

Cross-Scale Modeling of Riparian Ecosystem Responses to Hydrologic Management

Carl Walters^{1*} and Josh Korman²

¹Fisheries Centre, University of British Columbia, Vancouver, British Columbia V6T 1Z4; and ²Ecometric Research Inc., 3560 W. 22nd Avenue, Vancouver, British Columbia V6S 1J3, Canada

ABSTRACT

There is much demand for quantitative models to aid in comparison of policy options and design of adaptive management policies for riparian ecosystems. Such models must represent a wide variety of physical and biological factors that can vary on space–time scales from meters–seconds to basin–decades. It is not possible in practice to develop a complete model for all variation. Incomplete but still useful models can be developed by using state variable identification methods that focus scientific attention on causal pathways of most direct policy concern, and by using various analytical methods to provide cross-scale analytical predictions about effects of microscale variation. The main value of such

models has not been to provide detailed quantitative prescriptions, but to help identify robust, qualitative arguments about efficacy of various policy choices. However, they have not been successful at representing some important dynamic effects in riparian systems, where small physical changes (such as overtopping dikes) and infrequent extreme physical events can cause habitat changes at large spatial scales and ecological impacts that last for decadal or even longer time scales.

Key words: riparian; ecosystem modeling; multi-scale; hydrologic management; fish; trophic structure.

INTRODUCTION

Most major riparian ecosystems in North America have been impacted by water management aimed at power production, flood protection, and diversions for consumptive uses. There is broad consensus among ecologists that these impacts have been largely negative in terms of natural ecosystem structure and function (Poff and others 1997), and there appears to be growing public willingness to consider (and pay for) quite drastic options like removing dams from the Columbia River, “deplumbing” the Florida Everglades, or removing Glen Canyon Dam from the Colorado River. Evaluation of such options and comparison of them to less costly mitigative measures (like partial restoration of seasonality in flows) has involved the formation of large, interdis-

ciplinary research teams with mandates to provide predictions about a wide range of biophysical and economic impacts, that is, with a very strong mandate or demand to adopt “ecosystem approaches” to policy analysis. Attempts to integrate the very complex activities and recommendations of such teams generally have involved formal mathematical modeling of some sort, and model development has been plagued by a variety of institutional and technical problems (Walters 1997). There is growing interest in treating ecosystem restoration as an actively adaptive, experimental process (for example, experimental flooding in the Grand Canyon; Collier and others 1997), and modeling is particularly critical in the design of adaptive policies as a means to identify key uncertainties and to screen experimental alternatives.

As demand has grown for modeling as a tool for synthesis and adaptive policy design, administrators

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*Corresponding author. e-mail: walters@fisheries.com

and managers have been treated to a bewildering variety of claims from scientists about what modeling can and cannot do, and about how to go about it. These claims range from absurd optimism about our ability to identify and represent “all the relevant factors” to complete denial that any useful predictions can be made in the face of ecological complexity. Seldom are people charged with development of research priorities (or allocation of funding for modeling activities vs field research) treated to anything like an objective, scientific ordering of modeling options in terms of costs, relative predictive ability, and possible pitfalls.

Much of the debate about how to approach modeling of riparian ecosystem processes arises from grossly different disciplinary perceptions about how to deal with dynamics at multiple space–time scales. Hydrologists, chemists, and geomorphologists most often pretend good “mechanistic” understanding of fine-scale (centimeters to meters, seconds to hours) processes like sediment erosion-deposition and reaction kinetics and consider it largely a matter of simple (but perhaps computationally costly) summation or integration to see how such processes affect larger-scale state measures, such as turbidity or channel morphology; furthermore, they expect such integration to produce damping of effects across scales (effects of any local change disappear smoothly over time and space). They further point to the need for detailed local modeling to represent effects of fine-scale policy actions, such as strong diurnal fluctuation in releases of water from hydroelectric dams to meet corresponding fluctuations in power demand. In contrast, ecologists more often are concerned with responses at much larger-longer scales, from weeks to months and km² for animal recruitment processes up to decades for basin-scale development and succession of riparian vegetation. They may admit the importance of some fine-scale physical and chemical “events” (such as short floods that can scour riparian vegetation from shorelines and reshape floodplain habitats) to the larger-scale biological dynamics, but would not expect simple damping of effects across scales in view of “active” biological feedback processes like animal dispersal and population “outbreaks” that can generate highly nonlinear propagation of ecological responses to large scales. This description of disciplinary approaches is, of course, a generalization; we have encountered many instances in our riparian modeling efforts where ecologists focus on very fine spatial and temporal scales or particular “pet processes.”

