

Hydrologic Sciences

Taking Stock and Looking Ahead

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1 Wolman Lecture: Hydrologic Science . . . in Landscapes . . . on a Planet . . . in the Future

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Background

In 1991 the National Research Council's Committee on Opportunities in the Hydrologic Sciences (COHS or the committee) proposed that there exists, or ought to exist, a distinct geoscience referred to as hydrologic science. Hydrologic science would be analogous to atmospheric science, geologic science, or ocean science but different from traditional hydrology, which the committee equated with engineering or applied hydrology. Both the aims and practice of the newly defined science were to be different from traditional hydrology. The goal of this paper is to examine whether this fledgling science has taken flight; whether it really has become distinct; and, if so, what it needs to sustain flight.

The committee adopted and elaborated on an Earth-science-based definition of hydrologic science, originally proposed by Meinzer (1942) and modified by the Ad Hoc Panel on Hydrology (1962):

Hydrology is the science that treats the waters of the Earth, their occurrence, circulation, and distribution, their chemical and physical properties, and their reaction with their environment, including their relation to living things. The domain of hydrology embraces the full life history of water on the Earth.

This definition reflected developments that had been quietly occurring on several continents since the International Geophysical Year (1957–1958) and that presaged the International Hydrological Decade (1965–1974) and the continuing International Hydrological Program sponsored by United National Educational, Scientific and Cultural Organization (UNESCO). These programs emphasized the study of hydrologic processes at all scales, the role of water in global-scale environmental processes, the need for regional-scale hydrologic analyses, the importance of hydrochemical processes, and the role of humans and other creatures in the hydrologic cycle. Because geophysicists and geographers were instrumental in developing these programs, and

because of the breadth of those two fields as practiced in Europe, the research was seen as relevant to social concerns. It often assimilated hydrological research by engineers and foresters but was free from the pressure of immediate problem solving and design. However, these activities were not widely recognized by most hydrologists and were not strongly coordinated and taught as a coherent science.

The committee concluded that, although fundamentally an interdisciplinary activity, hydrologic science, being concerned with continental processes and their participation in the global water balance, should be viewed as a distinct geoscience interacting on a wide range of spatial and temporal scales with the oceanic, atmospheric, and solid-Earth sciences. Exploring these interactions is fundamental to understanding the behavior of water and the materials that it transports. Two points concerning the coherence of this scientific activity were left unresolved by the committee's suggestions: (1) a large number of scientists who study hydrology participate mainly through groups and funding agencies concerned with hydrometeorology and view themselves primarily as atmospheric scientists, and (2) there is still some question about whether hydrogeologists will integrate their activities within the vision of the distinct hydrologic science proposed by the committee or find it more attractive to act mainly within forums of geology and geophysics (Back, 1991).

Applied Hydrology or Applicable Hydrologic Science?

After reviewing the history of hydrologic applications, the committee argued for the existence and continued support of a hydrologic science distinct from traditional engineering hydrology. This is a point that needs to be carefully stated. The original distinction was not made to deny the value of problem solving or to judge the relative intellectual status of various activities. In fact, the COHS report was replete with promises of contributions to many societal concerns if hydrologic science were to be supported: ". . . the strengthened scientific base of hydrology will contribute directly to improved management of water and environment" (NRC, 1991). The report held out the promise of solutions to specific problems such as "the possible redistribution of water resources due to climate change, the ecological consequences of large-scale water transfers, widespread mining of fossil ground water, the effect of land use changes on the regional hydrologic cycle, the effect of nonpoint sources of pollution on the quality of surface and ground water at a regional scale, and the possibility of changing regimes of regional floods and droughts" (NRC, 1991). The committee argued, however, that historically this prospect had been too immediate: "elaboration of the field, education of its practitioners, and creation of its research culture have . . . been driven by . . . engineering hydrology" (NRC, 1991). Klemes (1988) has also been

adamant that scientific research in hydrology be kept distinct from the technological activity of using hydrology to manage water resources.

The committee recommended instead the study of hydrologic processes and patterns at a variety of scales. Such pursuits would be free from the pressure to generate simulation models with a weak conceptual base, represented by boxes and arrows in flow diagrams, or with parameter values that have been obtained in a cavalier fashion for immediate application in design or regulation. The committee's hope was a long-term strategy, promising that "basic science" would pay off. Since 1991, however, much has changed in the national perspective about what research should be funded. Although basic environmental science seems to have fared quite well despite dire warnings, there is concern on various sides of hydrologic science that the growing call for applications will unduly constrain the kind of research that is supported.

The distinction between traditional applied hydrology and the applicable hydrologic science promoted by the committee represents two subtly different presentations of hydrologic understanding. Engineering courses and texts, which gave most of us our introduction to hydrology, tend to present the subject to users (decision makers, designers, students) as being complete or at least sufficient for action. This tends to focus attention on what is known or at least agreed upon, such as the hydraulics of simple channels or the physics of flow through homogeneous soils. It is natural in such an activity that one tends to search for the simplest definition of a problem to be solved or a task to be accomplished. A typical engineering hydrology task would be to predict flow depth, velocity, and overbank discharge for the design of a river-dredging project that will not cause undue harm to water quality or riparian wetlands (the latter being a relatively modern concern). This approach, focusing on one or a few aspects of the environment and adjusting to them or bringing them under control, has spectacularly advanced human health and welfare. Unfortunately, during the impressive history of this problem solving, society has sometimes changed its mind about what the problem to be solved is and has come to expect more sophisticated, higherdimensional solutions to its needs and desires. For example, the simplification or destruction of aquatic and riparian ecosystems that accompanied many engineering activities formerly admired by society is now generally thought to be undesirable and in need of reversal (as long as the projects retain their capacity to support our material needs, of course!). Also, at least a few of the opinion-making elements of society have realized there are some limits to the stress that environmental systems can sustain and still remain in a preferred state. Meeting these expectations often requires a broader analysis of the systems and their influences than was the case earlier.

Engineering hydrology also tries to put into the hands of regulators, developers, and their designers the tools (equations, mathematical models,

computer codes) that allow objective and quantitative decision making. Raising the confidence of the user, rather than emphasizing uncertainties and digging for deeper forms of knowledge about mechanisms, understandably takes precedence in the mind of the creator or compiler, especially in cases where there is an economic incentive to provide the standard method. Over the past three decades, there have been attempts to make some of these tools more rigorous and mathematical but not necessarily more physically realistic. Alternatively, other hydrologists have tried to make analytical tools more realistic representations of environmental processes by incorporating spatial and mechanistic aspects. After an optimistic beginning, these trends have recently been characterized as uncritical and even misleading. A number of senior engineers have, perhaps unfairly, laid responsibility for both the trend toward elaborate mathematical analysis devoid of mechanism and the optimistic attempts at spatially distributed simulation modeling solely at the door of engineers (Klemes, 1982, 1986; Nash et al., 1990). In fact, responsibility for unwise over-extensions could be laid at many feet. Nevertheless, it is said repeatedly by experienced practitioners (Beven, 1987; Beven et al., 1988; Loague, 1990; Grayson et al., 1992) that uncertainties in physically based simulation modeling are large and, when applied to design, planning, regulation, and other decision making, tend to mislead the consumers of such modeling results. Beven (1987) has even stated that spatially distributed, physically based hydrologic modeling is ripe for some kind of intellectual crisis and revolution that would redefine its scope and realistic possibilities. Similar comments on the misleading aspects of probability analyses, largely resulting from incorrect understanding on the part of users, have been made by Klemes (1982, 1989) and Baker (1993). There is need for a broader discussion of the goals of hydrologic modeling and of the relationship between model construction and empirical investigation than one currently finds in engineering hydrology alone.

The alternative presentation of hydrologic understanding, offered by COHS, is that of a science that confronts and even gains energy from its own uncertainties. In other words, it constantly focuses attention on what is not known and emphasizes the need for empirical exploration and explicit attempts to falsify or validate ideas. Such an activity seeks fundamental knowledge of natural processes, beyond the level required to solve a specific problem in construction, environmental management, or regulation. Freed from immediate problem solving and able to continually assess its own progress, the activity aims to construct and improve a coherent body of general theory. It also profits from lateral perspectives into ancillary sciences, looking for combinations of knowledge or analogous approaches. Thus, it is more likely to define questions about the operation of hydrologic systems in broad multidimensional terms. Activity of this kind has a good track record of discovering phenomena and insights that can be used to anticipate or solve problems in unexpected ways.

