THE ART OF THE UNSOLVABLE:
LOCATING THE VITAL CENTER OF SCIENCE FOR ENVIRONMENTAL LAW & POLICY

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Forum on Law, Science & the Environment
Lewis & Clark Law School, April 2007

I. Introduction

Wildlife management, a critical issue in many areas of the United States, is a problem that superficially appears science ought to be able to resolve handily. Yet, it has proven to be far from simple and is exemplary of the complex dynamics that can emerge from simple biological interactions. Deer populations, for example, can be modeled using a formula with just one variable, but this analytic simplicity is deceptive. The formula, it turns out, can be stunningly sensitive to minor variations in its single parameter—a difference of just one tenth of one percent can determine whether a management decision is likely to succeed or fail.[1]

This example highlights a basic truth that is often overlooked. Science is limited by both the power of its methods *and* the characteristics of its subject matter. Ideal scientific problems are ones with sufficient complexity and generality to make them interesting, but not so much that they becomes intractable. Identifying good scientific problems is therefore essential to success as a scientist and to successful science. In this light, "[i]f politics is the art of the possible, [science] is surely the art of the soluble."[2]

Scientists working in fields relevant to environmental law are rarely able to select problems with an optimal balance of broad implications and potential solutions. Escaping from the aridity of the laboratory comes at a steep price—the inchoate swamp of the natural world. Issues ranging from the toxicity of industrial chemicals, to the protection of endangered species, to the projected magnitude of global warming transcend existing scientific knowledge.

This complexity poses an unsettling question: If scientific uncertainty is so pervasive, what exactly do scientific methods contribute to environmental policymaking? Resolving this question has proven to be exceedingly difficult, both because of the technical challenges and the high stakes. Typically, it is answered in the negative—folks know bad science when they see it—which more often than not simply involves dissecting the inevitable gaps in an opponent's scientific methods.[3]

The resulting war of attrition has spawned a corrosive brand of skepticism. Critics on both sides of the debate baldly challenge environmental science for being reductive—a position akin to criticizing a painting by Picasso for its failure to represent its subject matter

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[1] Deirdre N. McCloskey, *History, Differential Equations and the Problem of Narration*, 30 *HISTORY AND THEORY* 21, 35 (1991).

[2] P. B. Medawar, THE ART OF THE SOLUBLE 7 (1967). Medawar portrays science as a pragmatic enterprise: "[g]ood scientists study the most important problems they think that they can solve. It is, after all, their professional business to solve problems, not merely to grapple with them." *Id*.

[3] Holly Doremus, *Science Plays Defence: Natural Resource Management in the Bush Administration*, 32 Ecology L.Q. 249, 252-53 (2005) (discussing the "strident pitch" of the debate over the quality of science used in environmental policymaking).

realistically—and ignore the unavoidable epistemological constraints. Arthur Leff has framed the dilemma incisively: "the less [a scientist] accepts as relevant, the less he can say that is not misleading; the more he accepts as relevant, the less he can say at all."[4] Environmental science is vulnerable to attack because striking this balance so often rests on tenuous grounds.

Determining the proper role of science is complicated further by the thorny moral questions that are interwoven with methodological considerations. Most risk assessments, for example, focus on certain risks of human mortality (e.g., contracting cancer), while omitting other mortality risks and only rarely considering morbidity. Yet, regardless of whether the relevant data are obtainable, undercounting potential risks to human health will skew the analysis. This blurring together of difficult methodological and moral judgments has confounded efforts to resolve controversies over environmental science.[5]

The image of science that has emerged from this debate is distorted by expectations that are simultaneously too great and too modest. By clinging to a classical vision of science, critics set environmental science up for failure; by presuming that scientific results are primarily the product of ideology, they risk trivializing their value.[6] These polarized views have mired debate between a world of inviolable, deterministic science and an overly cynical one in which science cannot be trusted unless it is purified of all corrupting influences.

This Article develops an alternative account of what science offers environmental policy. As prefigured above, the simple answer is that the power of science depends on the nature of the problem and the strength of the tools available to analyze it. Good science ranges from the highly precise and accurate methods found in the hard sciences (*e.g.*, Newtonian physics) to heuristic models that expose general patterns in complex systems (*e.g.*, ecology).[7] Science is thus inherently pluralistic, as the different scientific disciplines attest, and a unitary conception of environmental science is neither a desirable end nor a viable goal.