This article reviews concepts and modeling methods that may help to reduce misunderstanding

among scientific disciplines about how to represent widely divergent space–time scales in riparian ecosystem analysis. Most of the following commentary is derived from considerable experience (and debate) fostered through adaptive environmental assessment and management (AEAM; Holling 1978; Walters 1986) processes aimed at producing quantitative predictive models for riparian policy analysis on major ecosystems ranging from the Florida Everglades (Walters and others 1992) to the Colorado River in the Grand Canyon. The AEAM process has been used with variable success since the early 1970s (Walters 1997) to bring together disciplinary scientists and resource managers in workshop settings where shared interest in model development is intended to foster careful problem definition, clear communication, and precise definition of cross-disciplinary information needs. The main lessons we have learned from this experience are to (a) discount claims about the possibility of modeling everything that might be important; (b) define the mode structure and variables in reference to policy issues rather than scientific interests and disciplinary traditions; and (c) use various techniques and approximations to reduce computational complexity and thereby encourage use of models as games to sharpen intuition and stimulate imagination rather than provide precise quantitative predictions.

It is important to recognize that there is a gradient both in modeling styles and objectives. On the one extreme, there are very fine-grained, high-resolution models focused on providing very precise predictions for a small set of indicators. Such models, which can be labeled “reductionist models,” are generally useful for research purposes, and one of the justifications for modeling in fine detail is to help identify key microscale processes and events that may have larger-scale consequences. On the other extreme, there are the coarse-grained, low-resolution models that provide relatively imprecise predictions for a large suite of indicators (for example, various components of an ecosystem) over management-relevant time and space scales. Our riparian modeling experience has had a strong management focus, pushing us to develop and evaluate models on the coarse-grained end of the model complexity/resolution axis. The dilemma that we have encountered is that there is a strong demand for management-focused models that also are fine grained in detail and resolution. In this article, we describe various techniques, and their pitfalls, that try and capture important fine-scale dynamics in simpler ways so they can be applied in management-oriented models. We also describe some of the difficulties and pitfalls in trying to apply fine-

grained models in these situations. Our discussion is not intended to discredit fine-grained modeling approaches, but to describe their limitations in the context of applied management situations, and suggest alternative ways of capturing important fine-grained dynamics in coarse-grained management models.

FRACTAL DYNAMIC STRUCTURE AND THE IMPOSSIBILITY OF CONSTRUCTING “COMPLETE” ECOSYSTEM MODELS

In most areas of applied ecological modeling like fisheries stock assessment, we are concerned with quite limited policy options and management performance variables, at particular space–time scales. In such cases, debates about model complexity and completeness can be resolved by applying objective criteria and tests to proposals for including (or discarding) extra detail and/or breadth of factors: are the proposed inclusions likely to improve predictions at policy-relevant scales enough to justify costs of obtaining whatever extra information is needed? Such heuristic tests are difficult to identify in riparian ecosystem management settings, where policy interventions can occur over many scales (from hourly water release schedules to decadal plans for flood plain zoning) and where policy interests range from very local (for example, particular shoreline and backwater habitats) to basin-scale. Virtually any modeling effort, from detailed tracking of movement of individual sand grains (or dispersing fish) through to whole-system mass-energy budgets, can be justified as contributing directly to “useful” policy prediction.

The wide range of policy concerns in riparian settings usually leaves us with three strategic choices for investment in policy modeling: (a) reductionist: attempt to develop a single model that describes all dynamics at the finest possible scale, and predicts larger-scale responses as emergent properties; (b) selective: attempt to model dynamics at all scales simultaneously, but admit that the representation will be incomplete and will contain many “judgment calls” about what details and variables to exclude from the analysis; and (c) fragmentary: develop a set of independent “mini-models” of particular dynamic relationships and processes that affect selected policy variables that are of special concern, with linkage among these models limited to insights that each might provide about appropriate forms of relationships or parameter values to use in representation at other scales (for example, a model for tracking movement of individual sand

particles might give insights about statistical properties of large-scale sand deposition and erosion rates).

There are at least three reasons that make it difficult to apply the reductionist approach as a practical option: (a) fractal structure of the physical habitat template for riparian ecological processes; (b) need for extensive model testing and policy gaming, which are impossible when the model is computationally complex; and (c) costs of information gathering to establish microscale state and relationships. The second and third of these reasons are practical consequences of the first.

Fractal structure of habitat templates means that habitat structure does not become less complex as we look more closely, yet organisms use pattern and structure at all scales to minimize energetic costs, reduce predation risk, and maximize foraging/growth and reproductive opportunities. A simple way to understand this issue is to imagine taking a series of snapshot photos of a riparian ecosystem from different perspectives, with each photo having a grain or pixel size that we might use as the “finest” or most complete model state description. A photo covering the whole watershed obviously would show strong downstream pattern in variables like water clarity, velocity, animal community structure, and shoreline vegetation. A photo of a single 1-km “reach” at midstream also would show strong pattern, but now with major physical and biological differences related to riffle-pool-backwater structure. A photo of a single 1-m² plot within a riffle in this reach would again show just as much pattern, but now between patches representing exposed rock surfaces, crevices, and the protected undersides of rocks. Even finer photos might show ecologically meaningful patterns for at least some organisms, but the basic point is obvious: no photo or reductionist scale choice for modeling is “complete” or correct: with respect to the finest scale represented explicitly in computations, finer-scale structure has to be either ignored (for example, treat finer scales as uniform, homogeneous), or described through statistical properties (for example, proportion of benthic insects that are on rock surfaces and hence exposed to fish predation).

