification for disregarding such predictions when offered by others.

Another lesson that emerges from this case is the close relationship between projects of producing professional authority through quantification and projects of producing professional authority through the rejection of quantification. Without his training by Bryan, Bjerknes, and others in quantitative methods, Leopold would likely never have started down the path that led first to hydraulic geometry and then to the equally universalizing and mathematical but far less certain or precise project of stochastic geomorphology that he developed with Langbein in the early 1960s and remained committed to until the end of his career. In this sense, his embrace of indeterminacy and uncertainty was a product of his commitment to quantitative and universal models. Similarly, turning to the broader institutional and disciplinary shifts that paralleled Leopold's personal development, it is clear that both the advocates of large scale water engineering projects in the arid U.S. West and opponents of those projects such as Brower and Leopold believed that effective planning depended on precise, certain, quantitative knowledge. The main difference between them was that the former believed such knowledge was within reach while the latter did not. Instead, they embraced ‘estimated truths’ — that is, claims about nature that were authoritative precisely because they denied the possibility or the desirability of complete certainty or precision.

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[69] L.B. Leopold, A reverence for rivers, *Geology* 5 (1977) 429–430.
[70] Leopold, A reverence for rivers, 430.