The theme of this paper, then, is that there is value in fostering a distinct

hydrological science but that it will remain vital only if

- it discovers new phenomena, processes, or relationships governing the behavior of water and its constituents;
- it focuses on real hydrologic phenomena, such as floods, droughts, drainage basins, material storages and fluxes, and even large-scale engineering effects such as streamflow modification, soil conservation, or channel modifications; and
- it communicates itself well, both internally to develop cohesion and record progress and externally to develop support.

We should concern ourselves with phenomena that are of some interest to society, even if society must be continually informed about the significance of our research targets. The challenge at present lies in defining ways in which hydrologic scientists might gain support for their activities through functioning as engaged members of society. It is neither wise or even satisfying for hydrologists to develop a welfare mentality in which we expect that our research should be supported simply because we exist.

We also need to avoid the delusion of what Petroski (1997) has called the "linear" model of research and development in which it is assumed that basic research is the only precursor to intelligent problem solving. He recounts many examples of practical solutions to engineering needs preceding and even provoking fundamental understanding. The large-scale support for unfettered, curiosity-driven research promised by Bush (1945) seems in hindsight to have been largely a myth, popularized mainly by scientists who have enjoyed considerable freedom under an umbrella of funding provided subtly by some national concern for military, economic, health, or environmental security. Perhaps a more useful model for a scientist's career would be to see oneself as operating in a "web" of information arising both from nature and from society's interests. Keeping oneself broadly informed about nature, and about social concerns and needs related to the environment, can stimulate ideas and feedback and provide opportunities for gaining support, testing ideas, and contributing to human welfare. Thus, the desire for "freedom" from the pressures of immediate problem solving needs to be tempered with an acknowledgment of the value of stimuli that arise from the practical needs of society. This is true even when those needs are as diffuse as the needs of economists and policy makers to understand the limits of our ability to predict the consequences of large-scale changes in climate or land cover.

The following anecdote provides an encouraging example of how a pragmatic concern with water engineering has yielded fundamental hydrologic science of the caliber that any scientist would honor. In the 1950s the water supply engineer Law (1956) pointed out that Britain's post-World War I strategic need to establish a timber supply by planting conifers on the uplands of northern Britain would conflict with water supply to the rapidly modernizing industrial cities of England because increased canopy interception and

evaporation would reduce the yield of runoff into reservoirs. The initial response from foresters was negative. But Law's (1957) field measurements strengthened his argument, which was taken seriously enough by government resource managers and hydrologists to gain their support for one of the most successful long-term paired-catchment experiments documenting land cover effects on water yield (Calder and Newson, 1979). The experiments were illuminated by studies of the physics of plant-water interactions (Rutter, 1967; Calder, 1990; Shuttleworth, 1989, 1991) that are still in operation. Such experiments have proven useful in analyzing land-atmosphere interactions relevant to both river runoff and global climate processes (Shuttleworth et al., 1984; Shuttleworth, 1988; Gash et al., 1996).

The distinction between what is fundamental and enduring and what is immediately useful in hydrology is further shaded by Black's (1995) illumination of the critical importance of "unused" resources. Black points out that, although an individual needs a relatively small amount of water to survive physiologically, much larger amounts are needed per capita to survive as a community and that all of Earth's water is needed to buffer the conditions that allow us to survive as a species. Understanding the state and functioning of water at any of these scales and the relationships between the various scales can be a useful contribution to society, if findings are translated into a form that is accessible to other members of that society. Engineering hydrology was not developed to analyze large-scale processes with multiple feedbacks between various loops in the water cycle. The current large effort led by the U.S. government to explore global sustainability surely ought to include a greater component of hydrologic science, even though it goes unrepresented on the Committee on Global Change Research of the National Research Council (1995).

Upon review this distinction between engineering hydrology, with its emphasis on immediate applicability, and a more measured but still pragmatic hydrologic science may seem artificial and unnecessary. However, it has a powerful influence on how hydrology is taught and practiced. Although it is not impossible to combine both aspects in a career, a graduate education program, or a government agency, there are important differences of emphasis. Most participants will become involved in one activity or the other, rarely crossing the boundary between approaches. There is even considerable suspicion across the cultural divide. Consequently, it is important to emphasize that a distinctive contribution to society can be made by harnessing the power of science in the analysis of hydrologic processes in ways that are not usually brought to bear in most engineering applications.

Impetus for Hydrologic Science

The impetus for conducting a distinctive hydrologic science is the persistence

of important gaps in society's knowledge about the roles of water in the operation of the Earth at all scales from global to soil profile. These roles are so multifaceted and complex that they cannot be elucidated by the narrowly focused approaches traditionally used in hydrology and taught in hydrology courses and texts, which have emphasized rather uncritical "can-do" approaches to the analysis and solution of water control and environmental problems. Although I have argued above that scientists should never forget their responsibilities, I remain supportive of the original COHS recommendation that there is reason to foster a unified discipline of hydrologic science, distinct from immediate design and regulatory needs. The distinctive requirements of such a science are that

- it seeks to identify and study fundamental processes, sometimes going beyond the needs of the immediate task or the community interest of the moment;
- it explores connections to collateral influences on the behavior of water, such as the role of biota, the solid Earth, or the oceans;
- it aims to construct a coherent body of transferable theory; and
- it is obligatorily self-critical, taking seriously the importance of falsification through critical measurements.

There is great value in fostering such activity, both because it is the interesting and distinctive option in hydrology at this time (Klemes, 1986; Beven, 1987; Dooge, 1988) and because such an activity can contribute to understanding problems of keen societal interest and utility, now and in the future. This opportunity can be illustrated with two examples, both drawn mainly from surface hydrology, that is close to my own experience. Although equally interesting contributions are being made in hydrometeorology, hydrogeology, and biogeochemistry, it is easier to make this point with familiar examples. I argue later that, although the promise of making useful contributions to knowledge exists, the continued vitality of the fledgling hydrologic science is not assured, and it needs some attention from those who place value on it.

The two examples derive from assertions that may seem whimsical at first glance. A student will find no more than a passing reference to them in even modern hydrology texts. Yet they are two of the most important realizations in hydrology in the past two decades. The assertions are as follows

- We live on a planet, and that fact has hydrologic significance ranging from the mechanisms by which the energy and water balances of the entire Earth are stabilized in a range that is hospitable for humans and other biota to investment decisions that must be made by local water authorities about their future water supplies. Furthermore, we now realize that human activities can

influence the hydrologic cycle on this scale by altering the chemistry and radiative properties of the atmosphere and possibly by transforming the Earth's land cover on a sufficiently large scale. There is considerable interest in anticipating, or at least defining, the uncertainty about the impact of global change on the hydrologic cycle and water resources.

- We live in landscapes, the topography of which is a dominant influence on spatial and temporal patterns of water storage and surface and subsurface transport of water and its constituents. We also recognize that humans can influence the hydrologic cycle on the scale of even the continental-scale river basins. This is a consequence of the spatial extent of land cover changes and the degree to which humans have intensified or perturbed biogeochemical cycles through pollution and extensive land transformation.

Both of these insights present enormous challenges to hydrologic science and simultaneously provide it with opportunities for contributing to human welfare and reducing ecosystem disruption. They do, however, require us to change, or at least expand, our hydrologic science.

Significance of Planetary-Scale Hydrology

Asrar and Dozier (1994, p. 6) describe the Earth system as "two subsystems—physical climate and biogeochemical cycles—linked by the global hydrologic cycle." Few traditional hydrologists would have thought of elevating the significance of the hydrologic cycle to this level. However, the implied challenge is a measure of the degree to which hydrology has recently been called on, even entrained, by atmospheric scientists to answer questions about the linkage between land surface processes and the atmosphere.