Likewise, physical habitat responses and trophic interactions can be highly structured in time, for example, insect emergence and fish feeding/predation interactions are often concentrated in brief dawn/dusk “bouts” of activity. The importance of micro time scale interactions becomes particularly obvious in settings where human disturbances and manipulations have very “fast” components, such as diurnal variation in recreational fishing activity or water releases from reservoirs.

An important implication of fractal structure for comparing alternative modeling approaches is that it is meaningless to invoke distinctions like “mechanistic versus statistical” or “black box versus white box.” Every model has limited resolution (finest scale, grain) and hence must contain some essentially statistical description of structure and event dynamics at finer scales than those represented explicitly. In short, every model is constructed as a collection of nasty black boxes and statistical descriptions, no matter how the modeler may try to disguise these descriptions by explaining them in terms of physical and biological mechanisms. This means we should never judge models by how detailed (and hence “realistic”) they appear to be; rather, we should judge them by how clearly and credibly they represent statistical properties of the cross-scale linkage between the finest patterns/processes represented explicitly and the very nonrandom patterns of even finer-scale dynamics.

Put another way, one of the most dangerous pitfalls in ecological modeling is to choose some maximum space–time resolution based on practical considerations (finest map data, computational resources), then to base descriptions of phenomena at even finer resolutions on assumptions of homogeneity or randomness within each explicit model pixel. This approach may be simply wasteful if policy concerns are only about larger scales (there may be far simpler and statistically defensible ways to make the larger-scale predictions than by adding up effects over every component pixel) but worse may lead to grossly incorrect descriptions of actual statistical patterns. As a simple example, consider the problem of predicting recreational fishing effort in some river reach or site (Figure 1). If we base this prediction on arguments about behavior of a “typical” angler (homogeneity assumption), we likely will conclude that effort should be zero unless fish density is high enough to attract the angler from alternative sites or activities, and should be high if the site is the most attractive option available (Figure 1, top). If instead we base the prediction on recognition that there is a heterogeneous population of potential anglers, so the effort response will consist of a statistical summation or integration of individual attraction events, we will predict a very different response pattern that can be interpreted precisely as a cumulative probability distribution rather than a behavioral reaction pattern (Figure 1, bottom). Obviously the two functional models in Figure 1 have very different implications for time dynamics of fishing mortality in relation to changes in the fish population. These same arguments apply to physical as well as biological processes; for ex-

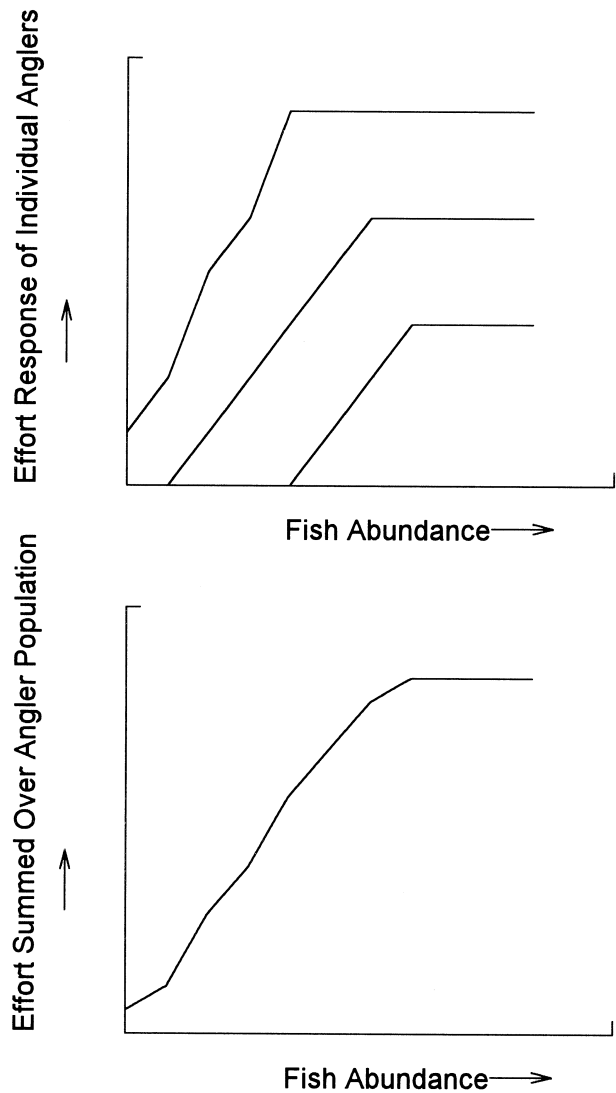


Figure 1. Differences between the response of individual anglers to changes in fish abundance (top) and the aggregate response of all anglers (bottom).

ample, the microscale physics of sediment movement in relation to local velocity/energy field may be well understood, but equations based on local physical “laws” may not describe statistical patterns of large-scale sediment transport.

