Water Science and Application 2

Land Use and Watersheds

Human Influence on Hydrology and Geomorphology in Urban and Forest Areas

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Introduction to Section 2—Problems in Measuring and Modeling the Influence of Forest Management on Hydrologic and Geomorphic Processes

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Hydrologic and geomorphic changes resulting from alterations to the land surface and canopy cover in forest lands are subtler than those occurring in urban areas. Changes in infiltration capacity that alter the partitioning of runoff between overland and subsurface routes are smaller in managed forests than in urban areas, except locally on roads and following rare, intense burns in some biomes. Sedimentation rates and water quality degradation are not as intense. However, the changes in forests are more extensive and they diminish wildland resources that require prodigious amounts of space and high-quality water and the occasional disturbance of habitats (through fire and flood) in ways that are inconvenient for human inhabitants or resource managers. Thus, the influence of forest activities on runoff, erosion, and the form and biophysical functioning of landscapes have long been a source of contention between people who place a high value on wild living resources and the landowners and technical specialists most closely associated with timber harvest (from which, of course, most of us also profit).

For most people and their opinion leaders, forested mountain lands have been out of sight and out of mind for decades—the realm of timber managers—until recent debates about endangered species, water quality, and flood hazards to communities that are spreading into forested mountains have ignited a broader interest. Alteration of forest cover through inadvertent land-use experiments has highlighted the various functions of a forest cover (canopy interception, evapotranspiration, root strength), the various influences of timber harvest (road construction), and how they interact with topography, rainfall, and snowmelt to control the magnitude and tempo of water and sediment delivery to channels. Fewer studies of the influences of soil type on watershed response to timber management have been made so far. However, relevant studies of the disturbance at landscape scale have suffered from a variety of constraints because the per-unit-area value of these wildlands is low compared to the high-value, intensively engineered, urban lands, where concerns about safety and sanitation established, early in the 20th century, the social expectation of intensive investment in site characterization and long-term monitoring. We have not yet established the social value of these operations in forested lands convincingly enough to garner support for thorough, long-term empirical studies.

Monitoring programs in most forest lands (with a few exceptions in carefully managed, uncharacteristic, experimental watersheds) are typically too brief to sample the variability of natural and disturbed hydrologic regimes, and thus have a high probability of missing critical events such as large floods, landslides and debris flows. Sparse monitoring rarely captures the intersection of large rainstorms with the transient effects of timber harvest, such as the removal of canopy or root structure, or the installation or grading of road fills and drainage systems. Records are gradually lengthening at a few sites, as represented by the paper in this volume by Lewis et al, and the regional-scale statistical survey of floods by Jones and Grant [1996], but providing purely empirical resolution to questions such as whether timber harvest increases the magnitude of large floods or the risk of landsliding will always suffer from the difficulties of small sample size in the critical range of large events.

Constraints on the number and size of monitored sites, and the need to make collateral measurements in order to understand the occurrence of destructive processes, also result in most measurement programs being too local to obtain accurate spatial averaging of, say, storm rainfall, snow accumulation or melt rates, landsliding, or other erosion processes. Thus, it is difficult to use monitored watersheds to estimate the probability density functions of various processes that trigger floods and that load materials (sediment, nutrients,

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woody debris) into channels and re-shape their ecosystems. It would be difficult, for example, for an engineer with a design problem or a water-quality target to use monitored watersheds in the Pacific Northwest to estimate probable sediment yields from wild and managed forest watersheds.

The papers in this section begin to redress some of these difficulties. Most of them utilize explicit models of processes to formulate hypotheses that can be tested against the sparse empirical record, and they take advantage of the largest source of new hydrologic data in the history of the science: digital spatial databases of topography and, to a lesser extent, soil properties. Despite the progress reported herein, however, the message that resonates throughout the papers is the constraint of data quality and availability, and thus the need for investment in field data collection programs to define the fluctuating condition of forested wildlands in various regions of the United States. Indeed, the condition in which we now find ourselves (as agencies facing lawsuits and other instruments of policy debate, or as individual scientists being asked questions about issues of rising public concern with inadequate data in our hands) indicates that society would probably have been better served by investing more funds and effort in measuring the properties of wildlands and monitoring their changes in the intervening years. We would now be in a better position to make decisions about acceptable ranges and even required changes of habitat condition or about water quality variability, ecosystem restoration, and sustainable development. It would be wise to avoid continuance of the same mistake, and hydrologists can help by articulating the need and designing the solution more effectively than has been our record to date.