Hydrometeorologists seek collaborations to understand the land-atmosphere interactions that influence tropospheric circulation on all scales, particularly the global and regional redistribution of water and heat. Shuttleworth (1988) and Sellers et al. (1997) summarize recent developments and outstanding questions in this field. In order for them to improve their models of land-atmosphere interaction, hydrometeorologists need to refine their knowledge of spatial and temporal patterns of moisture storage and availability for evaporation. Therefore, they need to know about the distributions of soils, plants, and topography and their roles in holding moisture or releasing it to deep ground water or streamflow. Typical wavelengths and amplitudes of topography in various physiographic regions, as well as regional patterns of

soil and plant distributions, affect the rate at which water drains from a landscape and therefore the amount and pattern of its storage and availability for evaporation. Although there have been many illustrations of spatially distributed hydrologic modeling, most of them have been concerned with outlining model structures and demonstrating that they can be calibrated to match streamflow responses. There has been little use of thoroughly substantiated models to explore the effect of first-order geographical patterns in a way that has some theoretical relevance and widespread applicability, even if only in an approximate way at this time. Eagleson's (1978) papers indicate the combination of rigor and intellectual reach that is needed as a starting point in this field. Blöschl and Sivapalan (1995) and other contributors to the same volume review the hydrologic significance of spatial patterns of soil and topography, and Wood (1995) illustrates how a combination of modeling and remote sensing could yield information about the effect of measurement resolution on computed results.

An important challenge for improving our knowledge of hydrology at these continental scales is to better represent processes, material properties, and boundary conditions that are characterized by such small-scale spatial and temporal variations that they cannot be resolved with foreseeable measurement and computational resources. Such processes and properties have to be represented in models through the strategy of parameterization, which expresses the averaged behavior of these unresolvable effects and their multifarious nonlinear interactions on process rates (e.g., the average rate of evaporation, average rate of erosion, or average speed of water evacuation from a landscape). These effects are often presented as a nuisance that hinders prediction because of "parameter uncertainty." Viewed from another perspective, however, they present a focus for investigating characteristic patterns of material properties, of topography, and of the processes themselves, leading to hydrologic discoveries. There is much new science to be done in investigating these patterns and in discovering new aspects of the behavior of water and the materials it carries, rather than focusing most of the discipline's attention on improving computational methods and on calibration of simulation models. Learning how to represent these patterns of materials and processes offers the prospect of improving their model formulation in ways that will truly enhance our understanding of the hydrologic cycle.

Encouraging possibilities exist for new forms of hydrologic measurement, taking advantage of the revolutions in electronics and particularly remote sensing. However, these advances are not panaceas. Satellite-based remote sensing yields a pixel-averaged view of the Earth's surface from the top of the atmosphere, with varying degrees of spatial resolution depending on the sensor. In general, remote sensing is better at providing spatial coverage than the temporal coverage valued by small-scale hydrology. We must strive to combine remote sensing products with more thorough and creative fieldwork in order to investigate what is being represented. It is difficult to

imagine the range of opportunities offered by satellite-based remote sensing because of the rapid evolution of this technology (Asrar and Dozier, 1994). Much work remains to be done both in interpreting and using remote sensing products for hydrology and in articulating the specific data types and resolutions needed to solve critical hydrologic problems. That work is gradually developing into field campaigns for coordinated measurement and, more slowly, into the development of a theory-based consensus about what spatial and temporal patterns of properties and processes need to be investigated.

The other side of this emergent interest in planetary and continental-scale hydrology is the need to interpret the hydrologic consequences of any anticipated combinations of climate change and other large-scale environmental change, such as population growth and deforestation. Society needs an improved capacity for predicting the status of water resources (*sensu lato*) on seasonal-to-interannual time scales that immediately influence human activities. In addition, the results of global and regional models of change (atmospheric general circulation models [GCMs], demographic projections, land use scenarios) need to be translated into predictions of ground-level conditions and processes such as soil-moisture regimes, ground water recharge, runoff volumes, floods, droughts, lake levels, and soil erosion patterns. This effort has begun (Wigley and Jones, 1985; Wolock and Hornberger, 1991; Marengo et al., 1994) but is stymied mainly by uncertainties in GCM-predicted magnitudes or even signs of changes in annual and seasonal precipitation and radiation loads and the need to interpret precipitation event and plant characteristics to be expected under altered climatic regimes. Uncertainties about terrestrial factors can be partially overcome by cautious space-for-time substitutions that introduce reasonable scenarios of expected changes, even if they cannot yet be predicted from first principles. Such scenarios are more likely to be accurate if generated by experienced field scientists with a theoretical turn of mind.

The problem of anticipating global warming effects may be a little easier for river basins in the snow zone. Lettenmaier and Gan (1990) and Nash and Gleick (1991) have used a simple model of snowmelt and the resulting basin runoff to analyze the sensitivity of snowpack and soil moisture storage and of seasonal and flood flows to various climate scenarios generated by two GCMs for hypothetical enhanced atmospheric carbon dioxide conditions. The results suggest that significant changes should be expected in the seasonal timing of runoff—that is, more runoff in autumn and winter because of lower ratios of snow to total precipitation and lower spring-summer runoff because of lower snowpack storage. Such changes would have expensive consequences for the kind and amount of artificial streamflow regulation that might one day be needed. However, much better spatial and temporal resolution is required in both the atmospheric and land surface models before much confidence can be placed in anything other than the sign of the results. The easily available models, originally developed for calculating the water balance of individual

soil profiles or for quasi-black box forecasting of floods, are simply too crude to rely on the quantitative results. However, the results are convincing enough to give some urgency to the task of improving modeling capability for large river basins. They may also compel society to seriously consider the possibility of major disruptions of its water storage, supply system, and flood risk in the snow zone. In addition to model developments, uncertainty must be reduced concerning representative field conditions in basins typical of the physiographic region. These include the amount of ground water storage, the interannual carryover of water, and the influence of elevation effects on snowpack accumulation and the energy balance. A combination of field surveys and remote sensing of the current range of conditions will be needed to allow model-based extrapolations into unsampled environments. This can best be accomplished with coordinated empirical and modeling studies. Throughout such an effort, however, hydrologists must be responsible for explaining to people the significance of even preliminary results and why such an investment in measurement is required to gradually reduce uncertainty.

By comparison with the climatological and meteorological uncertainties, differences of emphasis among hydrologists about modern process-based models and the difficulties of parameterizing them seem small. Beven et al. (1988), Wood et al. (1988), and Robinson et al. (1995) among others have drawn together the current level of field experience into a convincing theory of how hillslope and channel network characteristics govern basin hydrologic response, including the distribution of soil moisture at a range of geographical scales. This statement is not meant to ignore the massive problems introduced by both systemic and parameter uncertainty in process-based modeling of surface hydrology (Beven and Binley, 1992; Grayson et al., 1992; Beven, 1993; Jakeman and Hornberger, 1993). However, some of those problems arise from a lack of attention to uncertainties that can be reduced with field measurement or from asking intractably difficult questions in the first place. The optimal recursive blend of computation and field measurement is rarely explored in answering broad, socially relevant questions in hydrology. This situation needs to be changed, possibly through strategies suggested at the end of this paper.

Appreciation that planetary-scale atmospheric changes might affect continental climate and hydrology has also resulted in the resurgence of empirical hydroclimatology. Redmond and Koch (1991) have investigated the statistical association between indices of large-scale atmospheric circulation, seasonal precipitation, air temperature, and regional patterns of streamflow during the past half-century. They showed that winter precipitation (October-March) and annual streamflow are generally low in the Pacific Northwest during El Niño-Southern Oscillation events. These conditions involve a weakening of atmospheric pressure gradients across the Pacific, a weakening of easterly winds along the equator, and higher-than-average sea surface temperatures off South America. Equatorial sea-surface temperatures

strongly affect the transport of heat, pressure distribution, and wind patterns in midlatitudes. Values of the pressure anomaly indices known as the Southern Oscillation Index and the Pacific/North America Index during the preceding June-November period correlate with Pacific Northwest rainfall during October-March. Winter air temperature, on the other hand, is negatively correlated with this atmospheric index. Drier-than-average winters thus tend to be warmer than average. This association enhances the variability of snowmelt contributions to streams.