DEFINING WORKABLE STATE VARIABLE SETS

Dynamic models have two parts: a set of state variables representing what a system is like at any instant, and a set of rules for how these variables change over time as functions of forcing natural inputs, policy choices, and past dynamic states. A central argument in the previous section is that cross-scale issues should not, and indeed cannot, be

resolved simply by expanding the state variable set and expressing rules of change for shorter time steps. Instead we always need to make a series of judgment calls about breadth of variables, detail, and how to incorporate the effects of invisibly fine dynamics in the rules of change. The ecological modeling literature unfortunately does not provide much guidance about how to make these judgments; instead, it offers mainly simplistic arguments about “top-down” versus “bottom-up” approaches (start simple and elaborate as necessary or start at greatest practical detail and aggregate as appropriate). Such arguments miss a very basic point: from whatever direction the variable selection process is approached, this process is necessarily an adaptive one. That is, we never just “build a model;” instead, we develop a whole series of them, with more or less explicit thinking at each step about whether to articulate further detail or discard parts of the formulation. Experience and literature seldom provide a complete road map for these steps; instead, modeling discussions and thinking usually involve some unpredictable “intuitive jumps” where alternative approaches (and nasty criticisms) seem to appear as if by magic. What experienced model developers actually do, and what we try to enhance in workshop processes like AEAM (Holling 1978), is to use various psychological tactics for stimulating imagination so as to increase the odds of such intuitive jumps; such tactics range from simple brainstorming (free association thinking) to encouragement of frustration and fear of making embarrassing mistakes.

Through AEA experience with trying many tactics for state variable identification and from discourse with participants in AEA workshops, we have moved gradually toward an eclectic approach that involves deliberately working from several directions. In order of priority and timing, these directions are outward, top-down, and bottom-up as described in the following subsections.

“Outward” from Key Policy Variables, Beginning with Factors Responsible for Change in These Variables

This tactic insures that however fragmentary or incomplete the “final” working model may be, it will at least contain a set of causal links or assertions about factors that directly impact variables of management interest. For common biophysical variables like sediment loads and animal population sizes, it most often results in familiar decompositions of predictions about change into more manageable components (for example, loading-suspension-deposition rates, birth-death rates) that can then be

represented as functions of various factors and policy choices.

“Top-Down” from Broad Phenomenological Descriptions of Ecosystem Structure and Function

Working outward does not always result in state variable sets that reflect broad understanding about historical dynamics and basic mass-energy conservation principles. For example, in development of Grand Canyon ecosystem models, we did not pay much attention initially to exotic fish species, concentrating instead on physical habitat relationships of native creatures and potential impacts of improving that habitat (for example, restoring more natural flows and temperatures); only when we began to review broad descriptions of historical change did we begin to wonder whether “turning back the clock” on physical factors would work at all, since irreversible exotic introductions may have done much damage even before any major habitat alterations occurred. Also, thinking about broad mass transport relationships and upstream-downstream shifts from autochthonous to allochthonous production sources helped us identify a suite of potentially negative impacts of physical habitat restoration that we otherwise might have failed to notice.

“Bottom-Up” from Microscale Process Arguments and Observations

The essential idea here is not to model everything possible, but rather to challenge any broad system description based on the previous two approaches for consistency with existing understanding of microscale dynamics, by looking for microscale processes and feedback relationships that could unexpectedly limit responses at larger scales or “explode” across scales through threshold and nonlinear dynamic effects. Note that this search for “vampires in the basement” is not based on the pretense that the best way to obtain a broad prediction is to add up a lot of small predictions, but rather on the concern that some “small” effects may have surprisingly large consequences.

The “working outward” tactic (Walters 1986) has been particularly important in helping prune unnecessary complexity from models and for insuring they attempt to answer the “right” questions. The top-down and bottom-up tactics are mainly useful for detecting gaps and inconsistencies in the variable set defined from policy interests alone. Many scientists are uncomfortable with the priorities above, preferring instead to begin with disciplinary descriptions of “The System” then adding extra relationships later to represent particular policy impacts; the

trouble with allowing this to happen is that it most often results in good models of things that interest scientists, but weak articulation of things that really matter to policy (but are often “uninteresting” from a scientific perspective).

So a key idea in the multiple-model adaptive approach is not to build any single, “best” model, but instead to deliberately develop and compare a whole collection of alternative models. A simple example of the difference this can make is in prediction of effects of trophic interactions. Suppose it is important to make policy predictions about how changes in the productivity of a “prey” species with biomass density H will impact biomass P of its predator(s). The simplest model we might propose for the biomass “flow” C from H to P would be the mass action rate $C = aHP$, where a is rate of effective search of the predator. Simple predator-prey theory tells us in this case that we will predict increases in the productivity of H to result in increases in P , but not in H , unless some other factor besides food prevents P from increasing or unless increases in potential productivity are somehow “shunted” to creatures that the predator cannot eat. In aquatic systems, one common recommendation about how to model the interaction more precisely is to divide the prey and predator populations into age-size components, H_i , P_j , with effective search rates a_{ij} reflecting trophic ontogeny of the predator, so $C = \sum_{ij} a_{ij} H_i P_j$, and to track the H and P components over time by using size-age structure accounting. But if only this complication is added to the analysis, the basic qualitative prediction that increases in prey productivity should result in changes in P but not H still holds, so articulation of a_{ij} rather than just a single mean a does not mean that we will make “better” predictions especially if the a_{ij} are more difficult to estimate than mean a . Suppose that in contrast to the usual size-age “decomposition” of predictions, we instead divide the prey population into two behavioral “vulnerability” classes, V and $H-V$, where we assume rapid behavioral exchange between the classes (for example, V might represent actively feeding prey that are exposed to predation through feeding behaviors, whereas $H-V$ represents inactive, hiding prey). If prey move between these classes at rates $k_1 V$ (prey moving into hiding) and $k_2(H-V)$ (prey moving out of hiding), then fast dynamics of exchange along with $C = aVP$ (mass action encounters, but only with the vulnerable prey) imply that V will vary over time to track slower changes in H as $V = k_2 H / (k_1 + k_2 + aP)$. That is, there will be a “ratio-dependent” predator-prey interaction (Abrams and Walters 1996) implying very different macro-scale correlations between prey