The papers collected here, which are among the best examples published in the field, testify to the continuing need for well-supported programs of field data collection and field process studies. Even the carefully designed and relatively long statistical study reported by Lewis et al. highlights the brevity of rainfall and runoff records and the complexity of interactions that affect peak storm flows and suspended sediment concentrations in very small logged watersheds. Although Lewis et al. demonstrate the extraordinary care that must be taken to control for various effects such as rainstorm size, time since disturbance, and antecedent wetness, the presence of many transient effects such as sediment storage and varying sediment sources still make the data set difficult to interpret and to extrapolate regionally. And this is for a relatively straightforward experiment with a simple road network and harvest practices, no devastating flood in the short record, and single ownership and timing of harvest! Dietrich et al illustrate how innovations in modeling the spatial distribution of landsliding are limited by the coarse nature of most digital topographic products in forested mountains. Modeling the timing of landsliding through timber cycles (as in the paper by Sidle and Wu), even for conceptual and planning purposes, requires information that is rarely available. The data-quality issue results from the lack of interest in data gathering by some resource management agencies, and the lack of inter-agency cooperation in field studies and data gathering (on, for example, streams that cross agency land boundaries or watersheds that don't coincide with a single agency's land). The lack of high-quality data is sustained by a tradition among hydrologists of utilizing whatever fragmentary records we can get from hydrometric networks that were not originally designed for scientific purposes.

Hydrologists have been so grateful for years for the scraps of data that we obtain from measuring stations, mostly located not for the purpose of understanding river flow or sediment sources, but for an agency's need to monitor contaminant loads or water quality standards, or a power or irrigation company's need to define its resource, or a local government's desire to keep an eye on their resources to deflect criticism or law suits. If we are lucky, there is a rain gauge in the vicinity of the monitored watershed, and we can locate those data in another agency. We have not invested time, as a professional community, in the design of (and garnering of support for) hydrometric networks that would be useful for testing theories, examining whether transferable watershed-scale parameterization is possible, etc. We have not aggressively investigated the possibilities of technology for efficient data collection - e.g. rapid installation and calibration of flow recorders at many locations in a region; sediment samplers calibrated to total suspended load; and remote sensing of basin-wide channel morphology, wood loading, or water temperature. Various remote sensing initiatives promise to assist forest hydrology through the enhancement of hydrometeorological databases. For example, the need to calibrate spaceborne radiometers has motivated support for well-endowed surface radiometer sites [Augustine et al., 2000]. A vital challenge for hydrologists desiring to improve rainfall-runoff predictions is to combine ground networks of rain gauges (after appropriate densification) with radar to define spatial and temporal fields of rainfall, which is, after all, the largest component of the hydrologic balance equation outside of the snow zone. Hydrologists need to participate in exploring and improving calibrations under a range of raindrop size and topographic ruggedness, exploiting data handling systems for rapid data processing, and examining the hydrologic implications of improved knowledge of precipitation.

Limitations of data add significance to cooperative efforts in sharing of old records within and between agencies and the academic community. Fortunately, US Geological Survey streamflow data (though not raw data such as sediment concentrations, gauging station morphology, channel hydraulics, or discharge rating curves) have long been routinely available, and the USGS has greatly improved access to flow records and digital topography using the worldwide web. The academic community, through the NSF-sponsored Long Term Ecological Research network, is also promoting the sharing of hydrologic data along with experience and insights gained about model applications. The USGS has also promoted the development and dissemination of models to assimilate data and transfer it to unmonitored sites [Leavesley et al., 2000].

It is generally acknowledged that mathematical models must play an ever-greater role in answering increasingly detailed questions about hydrologic mechanisms and conditions. It is impossible to collect and present sufficient measurements of processes over large areas and multiple years, or to sample and understand the many interactions between processes and the factors that control them, without resorting to complex sequences of mathematical expressions, and computing the implications of the limited range of conditions or the fragments of process understanding that we are able to capture empirically. However, models immediately raise the need for high-quality input and other data for testing. These data have to be obtained quickly and at low cost.

During the past two decades, of course, the most important increase in data availability has arisen through digital representations of watershed condition, especially in topography and cover, and the capacity for updating and refining such representations. In particular, the availability of digital topography, first through digitization of aerial photographs, and more recently from side-looking airborne radar and laser altimetry, has expanded the application of spatially explicit, topographically driven, environmental models in mountain watersheds. Important innovations have occurred in: conceptual and mechanistic runoff models [e.g. Beven and Kirkby, 1979; O'Loughlin, 1986; Moore et al., 1988]; hydroecological models [e.g. Band et al., 1991]; erosion and landform predictions [e.g. Dietrich et al., 1993]; and snow pack energy balance models [Marks and Dozier, 1992].