Cool wet winters provide deep snowpacks, whereas the thinner packs accumulating in warm dry winters tend to melt faster during winter and be unavailable for supplying summer runoff. Annual streamflow was also correlated with the Southern Oscillation Index for the June-November period preceding each water year with generally congruent results: during El Niño events, below-average flows can be anticipated throughout the northwestern states. Opposite results occur in the rainfall and streamflow of the desert southwestern region. Cayan and Peterson (1990) found similar positive correlations between the winter index and streamflow in the succeeding half-year in these two regions. Webb and Betancourt (1990) uncovered the association between intense El Niño events and floods in succeeding winters in the desert southwest. Thus, precipitation, air temperature, and streamflow are significantly related to atmospheric circulation patterns occurring thousands of kilometers away and several months before the high-flow season. They vary in a manner that produces alternating runs of wet and drought years and perhaps lower-frequency variability that has not yet been recognized in the relatively short records of atmospheric circulation indices. Whalton et al. (1990) have used even more extensive, though sparser, records to demonstrate large-scale patterns of climate state and their influence on hydrology around the Indian Ocean basin.

These associations raise the possibility of at least short-term predictability of regional precipitation and streamflow as models of atmospheric and ocean circulation are improved. Although the statistical associations are not very precise at present, there could be enormous economic implications of even weak correlations for improved reservoir operation for hydroelectric generation, flood control, and other purposes; the prediction of region-wide streamflow in salmon-bearing streams; and even the magnitude of fish runs several years later (because of the effects of a year of high smolt production on the numbers of fish returning to spawn several years later).

Public policy at all levels of social organization now requires hydrologists to interact with atmospheric scientists, biogeochemists, and others to gradually reduce uncertainty about the terrestrial and atmospheric processes at planetary and regional scales that may be affected by natural and anthropogenic environmental change. This is such a challenging field in which to create true breakthroughs in understanding that it will require a long-term commitment to gradually uncovering the tractable questions, developing

better ways of making measurements, and staying abreast of developments in atmospheric circulation modeling. Thus, it will require the kind of broad scientific training envisioned by proponents of a modernized hydrologic science.

Significance of Landscape Hydrology

One of the earliest formal analyses of the role of topography on hydrology at whole-landscape scales was the regional flow net theory proposed by Toth (1963, 1966). Topographic features of various scales from hillslopes to ridges (Meyboom, 1962) and up to mountain-lowland juxtapositions (Duffy, 1988) drive ground water circulations of various sizes, residence times, response times, and chemical and physical properties. The extent of these circulations, their interdependence, and the transition zones between them could be anticipated, documented, and interpreted. The conceptual and mathematical model, which Toth solved analytically for the simplest case of sinusoidal topography over a homogeneous isotropic aquifer, allowed major breakthroughs in understanding the behavior of ground water systems. It also provided a guide for the application of numerical models that were soon to transform subsurface hydrology.

At the surface, landscapes consist of complex hillslopes converging on channel networks that in map view have a treelike hierarchical structure. Topography creates preferred pathways for overland and shallow subsurface water (Kirkby and Chorley, 1967; Dunne et al., 1975; Beven and Kirkby, 1979) and for sediment (Dietrich and Dunne, 1978; Benda et al., in press) directly through the gravitational effect and indirectly by creating patterns of soil properties such as depth (Moore et al., 1993; Dietrich et al., 1995), macropore concentration (Ziemer and Albright, 1987), and rooting strength (Crozier et al., 1990). The transition from convergent portions of hillslopes to channels is a crucial process threshold (Dietrich and Dunne, 1993) affecting the evacuation rates of water and sediment. The density of channels per unit area is an important landscape characteristic that depends on climatic and geotechnical properties of the terrain (Horton, 1945; Abrahams and Ponczynski, 1976; Dunne, 1980).

As channels converge and enlarge downstream, they widen their valleys and create floodplains in which large volumes of water and sediment can be stored, although the time constants of storage for these two materials are radically different, being days to months for water in a large valley floor and hundreds to thousands of years for sediment (Dietrich and Dunne, 1978; Trimble, 1983; Meade et al., 1990; Mertes et al., 1996). Potter (1978) has interpreted how the continental-scale alluvial river valleys are localized in the context of global tectonics. In addition, empirical studies of the sediment budgets of large rivers and their extensive valley floors are being investigated for natural (Kesel et al., 1992; Dunne et al., 1998) and polluted-

sediment cases (Lewin et al., 1977; Marron, 1992; Graf, 1994). Much work in geomorphology is currently focused on understanding the relationship between the storage of water and sediment in valley floors and the flooding regimes and morphology of the alluvial environment (Gomez et al., 1995; Mertes, 1997; Nicholas and Walling, 1997).

There is, in other words, a coherent qualitative theory of the topographic control of water and sediment movement at the scale of whole landscapes, even up to those of regional scale. New methods of tracing and dating deposits and morphological change have even allowed quantitative interpretations to be made and used for decision making. These landscape-scale theories of basin runoff, material transport, valley floor geomorphology, and inundation mechanisms have direct relevance to attempts to build landscape-scale theories in aquatic ecology (Vannote et al., 1980; Junk et al., 1989; Stanford and Ward, 1993; Power et al., 1995).

Quantitative process-based models for whole drainage basins or regional landscapes that might be used as a basis for reducing uncertainty in decision making are much more fragmentary. Ahnert (1976), Kirkby (1986), Willgoose et al. (1991), and Smith et al. (1998a, b) have modeled the formation of channel networks and drainage basins as the result of hydrologically driven sediment transport. The models reveal how landscape properties reflect the climatic regime, material properties, and geometrical constraints such as tectonic deformation and baselevel change. Beven (1986) elaborates upon the basic TOPMODEL quasi-steady-state approximation for both overland and subsurface runoff from whole catchments. This approach is now widely applied and incorporated into interpretations of large-basin flood runoff (Miller and Kim, 1996) and the surface hydrologic implications of global climate change (Wolock and Hornberger, 1991). As with all other spatially distributed environmental models, the approach is difficult to validate because of constraints on our ability to map transient hydrologic characteristics, but the results agree in a qualitative way with much field experience.

In the analysis of hydrologic response in all but the smallest river basins, a problem results from the large number and diversity of hillslopes and responsive swales (convergent portions of the topography) in a basin. These source areas for runoff possess frequency distributions of topographic characteristics (e.g., gradient, length, concavity), hydraulic properties of soils, and plant characteristics. Their hydrologic response is stimulated by rainstorms that are themselves discrete in both space and time but have definable probability distributions of these hydrometeorological characteristics. Thus, stochastic approaches to runoff generation were proposed to capture the potential variation of responses within a basin and their aggregate behavior, which could then be used to derive a response function that could be routed down a channel network. Basin hydrologic response to storms could then be derived from hillslope runoff and channel

network structure (Rodriguez-Iturbe and Valdes, 1979; Gupta et al., 1980). This approach, named the Geomorphologic Instantaneous Unit Hydrograph, provided one of the most promising, simplifying insights in attempting to build a theory of basin response. Robinson et al. (1995) have examined the respective influences of hillslope processes, channel routing, and network geomorphology on the hydrologic response of natural catchments. Benda et al. (1998a) and Benda and Dunne (1998b) have developed a similar stochastic approach to the generation and routing of sediment from drainage basins, examining the influence of basin size, erosion processes, rainstorm and fire climate, and other controls on regimes of sediment transport and storage.

As in the case of continental-and planetary-scale hydrology, all of these attempts at theory building are limited by difficulties in representing subgridscale processes, measuring the spatial variability of landscape properties, and measuring the temporal characteristics of climatic and hydrologic responses over entire drainage basins. In some environments and at some scales there also exist continuing systemic uncertainties about the physics and chemistry of the mechanisms themselves. Fortunately, the systemic uncertainties are probably reducible to a significant degree through continued emphasis on programs of field observation. In addition, improvements in technology can probably assist in characterizing the variability because the sources of variability themselves are quite well understood (Seyfried and Wilcox, 1995). However, the complexity of the mechanisms and the intensity of property variation require an approach that in other fields is called "coarse graining" (e.g., Gell-Mann, 1994, p. 29). The goal is to formulate scientific problems with only the necessary degree of complexity, using simplified expressions to represent processes and their interactions. When formulating a particular problem, the scale must be chosen judiciously to maximize its utility. This challenges the belief that only progressively finer-scale studies are really "scientific" and "rigorous" and that "scaling up" from some presumably fundamental understanding is the only way to solve problems in hydrology. As we try to "scale up" our understanding of many hydrologic phenomena, either in formulating theory or in interpreting measurements such as satellite products, it appears that different processes exert their complicating influence at each scale. At some point we encounter the current limits of our ability to formulate, measure, or compute, and we resort to the strategy of parameterization of subgrid effects, as discussed above. Once again, at the landscape or basin scale, this need for intelligent and thorough parameterization provides a focus for integration of theoretical and field investigations.