productivity and both prey and predator abundance than would be predicted from simplistic assumptions about microscale dynamics. The point of this example is simple: only when you see the two approaches side by side does it become obvious that it might be smarter to invest time and effort in analysis of behavioral and distribution data (to obtain insights about the k 's) than to blindly assume “more is better” by subdividing the populations and estimating a_{ij} 's.

TACTICS FOR REDUCING COMPUTATIONAL COMPLEXITY

Whereas the state variable identification methods reviewed above can help to avoid unnecessary or irrelevant model complexity, for riparian settings they generally still result in a daunting array of physical, chemical, and biological variables that need to be simulated. State-of-the-art disciplinary approaches to computational representation of some of these variables would result in massive computational chores, for example, hydrodynamic velocity fields usually are simulated on scales of meters-seconds, and individual-based animal population models (DeAngelis and Gross 1992; Van Winkle and others 1993, 1996, 1997) can track hourly or daily fates of thousands of individuals. It is generally impractical to simply throw together a collection of such models into a single system representation.

A variety of tactics can be used to avoid impossible computational complexity. These fall into two broad categories: (a) separate execution of some physical “forcing” calculations (for example, detailed hydrodynamic submodels), with only aggregated (larger-scale) results transferred as inputs to ecological calculations; and (b) use of various analytical approximations for average and/or extreme values of fine-scale variables (“think before you compute” approaches).

An example of the computational separation approach is in assessments of water management options for Glen Canyon Dam on the Colorado River in the Grand Canyon. There we run a detailed model (Wiele and Smith 1996) of hourly water releases and consequent downstream propagation of stage “waves” from Glen Canyon Dam into the River, and we store typical diurnal-downstream patterns from this model (under alternative diurnal release policies) for input into calculations of aquatic ecosystem responses, such as changes in benthic primary production. In North American riparian settings, agencies like the Bureau of Reclamation and Core of Engineers have been quite cooperative about providing expertise and model results for such physical forcing calculations.

Analytical approaches for cross-scale representation generally are based on the assumption that prediction of changes in “slower” (usually ecological) variables can be based on averages or extreme values of “faster” (usually physical) variables, that is, that the slower variables do not “see” all of the finer-scale variation. Three techniques that we have found particularly useful are (a) variable speed splitting; (b) analytical integration over microscale variation; and (c) Lagrangian trajectory sampling. The following subsections provide more information about these techniques.

Variable Speed Splitting

Often we find that rate equations for “fast” (rapidly changing) variables like number of animals vulnerable to predators (example in previous section) or benthic algal biomass imply that these variables will move to equilibrium values on time scales that are shorter than are of direct policy interest, and will change over larger scales in response to slower changes in factors that influence equilibrium levels (total number of animals available to become vulnerable to predators, seasonal changes in water turbidity and temperature). In such cases, we can avoid much tedious and unnecessary computation and also obtain useful insights about factors that determine “average” (equilibrium) variable values, by simply solving the rate equations for the equilibrium values of fast variables as functions of the slower ones. In the predator vulnerability example, the rate equation for number of vulnerable prey is $dV/dt = k_2(H - V) - k_1V - aHP$, and as noted above this model has solution $V = k_2H/(k_1 + k_2 + aP)$ when $dV/dt = 0$; it is quite useful to see that V should be proportional to total prey population H , and inversely proportional to predator abundance P . In this example, the behavioral rate constants k_1 and k_2 most often would imply equilibration of V on time scales of hours, whereas H and P might vary on much slower scales of weeks to years. Variable speed splitting historically has been used widely in ecology, for example, in derivation of predation functional response equations like Holling’s (1965) disc equation or its Michaelis-Menten equivalent, and users of such equations often appear unaware of the original arguments; by recognizing these arguments, we see that they can be applied much more widely to fast dynamic phenomena.