It follows from this pluralistic view that a general standard for judging scientific results does not exist. Even the most widely accepted scientific convention, that empirical findings satisfy a ninety-five percent significance level, is not universal. To give just one example, subtle effects can matter a lot in environmental policy. Just as stealing a penny from every bank account in the U.S. would make you rich, weak effects spread over large populations can, in the

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[4] Arthur A. Leff, *Economic Analysis of the Law: Some Realism About Nominalism*, 60 VA. L. REV. 451, 477 (1974). S*ee also* Philip E. Tetlock, EXPERT POLITICAL JUDGMENT: HOW GOOD IS IT? HOW CAN WE KNOW? 19, 147 (2005) ("if we only accept evidence that confirms our worldview, we will become prisoners of our preconceptions, but if we subject all evidence, agreeable or disagreeable, to the same scrutiny, we will be overwhelmed.").

[5] Wendy Wagner, *The "Bad Science" Fiction: Reclaiming the Debate over the Role of Science in PublicHealth and Environmental Regulation*, 66 LAW & CONTEMPT. PROBS. 63, 66 (2003) ("Since the zigzag nature of science and policy blur the respective roles of science and policy in regulatory decisionmaking, these political checks and balances can be lost or at least impeded by the complex interweaving of technical and value judgments.")

[6] Doremus, *supra* note 3, at 259-60, 261-63 (arguing that advocates on both side of the environmental policy debate have taken advantage of "public misperception of science as a binary enterprise, essentially dividing scientific assertions neatly into two categories: those conclusively proven and those patently false").

[7] In fields such as econometrics, which are subject to similar levels of uncertainty, it is "generally acknowledge that [] models are 'false' and that there is no hope, or pretence, that through them 'truth' will be found." Peter Kennedy, A GUIDE TO ECONOMETRICS81-82 (5th ed., 2004). Instead, models are viewed as "rough guides to understanding." *Id.*

aggregate, have significant consequences. In such cases, statistical significance will rarely be met, but this failure only confirms that the effect is subtle, not that it is absent. Such limitations do not diminish the value of statistical testing; they show only that scientific standards cannot be applied mechanically and that, similar to legal rules, exceptions to them will always exist.

This Article seeks to identify benchmarks for science that respect the contingencies of environmental problems (and policies) without lapsing into a self-defeating form of scientific relativism. The Article begins by examining the controversy over the role of science in environmental law and placing it in a broader context by drawing on parallel debates in finance theory. It then argues that relatively simple models, supplemented by narrower, more realistic assessments, are essential to understanding even the most complex environmental problems. The Article concludes by briefly identifying misconceptions that unnecessarily exacerbate the gulf often perceived between social values and quantitative methods.

II. A View of Science Beyond Environmental Law and Policy

The risks posed by industrial chemicals represent an extreme example of how implacable scientific problems can be. The methods available for testing chemical toxicity are hampered above all by the complex biology of chemical toxicity and its sensitivity to context (*e.g.*, potency is often a function of the presence of other factors or compounds). Matters are made worse by the subtlety of the effects, which frequently involve harms that are manifest in one person out of ten thousand. The absence of effective testing methods have, in turn, impeded scientific understanding of the mechanisms underlying toxic responses that could aid in developing new experimental protocols or strengthen existing ones.

The stark nature of these uncertainties combined with the human drama associated with toxic chemicals have made toxics regulation a particularly salient issue politically. Failed or faulty regulation of industrial toxins has been the poster child, and at times the whipping post, for the false promise of science in environmental policy. If commentators wish to expose the evils of "junk science" or to dramatize the significance of value judgments inherent in technocratic approaches to policymaking, toxic risk assessment has been the example of choice.

Toxics issues have had a powerful effect on current understanding of environmental science because of this high visibility. Unfortunately, the attributes that make toxics issues salient also have polarized debate and fueled misperceptions that science is binary, either good or bad, when it spans a broad spectrum of degrees of accuracy and precision.

This section looks beyond the domain of environmental law to identify appropriate benchmarks for the role of science in environmental policymaking. The logic of this strategy is straightforward. Just as complex problems are made more accessible by studying simple variants, so too will it be easier to evaluate scientific methods by studying them in a less contentious political environment. A unique contribution of the Article is identification of such a field—finance theory and modeling.

Financial markets provide an exemplary test bed for the practical application of sophisticated scientific methods. They equal, or exceed, the complexity of many natural systems, as suggested by the long history of economics and ecology influencing each other. Further, the quantitative skill of financial modelers is superlative, and the resources on Wall Street are unrivaled. Even the strict instrumental rationality of financial analysts is a virtue, as it rigorously selects for scientific methods that work. Unlike environmental science, though, the moral implications of financial models are remote. These conditions provide an unconstrained, pragmatic benchmark for applying scientific methods to complex systems.