Problems of measurement also account for the difficulty of verifying any of these theories quantitatively. For example, the models used for illustrating concepts of catchment runoff response summarize extensive field experience; they calculate the consequences of accepting various postulates about mechanisms and parameter values that they include. However, as

Beven (1989, 1993, 1996) and other experienced modelers have often repeated, the quantitative results that the models provide are highly uncertain because of parameter uncertainty and in some cases the inaccuracy of current modeling concepts. Yet they are often mistaken for reliably precise prediction methods by those looking for a convenient, apparently objective way of making decisions about environmental management. Model capabilities are frequently overstated, or at least not critiqued, by their developers and other proponents. Much self-critical work needs to be done to define realistically the levels of uncertainty in model results. This is necessary if, in the face of such uncertainty, we are ever going to be able to provide scientific advice on how to make environmental management decisions about, for example, whether extensive timber management has long-term consequences for flood levels (Jones and Grant, 1996).

Given the importance of regional-scale river basins and their valley floors as loci of settlement, agriculture, commerce, and ecological values, it is surprising that so little theory and observation have derived from the major floods that bring both catastrophe (Chagnon, 1985) and productivity (Junk et al., 1989) to the river corridors of large basins. At drainage areas greater than about 10^4 km², physical theory in flood hydrology seems to be abandoned in favor of statistical analysis of floods as random variates. Klemes (1982, 1989) and Baker (1993) have forcefully argued that this was never a good idea, given such well-known difficulties as hydrologic persistence, the variety of flood-generating meteorological events, potential anthropogenic influences, and the manifest nonstationary behavior of the few century-long flood records. The strategy is even more likely to be misleading when society is facing uncertainty about the effects of environmental change on floods and droughts. The technique of estimating the probabilities of future floods and droughts based upon an analysis of the historical record of discharge extremes may have been reasonable in the early days of engineering hydrology, when engineers needed realistic, if approximate, estimates of the risk to projects. It is also understandable that the convenience of the method, and the need for a lingua franca with which to express risk to regulators and investors with an actuarial point of view, would lead to a high level of mathematical development of this field of statistical hazard estimation (Maidment, 1993). However, says Klemes (1989, p. 43), "the main weakness of the analysis is that it takes no account of the actual climatic, hydrological, and geophysical mechanisms that produced the observed extremes." A focus is needed on the processes that actually generate floods, including extreme runoff generation and therefore large rainstorms, snowmelt rate and timing, the slow drainage of previously wet soils, land cover changes, land drainage, and even dam failures or operational inefficiencies.

In recent decades there has been little quantitative study of floods as processes or of how the process of flood generation is influenced by large-

scale environmental controls, especially in large basins. Although attention is now being paid to the mesoscale atmospheric circulation processes that govern rainfall character, there is no comparable ground-level investigation of the flood-generating role of season-long soil moisture patterns, regional-scale patterns of soil waterholding properties and topography, the hydrographic structure and disparate geography of major tributary basins, valley floor geometry, regional land use change, land drainage, or diking over 1,000-km scales, as occurs along the Mississippi River and is proposed in the Bangladesh Flood Action Plan. Mertes (1997) and others are only just beginning to reevaluate the complexity of valley floor inundation. From the debates following the 1993 Mississippi floods, there appears to be little quantitative consensus about the role of valley floor storage in modulating the peak discharge of large protracted floods except near levee breaks. Aside from direct flooding hazards to the habitability of valley floors, we have little quantitative understanding of the hydrogeology of valley floor drainage and its role in wetland distribution and functioning or of floodplain sedimentation (Gomez et al., 1995; Nicholas and Walling, 1997) and the disposition and recruitment of contaminated sediment and water in populated floodplains (Marron, 1992; Meade, 1995). All of these are process-related issues that could be the focus of significant research aimed at improving the habitability of alluvial lowlands, now that large parts of them are irreversibly developed.

The 1993 floods in the Missouri and Mississippi basins, which caused approximately \$12 billion in property damage (Myers and White, 1993) as well as other monetary losses, massive water pollution, and damage to dikes, generated an extensive but short-lived debate about how the nation should respond to the prospect of future similar inundation. However, the debate was not guided by hydrologic science. In fact, in the four years since the nation's largest hydrologic event, one can find virtually no analysis of the flood in the several front-rank scientific journals in hydrology. Nor have these journals published any fundamental reanalysis of the nation's options and opportunities for creative responses to future events, either immediately after the flood or in succeeding years when there was time for more leisurely analysis of the runoff generation and storage processes governing the magnitude of the flood. By comparison, the equally momentous 1980 eruption of Mt. St. Helens generated significant scientific field investigation and data collection that was assimilated by the geological and geophysical community and later applied to guiding the social response to eruptions at Redoubt and Pinatubo volcanoes. After the Mississippi flooding, federal agencies compiled data reports, but the hydrologic scientists seem to have ignored the largest hydrologic event in this nation during their careers. There was, however, the usual debate about the computed recurrence interval!

Can Hydrologic Science Tackle Complex Large-Scale Problems?

The types of problems outlined above are complex, multidisciplinary, and challenging, and even the definitions of some of them need refinement. Those outlined are not even the most complex problems for which society could use solutions. The question naturally arises of whether they are too complex to be tractable at any fundamental level that would be called scientific by some of the critics of hydrology (Dooge, 1986; Klemes, 1986) or whether they lie in the area of "transcience" (Weinberg, 1972).

It could be argued that when some form of objective calculation is necessary we should rely on calibrating low-resolution models for large-scale hydrologic phenomena and concentrate studies of "fundamental" processes and complicated calculations at small scale. But the relevant processes are not limited to small-scale phenomena. The gathering of a flood in a continental scale river valley is a process. So is the impact of regional drought on the outcropping and discharge of ground water in a stream network or the down-valley advection and diffusion of a sediment wave released from hillslopes by agricultural colonization. Second, though it may be difficult, improving the analysis of large complex problems is an extremely interesting option for hydrologic science at this time. Examples include understanding the hydrologic significance of persistent planetary-scale atmospheric conditions or the consequences of perturbing the surface properties of regional landscapes.

Scientific analysis of complex large-scale processes can generate fundamentally new ways of looking at hydrologic change, require new modes of observation, and stimulate new questions for analysis at smaller scales. It requires some judicious application of the coarse-grained approach, referred to above (Gell-Mann, 1994), so that integrative theories may be developed similar to those of Toth (1963), Beven and Kirkby (1979), or Rodríguez-Iturbe and Valdes (1979). Such an exercise may force us to conclude that certain questions about small-scale hydrologic processes have been adequately researched for the present and are not likely to yield progress until someone produces a new integrating concept. In surface hydrology there is, for example, a particular need for replacing storm runoff models based on the Richards equation with more realistic descriptions of flow in macropore-filled regolith, the large-scale geometry of which may have characteristic patterns within a drainage basin. Another example is the need to modify or even replace current models of sediment transport that were originally developed and calibrated in flumes to compute instantaneous transport under an infinite sediment supply. A more realistic view is that of episodic sediment transfer down river networks during floods of finite duration, during which the supply of sediment may be limited by external processes or the state of the channel bed.

Developing realistic integrative theories about large-scale complex processes, even if they are coarse grained, would allow hydrologic scientists to focus on research targets that are of broad significance and would attract sustained

interest from scientists in other fields and society at large. Atmospheric scientists have pointed the way by studying how the energy and water balances of extensive continental surfaces affect atmospheric dynamics at regional and larger scales. That information is needed for reducing uncertainty in global circulation models, but once the activity grew it became apparent that it could also lead to improvements in local meteorology and hydrology. Such research activity arises within a field—in this case, atmospheric science—which has a strong theoretical tradition and therefore a basis for internal communication, agenda building, and the design and advocacy of short- and long-term data collection programs. Hydrologists would be well advised to emulate these skills from a close sibling group of colleagues who also work outdoors. A key to being able to tackle large complex problems appears to be the ability to forge a consensus approach between theoreticians and empirical investigators.

Has Hydrologic Science Taken Flight?