Analytical Integration over Microscale Variation

Generally we approximate spatial (and temporal) patterns through some sort of discretization scheme, in terms of spatial pixels or slices of habitat (and

time steps); a serious logical error is to assume that we have to treat events and patterns within such pixels as homogeneous or random. Actually, when we define a pixel-scale variable and its dynamics, the only assumption that we must make is that we can construct dynamics rules to represent the integral of finer-scale patterns/events over the pixel; by thinking of the pixel-scale rules this way, often we can easily design better integral relationships than would result from assuming internal randomness or evenness. For example, if we need to predict sediment resuspension rate from an area of river bottom, we need not assume the bottom has homogeneous resuspension energy (velocity) or sediment deposit composition; instead, we can view the total rate as the integral over a heterogeneous field of local energy/sediment composition sites, with statistical or discretized (for example, pool vs riffle, slow vs fast) description of the field. Like the example in Figure 1, integral rates derived this way might differ very considerably in form and pattern than would be expected by thinking about any single, “typical” physical situation (energy/composition combination) within the pixel.

Lagrangian Trajectory Sampling

In riparian ecosystems, many materials (and organisms as drift) move rapidly downstream while undergoing various transformations and addition/removal processes. Solving for total flux and spatial concentration patterns can be an extremely difficult computational problem in such settings (very short space and time steps needed for numerical stability of rate equations). There are two ways of describing these processes. A Lagrangian approach follows a parcel of water as it moves through space and time, whereas a Eulerian approach describes the state of the fluid at every point in space and time. Often we can simply sidestep the “Eulerian box” (see Appendix A.1.6; Pond and Pickard 1983) computational problems by thinking carefully about where/when/how often there are major changes in the space-time concentration field, and computing only enough space-time sample tracks to characterize the overall field. Each of these sample tracks involves a “Lagrangian” calculation: compute changes over time in a representative spatial parcel of water as it moves downstream through the space-time field (Figure 2). Such tracking calculations often can be done with analytical integrals over short river reaches, with breaks at points where rapid changes (such as tributary inputs) are expected; the rapid changes then are treated as point disturbances in the state variables. Similar ideas can be applied to sampling of the more complex spatial patterns arising from two- and three-dimensional hydrody-

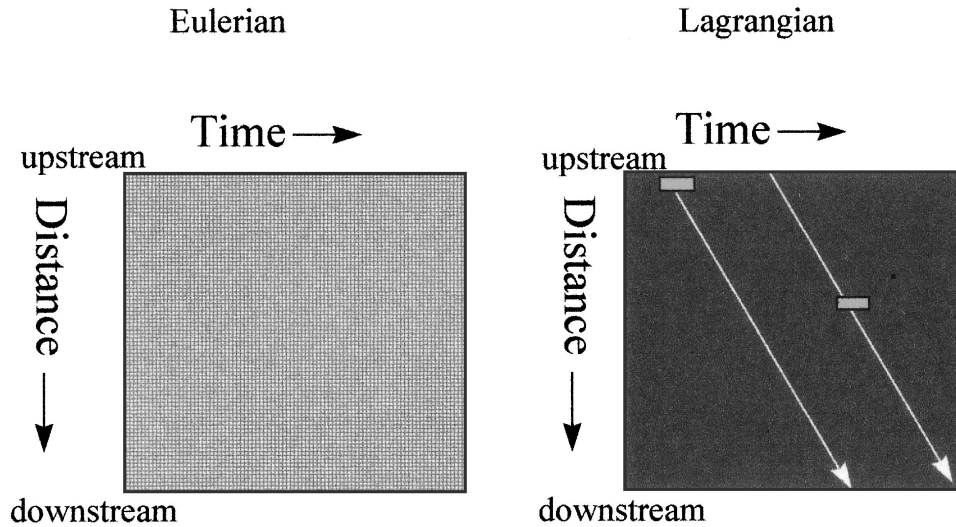


Figure 2. Description of Eulerian and Lagrangian methods for capturing dynamics of variables that are driven by processes occurring over small spatial scales or time periods. The Eulerian approach (left) involves computing the change in the state of all parcels of water (grid cells) over successive time periods. The Lagrangian approach (right) can be used to reduce computational costs by computing changes over time in a representative spatial parcel of water (boxes) as it moves downstream through the space–time field (the lines of the arrow represent the Eulerian grid) in a sample track (arrows).

dynamic models, though it obviously can become very computationally costly to obtain adequate tracking sample sizes.

Note that for each of the techniques outlined above, the critical modeling step is not in application of the technique, but rather in development of a clear picture of the variables and scales (pixel sizes) for which some technique might be needed or useful. At least as an interim step in the model development process, people involved in that process must be willing to stop and say “OK, we are stuck with these variables/pixels/scales; given these, how can we most efficiently represent dynamic variation.” In our experience, it can be extremely difficult to obtain such consensus even for momentary examination of a modeling option; many scientists are unwilling or uninterested in stopping anywhere short of a fully satisfactory reductionist description, even when it is obvious that the reduction process ultimately must fail due to incomplete state information and chaotic behavior of some microscale processes (for example, turbulence).