The papers in this section continue the developments referred to above, and focus on management and policy issues that have become particularly contentious in the Pacific Northwest of the United States, and are emerging in other timber regions. Despite increasing regulation of timber harvest in the past three decades (involving rates of cutting, method of logging, constraints on distributing various activities, and requirements for best management practices), society's sensitivities to the effects of timber harvest have simultaneously escalated. There is greater concern for aquatic ecology, the condition of streams, and for the preservation of biodiversity, particularly for certain charismatic species such as salmon and other endangered species. Details of questions have increased to levels such as: "What is the role of small to medium-sized floods in disturbing channel beds?" or "What is the effect of timber harvest on stream turbidity?" The issues are also intensified because of population moving into areas formerly reserved for logging, thereby intensifying concern about whether rainfall or rain-on-snowmelt floods are increased by timber harvest. Although qualitative ideas exist about the sign of the resulting changes, hydrology has not had the capacity for sufficiently quantitative predictions that can be used for defensible analyses of risk and of cumulative effects of multiple changes at the watershed scale.

Several of the papers establish that spatially explicit models, incorporating new spatial data, modern computing power, and insights from process studies from the 1960s-1980s, can capture relevant hydrologic processes at a resolution and a geographic scale sufficient for policy making and forest engineering plans, as long as data of high quality are available. The papers by Wigmosta and Perkins and by Bowling and Lettenmaier both illustrate how the Distributed Soil-Hydrology-Vegetation Model (DSHVM), a DEM-grid-cell-based model of hydrologic processes characteristic of maritime, mountainous environments, can be augmented to examine the role of road networks in intercepting subsurface flow and routing it quickly to a watershed outlet. The model was originally formulated by Wigmosta et al. [1994]. Bowling and Lettenmaier also examine the synergies between enhancing the capacity of the watershed to evacuate runoff in this way and increasing the volume of runoff due to the removal of canopy interception and evaporation. However, despite the availability of several spatial databases for these intensively studied basins of high commercial value, it was still necessary to augment the characterization of the road network through GPS surveys, including the localization of about 20 culverts per km2 of watershed in order to capture some runoff processes at the scale at which they express themselves.

Both modeling efforts pay attention to using observable processes and measurable quantities to constrain parameter values as far as possible. They verify predictions wherever possible (as in the case of Bowling and Lettenmaier's observations of the altitudinal limits of snow coverage.) However, it was still necessary to calibrate the volumes and timing of runoff by adjusting the transmissivity of the soil, and the geometry of the subsurface flow and water-holding fields. The need for calibration on each watershed raises the question of whether transmissivity (and perhaps other parameter values) can be regionalized and correlated with some identifiable controlling factor. It would be reassuring to know whether a set of catchments in the same geologic-physiographic region calibrate with the same transmissivity, or whether the representation of material properties by that parameter is so occluded with geometric effects, with typical rainstorm characteristics, or with recent moisture conditions that it is essentially not transferable from one basin or period to another. Effects of the calibration period may be particularly critical in the case of non-equilibrium forests.

The need for better definition of soil and root-zone dimensions and of precipitation fields in complex terrain arises again in the modeling of landslides later in the volume. However, testing of the landslide predictions is facilitated by spatially explicit observations of the absence, presence and timing of the critical events. Rainfall-runoff predictions are verified against point measurements of streamflow, and cannot be tested to any significant degree against spatial data. In fact, monitoring of runoff at a resolution needed to test hypotheses about mechanisms [Moore and Thompson, 1996; Anderson et al., 1997; Hutchinson and Moore, 2000] and attempts to measure hydraulic conductivity independently of the "watershed permeameter" [Davis et al., 1999] indicate just how much spatial averaging and conceptual revision are required in our application of familiar constitutive equations at the watershed scale.