Some recent commentators on the status of hydrologic research have referred to hydrologic science mainly in terms of its potential but have concluded that for the present the niche of geoscientific hydrology is essentially empty. Although such conclusions are meant to be taken as stimulation rather than condemnation, it is probably time, after a decade of such conclusions, to acknowledge that a review of the literature would leave one quite impressed. During the past decade, the output of all hydrologic journals has increased dramatically, and in the main research journals, such as *Water Resources Research*, *Journal of Hydrology*, *Journal of Geophysical Research*, *Reviews of Geophysics*, and *Hydrologic Processes*, many papers have appeared that would be classified as scientific hydrology by even the harshest critics of past practices. That conclusion can be extended to those journals to which hydrologists have contributed more recently, such as the *Journal of Climate* or *Earth Surface Processes and Landforms*. The problem is that the essence of hydrologic science as proposed by the COHS does not emerge clearly amid all of the other, albeit high-quality, papers on topics related to algorithm development, site characterization, and management.

Another measure of activity could be the types of hydrologic and related research funded by the Hydrologic Sciences, Water Energy, Atmosphere, Vegetation, and Water and Watersheds programs of the National Science Foundation between 1993 and 1997. Many fine ideas have been pursued, but they demonstrate more diversity than coherence. The Hydrologic Sciences program, in particular, has represented the breadth of opportunity expressed in the original COHS report that recommended its establishment. This is not to suggest that diversity is a bad thing for hydrology, merely that it would be difficult for an outsider to recognize the coherence of hydrologic science from the research projects funded so far by these programs. One could argue that, since it is wise for a fledgling science to invest in variety, the current low

funding levels preclude the development of critical mass in particular foci. While this may be a valid point to some, it does not hide the apparent lack of a coherent agenda in terrestrial hydrologic science. By contrast, in the hydrometeorology component of hydrologic science, funded outside these programs, both a more coherent agenda and a higher level of funding are apparent. Presumably, more significant funding was not granted as a whim but in response to a well-defined, coherent, and skillfully articulated and publicized research agenda. The proposals caught the imagination of policy makers and budget managers, who could clearly see the social benefits or at least public interest in the scientific ideas being propounded. By contrast, the hydrologic science of the ground surface and subsurface, though visibly in flight, is flapping around in sight of many exciting possibilities, needing to catch a good breeze in order to sustain flight.

What is Needed to Sustain Hydrologic Science?

Much excellent scientific hydrology is under way, and there are many attractive and socially useful scientific questions to work on, particularly at landscape and continental scales in addition to the hillslope, site, and water catchment scales that are already well represented. However, these questions about larger-scale processes will remain difficult to research in a scientific manner unless we can devise theories and measurements appropriate for those scales. To do so we need to solve some internal problems about the ways in which we interact in research and teaching and in how we communicate among ourselves and with society. I will close by making some suggestions for ways of overcoming some of our difficulties in this regard.

Convergence of Approaches

Dooge (1988, p. 79) has recommended that hydrologists should resist the fragmentation of approach to their subject. This point needs continual emphasis simply because it is so difficult to achieve and yet is an obvious impediment to the development of hydrologic science. Most of us enter hydrology from other fields, having no tradition of communicating except to sponsors and graduate school colleagues who identify with our particular interests in hydrologic contributions to flood management, water supply, soil science, water quality, or landscape evolution. We have learned empirical approaches in the field or laboratory, or mathematical approaches if that was more attractive, and we tend to search for problems, places, and sponsors that are amenable to the practice of our skills. Most of us are too busy or disinterested to broaden our skills in any radical way after graduate school, and we are not usually encouraged to do so. Thus, we become identified as a

particular kind of hydrologic scientist: geomorphologist, soil physicist, ground water hydrologist, surface water hydrologist, etc. We attend different sessions at scientific meetings, and spend little of our time contemplating the connections between weather, surface water, soil hydrology, and ground water that—for example, at the scale of a large river basin—might hold keys to the generation of floods. We become identified as "modelers" or "field hydrologists," "theoretical hydrologists" or "just descriptive." Few of us, with some notable exceptions, struggle to avoid the ossifying identification, and it is not much of a step from there to begin denigrating other approaches in the competition for support of all kinds. Yet experienced scientists repeatedly emphasize that scientific breakthroughs commonly arise when scientists break out of their disciplinary isolation and collaborate in the unexplored territory between specialties.

Specialization is obviously required for skill building, so I am not proposing anarchy in the organization and conduct of the science. However, imagine that early in graduate school and through constant mentoring we were reminded that (1) we are not owed a living but should seek to contribute to human welfare if we want to be granted the good fortune of a scientific career, (2) we can harness the power of science to provide society with an intellectual compass for adjusting sustainably and productively to its environment, and (3) the scientific questions on which we might therefore work are not sensitive to disciplinary differences or status concerns among hydrologists. Perhaps then we could view the differences in our skills as resources rather than impediments to the development of more unified approaches to the study of water. Such a graduate school orientation would encourage even extremely specialized scientists to appreciate different approaches in related fields of hydrology, to take seriously the need for communication with other hydrologists, and to express the connections between their own results and other insights obtained at different scales and by different means.

Hydrologic scientists will continue to come from a broad range of backgrounds. Even if new graduate programs are developed in hydrologic science—which is an excellent idea that some universities can afford—most people entering the field will be trained and employed outside such programs for the foreseeable future. Back (1991) has even recommended that hydrogeologists resist being trained in a unified hydrologic science and instead stay grounded in departments of geological science. Whatever the mix of backgrounds, respect and interest must be built across them in order to encourage hydrologists to spend the time necessary to learn and unite the various forms of understanding that we develop. This requires communication that is not even widely valued at present. There will be hydrologists who are skilled at abstraction and the formalization of theoretical models. Such people are needed to see through the complexity and disparate impressions of our experiences and to develop useful generalizations. Other scientists are intrigued by particular observations,

particular cases, or field observations of many disparate cases. They have seen a great variety of environments and measured hydrologic events or distributions, and they have good integrative skills, which gradually yield generalizations. They tend to be the ones who discover processes and relationships in real environments, but they do not spend much time on generalized theory, and some of them do not even value it.

We are fortunate to have this range of skills among people interested in hydrology, but it is unfortunate and debilitating that these individuals or groups rarely communicate and sometimes even demean each other's contributions. There are exceptions (Stedinger and Baker, 1987), but they are rare. University departments often choose to hire only one kind of hydrologist. Thus, their graduate students see only a sliver of the full range of approaches available in hydrology, and they build no skill in communicating with other kinds of hydrologists. This limitation will eventually reduce their satisfaction and even their job prospects unless they can remedy the situation during their careers. We have to overcome this problem in our education programs, in the conduct of our societies, and in the work of agencies that utilize hydrologic science.

We must find vehicles for combining various approaches. One possible mechanism is to focus on the construction of unified theories in hydrology. This will not be accomplished by a small group working alone indoors. It requires the interaction of people with a variety of experience who are prepared to spend time combining their information and talents. If the hydrologic societies cannot effect this through the repeated organization of integrative symposia or special sessions, perhaps it is time to take the example of theoretical physics, which for decades has utilized the summer institute as a means of bringing together groups to work for periods of up to three months on particular problems. These institutes require a culture of sharing information that has not yet arisen in all fields of science, including our own. Hydrologic science may not be ready for such a large-scale commitment, but it may be time to experiment with a limited form of the concept.

The ability to construct a coherent theory-guided agenda seems to be a key to the convergence of approaches advocated by Dooge (1988). Diversity of approach has great advantages, but it is important that we at least understand where our colleagues who utilize different approaches are located on the intellectual map of hydrology. It is a useful exercise for all of us, and particularly for the training of students, if we can articulate and at least partially defend the approach of groups who may be working in a radically different manner or direction from our own. Such activity could be a form of consensus building to guide hydrologic science. It will not suit the personality of every hydrologic scientist, some of whom we can count on to ensure a diversity of viewpoints. But if we are ever to conduct truly scientific investigations of large-scale complex hydrological processes of the kind

described above, it will be necessary to marshal our investigations more thoroughly than is the tradition in hydrology.