PRUNING UNNECESSARY COMPLEXITY BASED ON POLICY TESTS: MOVING FROM MODELS TO STRONG QUALITATIVE ARGUMENTS

In our experience with AEA processes, many scientists seem to take for granted that the value of

explicit modeling is to produce precise, quantitative predictions about the consequences of policy options, and furthermore that such predictions are necessary for wise policy choice. This is a serious misunderstanding about the nature of decision making, particularly in settings where the primary policy concern is about large-scale objectives, such as “restoration of natural ecosystem function” or “maintenance of biodiversity”. Prediction of some sort is implied by the very notion of “choice” (choices can only be compared by making some assertion, that is, some prediction, about the consequences of each), but it is a serious logical error to suppose that predictions must be quantitatively precise. For policy choice, what really matters is only that the prediction method or model be accurate enough about alternative outcomes to correctly *order* the choices; most often, correct ordering requires nothing more than knowledge of qualitative outcomes (existence and direction of responses) or ranges of outcomes (for example, policy A will produce at least twice the response of policy B). This means that the really valuable ecosystem models generally are not those that make precise predictions, but rather those that guide and focus policy choice by helping to “prune” or “screen” options; generally the power of such models in decision making comes not from the numbers they generate, but rather from the way they help structure clearer, stronger qualitative arguments about the relative efficacy of various options. An exception to this rule

is where a successful policy must meet some absolute standard.

Ecosystem modeling for water management in the Grand Canyon provides an example of how multiscale modeling can help screen options and uncover potentially fatal flaws in some of them. Before the modeling work, discussion about water management options for protecting endangered fish species had involved mainly predictions based on assuming that restoration of more natural physical habitat characteristics would lead to positive population responses by species adapted to these characteristics (for example, warming the water and restoring natural seasonal flow and turbidity regimes should help these warm-water species). But initial model runs indicated exactly the opposite response, for two reasons: (a) competition and predation effects due to buildup in exotic species (brown trout, channel catfish) that had become dominant in the lower Colorado River even before construction of Glen Canyon Dam; and (b) reduction in autochthonous primary production in clear river waters below the dam, which in recent years has likely offset loss of allochthonous carbon sources in the headwaters of the river basin (and that would not be restored unless the dam were removed completely, and possibly not even then considering changes in headwaters land use). In short, the model predicted that negative trophic effects could more than offset any benefits of physical habitat improvement; this argument had been made by some experienced scientists in the system (for example, Valdez and Ryel 1995) but apparently had not been taken very seriously in impact assessments. Of course the argument may be wrong, but at least we know that it should figure prominently in design of future adaptive management experiments and monitoring programs.

In this Grand Canyon setting there will doubtless be much debate about the best approach to resolving uncertainty about negative trophic side effects of physical habitat restoration. We have tried a reductionist approach, by modeling habitat linkages and population dynamics of exotic fishes in some detail (Walters and Korman 1999), to see if existing knowledge about these fishes would permit a clear yes-no impact prediction and to determine more precisely what additional data would be needed to make such a prediction. In our judgement, this is a hopeless approach; there are just too many relationships that would be difficult or impossible to quantify through small-scale or comparative research and too high a risk of overlooking some key relationship entirely. The alternative approach for debate is large-scale management experiments: try some physical restoration options and directly monitor

overall responses to these. The main arguments against this alternative (besides those from scientists hungry for funding to study various relationships) are (a) some treatment options are very expensive to implement (for example, warm-water release facilities at Glen Canyon Dam) and would require expensive long-term monitoring programs to evaluate; (b) experimental treatments are ecologically "risky" (compared to maintaining status quo management policies while waiting for scientists to somehow get the data needed for accurate modeling); and (c) the probability of detecting responses from experimental treatments may be very low given natural and measurement variability and weaknesses in the design of the experiment (lack of control, replication, and stratification) due to political and institutional constraints. Unfortunately the likely outcome of this debate will be a no-win compromise: inadequate research investment combined with modest (and largely uninformative) experimental policies.

WHERE WE ARE STILL FAILING: BIG EFFECTS FROM SMALL CAUSES IN RIPARIAN SETTINGS

Despite considerable progress in development of modeling methods for representation of cross-scale dynamics and policy impacts, we still have much difficulty making useful predictions about some relationships that could be particularly important in riparian settings. The following subsections review three problems that have been particularly troublesome.

Propagation of Small Physical Changes to Large Spatial Scales

River channels and associated floodplains have complex microtopography that can change rapidly under some flow regimes. This microtopography permits much biological diversity and productivity; for example, backwaters along the Colorado River are extremely productive compared with the main river channel and provide key habitats for various plant species, juvenile fishes, and birds. But these backwaters are linked to the main river by small channels through unstable sandbars, so small changes in flow can have severe impacts not only by cutting them off from channel recharge during low flows and scouring of bottom communities during higher flows, but also by altering the channel structure and hence the impact of future flows. In other words, there are large, critical habitat areas whose basic long-term dynamics depend on microscale dynamics of spatial linkage. We can try to describe the linkage

structure in terms of statistical patterns like flooded area versus stage curves, but such relationships are misleading if microtopography is subject to rapid change (erosion–deposition dynamics) and are difficult to link to representations of ecological processes such as succession.