Although it is difficult to imagine verifying or falsifying a complex model such as DHSVM, which includes such innovations as the use of a predicted two-dimensional wind field over complex terrain, snow accumulation and metamorphism, canopy processes affecting snow and liquid water, a multi-layer root zone, as well as overland, subsurface, and channelized runoff, the model provides a vessel for our communal understanding of the hydrologic processes that are believed to be important in maritime mountains. It also constitutes a way of systematically analyzing the various interactions that can only be sampled in a very sparse manner by even enhanced field monitoring programs. Such a model provides a template for field studies to investigate various components of the hydrologic cycle and to compute the significance of their interactions. The limiting resource appears to be the lack of a tradition in hydrology for organizing large-scale, long-running tests of modeling capability in well-instrumented field sites or regions. Such tests would require funds for watersheds instrumented to test hydrologic hypotheses and decisions about whether to concentrate process studies in a relatively few, heavily studied catchments to capture the expected synergies, or to distribute the field sites widely in order to expand the range of processes, environmental conditions, and parameter values. Another use to which models of this type could be put involves forensic studies of extraordinarily large and damaging floods for which adequate data, especially on rainfall or snowmelt, are available. This would avoid the problem of extrapolating parameter values derived from small floods to large ones. It might also make use of the rare, adequate data sets (e.g. on canopy interception processes) from the few intensively instrumented watersheds that have sampled very large events. Increasing transfer of information from experimental watersheds by means of spatially explicit modeling remains to be done. Given society's recent focus on pollution and water quality rather than floods, there would be greater support for such forensic studies if the flood triggers major pollution events, such as tailings dam failures or nitrate pollution of rivers. Another opportunity for well-instrumented studies of rainfall and runoff is presented by each large conflagration in western forests, but the academic hydrology community and the land management agencies have not yet coordinated their activities and resources to take advantage of such opportunities.

Hydrologic interest in forest roads also extends to their effects on sediment delivery to streams. Luce and Black have developed an extraordinary data set, monitoring sediment loss from 74 road segments that sample a range of gradient, plot length, cutslope height, and soil texture. The problem of modeling to assimilate and interpret the results initially seems simpler than watershed-scale erosion prediction, but the strong sensitivity of sediment production to small local differences in environmental characteristics and recent history of a road segment constrains process modeling. The results are satisfying in this sense: they can be rationalized in terms of qualitative conceptual models of field observations, and provide useful calibrations of the effects of time on sediment availability. On the other hand, transferable predictions are severely limited by sensitivity to transient effects such as sediment availability as conditioned by the recent history of grading and ditch clearing. Surprisingly, traffic intensity did not emerge as a factor controlling sediment availability.

Papers by Dietrich et al. and Sidle and Wu continue the theme of sediment production. Taken in combination, these papers provide a review of current methods for prediction of landslide sources, particularly of locations vulnerable to timber harvest effects. Sidle and Wu begin with a review of terrain mapping with multi-factor overlays, which is still the best way of integrating the effects of material properties such as strength, stratigraphy, anisotropy, heterogeneity, and of taking into account historical records and the experience of the delineator, even if the technique is not very useful for delineating specific landslide sites or their timing. Both papers explore the leverage to be gained from application of theories of subsurface flow and slope stability to predicting the temporal and spatial occurrence of landslides using digital topographic data.

Both spatially distributed models combine the infinite-slope stability model with a subsurface flow model for steady-state conditions (Dietrich et al.) or finite-duration storms (Sidle and Wu). Iverson [2000] has recently presented a subsurface flow theory that has the capacity for representing the different timescales of transient pore-pressure response involved in both short, intense rainstorms that induce rapid, vertical percolation in permeable soils and the slower, downslope percolation responsible for the drainage-area effect on shallow landslides and for triggering the instability of deep landslides in low-permeability soils long after rainfall. Coupled with digital elevation models, this theory will allow an extension of current topographically driven models of landsliding, --- at the expense, once again, of the need for transferable parameterization based on field studies since the model is also based on the Richards equation. All of these methods can take advantage of automated procedures for processing digital topography in hydrologic calculations, and of removing some operator variance in recognizing sites with topography that could promote failure. They can also efficiently transfer results to unsurveyed basins, using thresholds from a few basins in which detailed landslide mapping has occurred soon after a large rainstorm or in which there has been a detailed historical audit of landsliding throughout the air-photo history.

The Sidle and Wu model adds the transient effect of root decay and recovery resulting from timber harvest, fire and disease and the stochastic occurrence of rainstorms to calculate the temporal and spatial distribution of landsliding, and thereby to map failure probabilities across a disturbed landscape. However, our general ignorance of soil depths, especially in hollows, propagates uncertainties through these predictions of landslide occurrence, and can only be compensated by intensive site investigation [Dengler et al., 1987], the assumption of a probability distribution of depths [Ward et al., 1982], or stochastic simulations of very long series of events that includes the evacuation and filling of hollows with mobile colluvium [Benda and Dunne, 1998; Benda et al., 1998].