Development or recapitulation of theory would be an important tool in agenda building for hydrologic science. The theoretical physics model referred to above suggests that it would increase and intensify collaborations and lead to proposals possessing a wide degree of reviewer support. That is the surest way to build the proposal pressure that would justify better funding levels. Strengthening of a coherent scientific agenda, rather than diffuse calls for more "scientific approaches," would attract students, sponsors, and clients. Such an agenda would include important research questions; plans for measurement programs, including critical tests of ideas; and proposals for linking findings about aspects of hydrology at different scales.

Communication

If scientists with different backgrounds are prepared to invest time in more unified approaches to hydrologic analysis, a means of communication will be needed to integrate them. The elements of communication needed for a robust hydrologic science must be provided in graduate programs of the science, whether they are coordinated in single departments or in federations of departments. However, the science cannot await only the training of a new generation of young people. We need to promulgate immediately certain kinds of communication and retooling among ourselves, using our professional societies, oversight bodies, and personal conduct. Thus, I refer here to the broader need for an improvement of communication, which would presumably guide the design of graduate programs.

First, the need for hydrologic scientists to stay sensitive to major socio-environmental questions requires that we take seriously the task of tracking and responding to the "web" of information, proposed earlier as a model for a hydrologic scientist's career. There is no substitute for broad and continuing education and flexibility of interest in nature and society. That point needs to be emphasized to graduate students, particularly Ph.D. candidates, who—contrary to many recent high-profile discussions of the future of graduate education—remain our best hope for the evolution of a creative hydrologic science contributing to human welfare. Although it is true that some mentors hire students as research assistants and treat them as drones and apprentices in the Dickensian way described in recent critiques of graduate education, it is not true that all Ph.D. students are obliged to focus narrowly on only their thesis topics, thus becoming "inflexible, illiterate, unemployable, etc." Nor is it true that they can only become informed about society's interests by taking social studies courses during their graduate careers. I encounter many students who entered graduate school with broad interests, an appetite for reading widely, learning languages, and other skills.

Through preparing for careers in unpredictable futures, they learn a much wider range of skills than students of 20 years ago: they practice expressing themselves well in a variety of settings, they learn to teach well (or they have no chance of a tenure-track academic career these days), and they change their interests in response to emerging opportunities. Temporary intense focus on their thesis topics illustrates one of their strengths. However, it does not mean that they cannot stay informed about other subjects and practice the skills referred to above while they are working on their theses. Tracking and responding to the web of information, as referred to above, require mentoring through seminars, conversation, and the other intangible aspects of a lively intellectual climate that are recognizable in excellent graduate programs or, for that matter, in other well-led scientific agencies. Some suggestions for individual mentors, though not for leaders of entire programs, have been summarized in another report of the National Academy of Sciences (1997).

Second, since it has been emphasized by COHS and other bodies that hydrologic science is a geoscience, we need to develop some general knowledge of Earth as a system. This requires that we expand our horizons to stay informed about the other geosciences and the processes that they study. We also need to develop some appreciation of real geographical features, including why there are regional and secular differences in hydrologically significant characteristics of rainfall, soils, and vegetation. One can hardly expect hydrologists to develop fundamental yet realistic theories of hydrologic behavior unless they appreciate the range and pattern of the hydrologically significant properties of the planet and especially its continental surfaces.

Third, we need to develop a more coherent vocabulary, grounded in the philosophical principles of science. Mathematics is obviously a requirement because of the need to formalize theories for exact definition, communication, testing, and prediction. It is widely agreed that this is so, and to judge from journal articles, some progress has been made in improving the average analytical skill level of people entering the field of hydrology. However, Dooge, Klemes, and others have warned repeatedly that mathematical training alone is not sufficient to build hydrologic theory. Something more fundamental is required. We need to remind ourselves and our students of the principles and methods of science, including its goal of developing general theories of nature, its search for fundamental mechanisms, and its empirical tools both for exploration and hypothesis testing.

Lack of numeracy is declining as a communications block in the geosciences. Even many students who are esthetically attracted to traditionally descriptive field sciences are quite willing to study advanced mathematics, continuum mechanics, chemical kinetics, and other useful hydrologic tools given sufficient reason to do so. These are not the students who studied

mathematics early in their careers because they were certain that it would be good for them. They still need convincing and motivating by concrete scientific applications before they will extend their mathematical skills. Helping students acquire these skills should be a goal of modern graduate education in hydrologic science. Hydrologic science is also recruiting students with strong analytical skills but little experience with landscapes, planets, processes, or measurement. It is difficult to expect students with no exposure to basic climatology, physical geography, or ecology to generate the idea of studying, say, the processes that lead to rainfall distributions over topography and their role in flood generation or how climatic changes might gradually affect plant community characteristics and hence the water balance. Thus, we also need to create some time in the education of these scientists to learn about the physics, chemistry, and biology of waters near the Earth's surface. New kinds of introductory graduate-level courses that present a quantitative and theoretical approach to some of these fields would probably attract both kinds of students described above and help them to recognize the overarching geoscience themes that proponents of hydrologic science have outlined.

At present, we do not communicate well enough to build hydrologic science into a broad, rapidly growing, socially useful activity. That problem can only be solved by behavioral changes forced by professors, hiring committees, employers, and editors. The crucial action eventually, however, will have to be curriculum reform. In a few universities that have the flexibility and funds to invest in new, sufficiently large Ph.D. programs in hydrologic science, it will be easier to hire and acculturate a diverse faculty covering the range of subjects referred to in the COHS report, provided that the leadership is open minded on this subject. At other universities a program in hydrologic science will have to be coordinated from offerings already in various departments of engineering, Earth science, atmospheric science, forestry, and related disciplines. The strains that Klemes (1986) has described between hydrologic research and technological applications of hydrology in resource management are likely to be difficult to manage under such circumstances, unless the Earth or atmospheric science departments are stronger, larger, and more committed to studies of land surface processes than is usually the case. In the case of departmental or interdepartmental graduate programs in hydrologic science, both National Aeronautics Space Administration (NASA) Earth System Science fellowships and especially the original National Science Foundation (NSF) fellowships in hydrologic science (five per institution for five years) have been a crucial form of support that allows graduate students to pursue an interdisciplinary course between faculty advisers without having to devote a large amount of time to assisting the research of the faculty member.

Improving Measurement Capabilities

Measurement and even qualitative observation have always been weak components in the training of hydrologists. Most hydrology books never mention the characteristics of real land surfaces or describe processes occurring on them over a range of time scales. It is tacitly acknowledged that, because of the scale problem referred to in earlier sections, it is difficult to visually identify any characteristic or process being represented in a hydrologic model. Data, supplied from networks of rain gages or stream gages by the technical staffs of federal agencies, are usually taken at face value, their limitations given only passing mention. Most hydrology textbooks have early sections on how to fill gaps in data series and generally "make do" with whatever fragmentary data happen to be available in the project area of interest. This passivity has left most of us unskilled in conceiving of innovative and precise measurement techniques. Exceptions to this generalization are the skillful measurements of small-scale subsurface water storage and flow processes in some field and laboratory experiments and monitoring studies.

Yet there have recently been some technological revolutions affecting the availability, or in some cases the promise, of more and better data than have been available before. Despite recent retrenchments in some federal government stream-gaging programs, during the last decade of this century we are probably receiving more hydrologically relevant data than have been collected in the entire history of the science, and the pace of measurement shows no sign of slowing. Digital topography of the United States and most of the Earth's continents at spatial resolutions of 30 or 90 m is already, or soon will be, available, and higher-resolution data will emerge, as side-looking airborne radar is deployed from satellites and aircraft. Laser altimeters on aircraft already produce high-resolution topography for special purposes. Distributions of atmospheric water vapor are mapped with passive microwave sensors on spacecraft, and higher-resolution spatial and temporal distributions of individual rainstorms are measured with ground-level radar (Smith et al., 1996a, b). Global satellite measurements of radiation and surface temperature are available for monthly averages, with the promise of much higher resolution following the launch of satellites under the Earth Observing System and other programs by NASA and National Oceanic and Atmospheric Administration (NOAA) (Asrar and Dozier, 1994). Other data bases include regular measurements of snow distribution and the condition of plant covers, as well as low-frequency compilations of ground-level or satellite measurements of plant distributions, land cover, and soil properties. In the wings are promises of optical monitoring of snow cover (Rosenthal and Dozier, 1996), radar measurements of snow water equivalent (Shi and Dozier, 1996), high-precision topography for low-lying areas such as valley floors, and even surface soil moisture for some restricted range of environments. The entry of NASA and NOAA into the field of hydrology has thus been a revolutionary force, facilitating analyses that were simply impossible earlier. In the United States, although there has been some

reduction in the number of monitoring stations, even traditional measurements of rainfall, streamflow, water chemistry, and soil properties are more easily available than ever because of a vast effort by some federal agencies to disseminate data through electronic media.