Ecological Impact of Infrequent, Acute Physical Transients

Often we need to link relatively “slow” biological variables, such as vegetation establishment and growth, to physical variables, such as, flow that show complex, high-frequency variation. Usually we attempt this linkage by using aggregate statistical properties of such fast variables (for example, mean flow) on the assumption that slower variables are unable to “see” or respond to most of the high-frequency variation. But this assumption is only valid if slow variable responses are linear (smooth and incremental) over all values of the fast variables; if there are response thresholds or highly nonlinear responses to extreme variable values, long time scale averages may be very misleading. For example, even very short duration flow “spikes” or experimental releases can kill some riparian plants or cause scouring that uproots plants and alters substrate structure (soil particle size distribution and organic content) in complex ways. Obviously we can represent such cross-scale impacts as “event” changes in state of slow variables, with consequent recovery dynamics at longer time scales. But the modeling/measurement problem then becomes how to predict state response to the event, and we seldom have enough historical data on such event responses to provide empirical relationships that cover an adequate range of possible duration–intensity combinations (especially for experimental flow management regimes that deliberately create flow regimes that are outside of recent historical experience). We can try a reductionist approach by decomposing the event response into possibly more predictable components (acute physiological effects, substrate changes, etc.), but usually such decompositions reveal a daunting tangle of detailed responses and uncertainties that would be impractical to eliminate through comprehensive process experimentation.

Population–Habitat Interactions Associated with Ontogenetic Shifts

To be useful in most riparian policy settings, ecosystem-scale analyses of physical and trophic change have to be combined with (interfaced to) more detailed evaluation of impact on particular target species that have high use value (recreational fisher-

ies) or are legally mandated for protection (threatened and endangered species). Such species usually have complex life histories, sometimes involving seasonal migrations to spawn or breed (and perhaps overwinter) at particular sites and almost always involving ontogenetic shifts in habitat use by individuals as they grow and become capable of avoiding more predators and exploiting a wider range of food resources. Animal movements associated with these life history shifts can create strong cross-scale effects: even very short duration movements can expose animals to complex physical and biological impacts at quite large spatial scales or in particular spatial locations. For example, endangered humpback chub in the Colorado River migrate mainly into one major tributary [Little Colorado River (LCR)] to spawn; small juveniles dispersing downstream in the LCR encounter a variety of risks from introduced fish species, and particularly high risk for a short time when/if they enter the mainstem Colorado where they are likely to encounter a sudden temperature drop that may impair swimming and escape behaviors. An obvious tactic for such species is to use individual-based models (IBMs) to “sample” the space–time trajectories of risks and opportunities associated with alternative policy choices (that is, different flow regimes and temperatures); but this approach quickly becomes too costly (data requirements, model development, and delay in policy implementation) when a whole suite of species require such special attention. Also, the sampling process may require a fairly detailed space–time representation of biophysical conditions encountered by individuals.

So far we have approached ecosystem modeling exercises in places like the Everglades and the Grand Canyon with hope that we will encounter only a limited number of nasty cross-scale prediction problems of these three types, and that these few problems can be approached through a combination of more detailed modeling and carefully focused field observations and experiments. Experience to date supports this hope, but that experience is very limited.

CONCLUSIONS

Ecosystem modeling for riparian policy analysis can be approached with a variety of aims, from simple screening of broad policy options (to demonstrate that simplistic “Band aid” policies are unlikely to succeed) to detailed quantification of tradeoffs among multiple performance measures. Despite various pitfalls in variable identification and representation of cross-scale linkages as discussed in this

article, our capability to meet the simpler aims has improved dramatically over the last 20 years. We can even represent some tradeoffs with considerable precision (for example, power production value vs recruitment of some fish species) in settings where historical management changes and monitoring/research have provided a good empirical basis for testing key functional relationships that define the tradeoffs. The real challenge will be to see whether we can move beyond those cases where the modeling just tells us what we already know from experience, to make useful predictions about situations where experience and data are severely limited.

Restoration alternatives for riparian ecosystems in almost all cases highlight conflicting water requirements among resources. The most obvious conflicts involve perceived or real ecological benefits associated with a more natural hydrograph that must be traded off against financial losses in power generation and flood control. However, there are often conflicting requirements among ecological components; particular hydrographs and other restoration options (temperature and water clarity) may be beneficial to one species but harmful to another. In the Grand Canyon example, temperature and flow regimes considered beneficial for native fish are likely devastating for a highly productive tailwater rainbow trout population that supports a blue ribbon fishery. Making decisions in the face of such resource tradeoffs is an extremely difficult task and must involve assigning a value to each resource and how this value changes with changes in the resource. When faced with this situation, it is not our inability to precisely model these resource tradeoffs, but the inability of institutions to deal with this issue that is the biggest impediment to making a decision. The modeling approaches we advocate in this article allow relatively rapid model development (that is, 1 or 2 y) to highlight key uncertainties and to help develop an experimental management program. In the long term, the model development process is an iterative one: (a) an initial model is formulated and uncertainties are identified; (b) experimental management and monitoring is implemented to resolve these uncertainties; and (c) the model is revised based on these data and the cycle is continued.

Detailed and time-consuming modeling activities may not only be misleading but provide an excuse for continued procrastination of institutions resistant to making changes in current management practices.

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