Not surprisingly, both the Sidle and Wu and Dietrich et al. modeling efforts indicate that leaving trees in hollows reduces the risk of landsliding, at least over the multi-decadal time scale. Root cohesion is particularly difficult to represent in such stability models, since the mechanics are not well understood and the effect is time-dependent immediately after harvest or fire through at least a few decades of succession, during the period of species replacement and changing stem density. However, root reinforcement will probably become the focus of intense investigation now that there is a consensus about the effects of decreasing root strength on slope stability after years of the debate being diverted towards the (now discounted) destabilizing effects of removing the normal stress due to tree weight. The timber industry will probably now encourage investigations of the role of the understory, and the possibility of designing root systems through manipulation of forest-age structure during harvest cycles [Sidle, 1992]. However, as it is presently formulated in slope stability models, a single threshold value of "soil strength" may not be measurable at the relevant scale. It exhibits strong local variations, from the distance between tree perimeters to the half-widths of hollows, and between deep soils and shallow soils. Yet, if one tries to back-calculate "strength" from the discrimination between failure sites and stable sites, a large range of combinations of relevant controls (assumed values of pore pressure, friction angles, cohesion, and failure geometry) can interact to affect the derived values, even in an apparently homogeneous landscape. The Dietrich et al. paper, for example, demonstrates the difficulty of calibrating a spatial model of landslide occurrence, in which small uncertainties in parameter values (probably in the range of unknowability for most extensive applications) cause several- fold differences in the predicted spatial density of failures. The Sidle and Wu model (dSLAM) concentrates our thinking and formal analysis on the multiplicity of interacting factors affecting landslide occurrence, but the parameter estimation problem is daunting and measurement programs are in their infancy.

Rather than attempting a full prediction of landslide occurrence under transient conditions, Dietrich et al. concentrate on illustrating and (most importantly) evaluating the capability of their model, SHALSTAB, for recognizing the topographic attributes that localize shallow landsliding and debris-flow generation. The authors discuss the significance of ignoring other attributes (e.g. soil depth, root strength, and subsurface conductivity) that are essentially unknowable at present, at least without intensive site investigation. Once all of these quantities are fixed, the model can be run to test the hypothesis that the topographic attributes represented in the model (area drained per unit of contour width and local hillslope gradient) are good predictors of where in the landscape landslides will be concentrated. Considerable attention is paid to the role of topographic resolution in DEMs, and the limits of accuracy in mapping landslide sites.

Dietrich et al. suggest that their model predictions be interpreted as qualitative rankings of risk, and they illustrate why in the application of such a model for decision-making there is no substitute for intelligent interaction between model and interpreter. It seems unlikely that there will ever be (the dream of managers and regulators) a prediction tool that can be used routinely, objectively, and reproducibly by people with little or no hands-on experience of the phenomenon being modeled. This has long been the assumption underpinning the use of handbooks of engineering practice, and their modern incarnations in planning models, which are downloadable from the worldwide web and can be used as a cookbook, for example, to estimate Total Maximum Daily Loads of pollutants such as sediment. Instead, the accuracy of predictions is likely to depend on judgment and skill. Simply having applied the model many times does not constitute "experience". Moreover, effective use of the predictions will depend on value-based decision-making (the precautionary principle, maximum utility, or alternatives). Dietrich et al. provide valuable examples of applications of their model for: site-specific prescriptions by landowners; regional planning by government agencies; and hazard mapping. The model's capability lies in indicating that portion of the landscape in which most landslides are likely to occur. Site-specific conditions that dictate when landsliding might occur depend on knowledge of management and storm history.

What makes models difficult for managers and policy makers to comprehend and use is the concept that models are approximations, limited to certain types of representation by the resolution at which they are designed and supplied with input data. Once a model prediction is made, it is viewed by a scientist as a hypothesis to be checked by measurements (monitoring), and probably refuted or refined. Managers usually need an answer, with some urgency, even if it is obtained through methods that do not represent processes or conditions found on the watershed. Instead, they need an objective calculation device, relatively free from operator variance, from which all users (including contentious stakeholders or technical analysts hired by litigating opponents) will obtain more or less the same answer. The paper by Jackson et al. in Section 1 of this volume reports a modeling and decision-making approach for the urban environment in which all model users obtain essentially the same answer. Once the calculation (or a consequent decision) is made, it tends to be embraced and defended to the death as the basis for a decision that had to be made in uncertain times. Often, the agency embracing the prediction does no monitoring, in case the prediction proves to be inaccurate or even incorrect. These difficulties in using modeling and prediction are antithetical to scientific hydrology, but they limit support for improvements in model construction and in measurement systems for testing models under field conditions where there is a chance of reducing uncertainty or discovering new hydrologic principles.

The papers collected in this section provide grounds for optimism and challenge. Progress is being made in the application of hydrologic and geomorphic theory and in the use of an extensive new data source: digital topography. However, the progress demands improvements in other parts of our knowledge, including the geometry and material properties of various environmental features, and the best ways to think about and use these modeling procedures in our contributions to environmental problem solving.

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