These data sources are not without blemish, of course. The resolution of routinely available digital topography is still too coarse to reflect the scale of the dynamics of runoff and erosion; radar rainfall is difficult to calibrate and interpret; measurements of plant conditions are compromised by atmospheric aerosols and other effects; and the litany of difficulties goes on. Many uncertainties remain in interpreting the ground-level radiation signal received at the top of the atmosphere. However, all of the examples represent major improvements in useful hydrologic data, especially in terms of spatial coverage. They are simply too promising and pervasive to be ignored in the training and retraining of hydrologic scientists. A significant time commitment to remote sensing and spatial data handling is now required in hydrologic training, including time spent critically reviewing the relationship between the interpreted product and actual ground conditions (which themselves may be difficult to define). Remotely sensed products allow us to observe large remote features such as entire continents, river basins, mountain ranges, and floodplains. Thus, they are crucial to our ability to observe large-scale processes such as floodplain inundation (Sippel et al., 1994; Vorosmarty et al., 1996; Mertes, 1997), rainfall fields (Smith, 1996b), and the generation of massive sediment pulses from regional-scale intense rainfall. They also contribute to the goal of constructing credible spatially distributed runoff and evaporation models.

Most of the new data sources referred to above are being delivered to hydrology without being ordered. In the traditional manner of "making do" with data from networks installed for purposes associated with water management, surface water hydrologists are muddling through, grateful for every new bit of data we can get our hands on. In particular, the satellite-based sensors were developed for other reasons and have been turned to purposes useful to us mainly by geophysicists, atmospheric scientists, and others. Some of these groups are now in a position to recommend satellite and other remote sensing programs to provide critical measurements for their scientific purposes. However, among hydrologic scientists, only hydrometeorologists seem in a position to make such requests, because of the previously mentioned lack of theoretical convergence and agenda building among hydrologists. The chance to guide hydrologic data collection could be an important product of agenda building.

A particular example of directed data collection that was highlighted by COHS was the opportunity to participate in large, coordinated, multiinvestigator field campaigns, such as the First ISLSCP (International Satellite Land Surface Climatology Program) Field Experiment (FIFE), Hydrologic-Atmospheric Pilot Experiments (HAPEX) and Boreal Ecosystem-Atmosphere

Study (BOREAS), and the upcoming Large-Scale Biosphere-Atmosphere in Amazonia (LBA) (Amazon Basin) and GEWEX (Global Energy and Water Cycle Experiment) Continental-Scale International Project (GCIP) (mainly Mississippi River basin) activities. So far, it is the hydrometeorologists who have been able to take advantage of these opportunities, and surface water hydrologists continue to lag behind in the effectiveness of their data requests.

Oversight

Though referred to above as a fledgling, hydrologic science is important to transcendent societal concerns such as the reciprocal interaction between humans and climate, global sustainability, environmental justice between nations and generations, and the influence of continental perturbations on the nearshore ("green-water") ocean. For this reason the science needs to be fostered as a strategic concern—perhaps as the special concern of some standing oversight body, analogous to the Climate Research Committee of the National Research Council (NRC), a committee overseen by the Board on Atmospheric Sciences and Climate. The most appropriate venue for such a Water Science Committee could be the NRC's Water Science and Technology Board (WSTB). The WSTB already works for hydrologic science, including its sponsorship of the original Committee on Opportunities in the Hydrologic Sciences and in fact has been the only such voice in this country in recent decades. However, most of the board's work is ultimately related to the technological issues that reflect the interests of the federal agencies that support the WSTB. Creating a Water Science Committee to nurture and develop hydrologic science would provide for a separation of emphases similar to that being proposed among hydrologists themselves.

It would be desirable for such a committee to continue fostering hydrologic science, more or less as described in the original COHS report but with more direct acknowledgment of the ethical responsibilities of environmental scientists to work on problems of broad social concern. The standing committee would be composed of a small, broadly informed group of hydrologists with a range of backgrounds who take seriously the responsibility for hydrologic science as a whole, rather than representing, say geomorphology, ground water hydrology, or hydrometeorology. They would have to see themselves as trustees, representing the interests of the next generation of hydrologists and all the nonhydrologists who pay the bills of the science.

The committee members could articulate trends or gaps in knowledge, thereby providing continuous advice to the directors of funding programs and building consensus about research strategies for the science. The committee could sponsor regional seminars and special sessions at professional meetings. It could publish occasional commentaries. Dissemination of

information about the contributions of hydrologic science to human knowledge and its plans for extending that knowledge would be another important activity. Several other sciences, most notably astronomy, have proven that frequent representation on the weekly science pages of the *New York Times*, combined with a strong community research agenda skillfully communicated to Congress, seem to be associated with the ability to mobilize massive investment in that research agenda. On the water planet, hydrologic science should be at least as diverting as the stars.

In its oversight of the science the committee could take some responsibility for maximizing the nation's entire investment in hydrologic research by promoting interactions between academe and the federal agencies interested in water. This interaction has declined precipitously during the past 10 to 15 years as federal budgets have tightened. However, communication depends on attitudes, even in the absence of money. A decline in optimism has also reduced the probability of new research collaborations and the transmission of ideas. During times of slow hiring, federal agencies become isolated from the stream of bright young people who continue to pass through universities. Academics lose contact with valued colleagues, underutilized data sources and equipment, and interesting scientific problems motivated by the agencies' responsibilities. This running down of the federal-academe relationship is a loss to the nation, and it needs to be reversed at little or no cost. Exchanging information and sharing joint responsibility for a national scientific committee with an optimistic charge could initiate this reversal. Visits and expressions of interest in each other's programs (research and nonresearch) would be easy steps in reestablishing a fruitful relationship.

Concerning how to fund such an activity, a straw proposal is presented to provoke thought among those who look wearily on any proposal for new activity at this time. A small and active (publishing several essay-length reports per year) standing NRC committee might cost \$150,000 per year. Six annual contributions of \$25,000 would suffice. These contributions could be sought among a variety of institutions that stand to gain from direct representation on the committee or from a continuous stream of ideas about opportunities for research that might be useful to its operation. The directors of the NSF and NASA hydrology programs might find that such information would reduce their need for outside advice from current sources, so that they might be able to divert some of the resources they currently use for that purpose. Some federal agencies with a need to keep informed about developments in scientific hydrology might also be induced to contribute. Since secondary goals of the committee would be to advertise widely the contributions of scientific hydrology and to stimulate agency-academe productivity, there are reasons for the committee to be useful to several agencies. Finally, the national organizations that represent private interests in water and power might also be induced to provide stable annual funding for a committee with a long-range strategic view of the science.

Summary

This author does not share the Sputnik-era confidence of some colleagues that hydrologic science left to its own devices would automatically improve human welfare. However, a modestly supervised hydrologic science, imbued with strong philosophical and ethical principles about the conduct of scientific research on behalf of society, could be of enormous benefit to the nation and to its collateral field of applied hydrology. Much good hydrologic science can already be found in the premier journals of hydrology, but it is spread so thinly amid excellent representations of other types of work that the ethos of hydrologic science does not emerge. As Klemes (1988) has pointed out, the science and the nonscience in hydrology frequently become mixed up, to the confusion of both.

Before a strong hydrologic science can grow and interact productively with the other geosciences, some actions and permanent behavioral changes are needed. We need to develop more unified approaches in our choice of important research targets and in our quest for theoretical generalizations about fundamental processes. We must put aside differences of background and have the patience to communicate with each other so that everyone understands the current state of knowledge, or at least knows exactly why he or she disagrees with it. We have to extend our ability to use or at least to understand a wide variety of new technologies that for the first time offer to measure the spatial characteristics of hydrologic processes and characteristics at scales up to regional and global. Finally, the oversight mentioned above needs to be provided by the NRC, which would not only act as an authoritative voice on scientific hydrology but also generate a stream of creative advice about continuing opportunities in hydrologic science.

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