A. Scientific Pragmatism on Wall Street

While it might initially appear incongruous to discuss financial modeling in conjunction with environmental science, links between the biological sciences and economics have a long and notable history. Market theory, for example, owes a great debt to the concept of "the survival of the fittest" drawn from Darwin's theory of natural selection.[8] Similarly, mathematical theories of optimization, particularly game theory, have been exported from economics to biology, where they have proven to be tremendously influential.[9]

Financial analysts, like biologists, must contend with enormously complex systems that vary over time, contain highly heterogeneous elements, and involve many non-linearities.[10] As we saw in the introduction, feedbacks can make systems highly sensitive to local conditions that, in turn, may be subject to significant temporal variations—each component is both subject to the influence of surrounding elements and is part of the changing environment that these other elements experience. As a consequence, the dynamics are more those of a crowd than the bounded motion of a ball moving down an inclined plane.

A byproduct of this complexity is the largely unconstrained behavior that results. Just as legal discretion expands with the number of factors a judge is permitted to consider, the vast number of variables found in economic systems causes their evolution to be unbounded in many respects. This open-ended nature introduces an element of *contingency* and limits the role of traditional scientific methods. A basic premise of traditional scientific methods is that natural systems operate according to certain laws and that they display a discrete set of patterns that reflect the characteristics of the laws that govern them. By contrast, movements of stock prices are dependent on "unique historical 'accidents' that cannot, in principle, be predicted."[11]

None of these obstacles has impeded the use of quantitative methods in finance theory or day-to-day financial analysis. To the contrary, financial modeling is increasing in its importance on Wall Street.[12] During the 1980s and 90s there was an infusion of quantitative analysts with Ph.D.s in physics, mathematics, and computer science. The development of more exotic

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[8] Richard Levins & Richard Lewontin, THE DIALECTICAL BIOLOGIST 84 (1985) (noting the striking parallels between evolutionary theory and classical economic market theory).

[9] *Id*. at 25.

[10] Donald G. Saari, *Mathematical Complexity of Simple Economics*, 42 Notices Am. Math. Soc. 222, 222 (1995).

[11] Stephen J. Gould, THE HEDGEHOG, THE FOX, AND THE MAGISTER'S POX: MENDING THE GAPS BETWEEN SCIENCE AND THE HUMANITIES 202, 224-28 (2005).

[12] Gary Stix, *A Calculus of Risk*, SCI AM. 92, 92-93 (May 1998) (noting the growth of "financial engineering or "econophysics" during the 1990s with the expansion of new financial instruments).

financial instruments, particularly the rising importance of options trading and hedge funds, has added further impetus to using quantitative models.

Financial analysts acknowledge that their models cannot be fully verified and that they are inevitably partly true and partly false. They self consciously distinguish their methods from those used by hard scientists and engineers:

> In engineering . . . optimization is sensible because each scenario is precisely understood, and you're trying to find the best one. In financial theory, in contrast, each scenario is imprecisely wrong . . . . While averaging may cancel much of the [errors in a model], optimization tends to accentuate your lack of knowledge."[13]

In other words, if you begin with a set of assumptions that you know to be partially false, seeking the optimal solution based on them stands to magnify the effects of these starting imperfections.

Financial modelers rely instead on phenomenological models that are derived by analogy from preexisting models for other systems. This process involves identifying well-understood phenomena with similar characteristics and then revising the associated model parameters using financial data. A concrete example of this approach in physics is the use of the equations that describe a drop of water as a model for the nucleus of an atom.[14] Only where an analogous system is unavailable will modelers resort to statistical methods, which being the bluntest analytical tools have the lowest fidelity.[15]

One of the most successful examples of this approach is the Black-Scholes model for option pricing. Yet, consistent with the descriptions above, "[t]he real world violates most of the principles of options theory."[16] Black-Scholes operates much like the Coase theorem in law and economics, whose central assumption—costless transactions—is obviously false, but whose virtue is providing an intuitive framework for thinking about more realistic scenarios. Similar to the Coase theorem, it is the conceptual tractability, not its accuracy, that drives analysts to use the Black-Scholes model.

Emanuel Derman, a former Wall Street investment banker turned professor of finance theory, explains the reasoning behind this success as follows:

> . . . all Black-Scholes requires of you is your opinion about future [price] volatility. It then converts your conceptual thoughts about future uncertainty into a fair dollar value. This is no black box or voodoo model; it's reason transmuted to numbers, and that's the right way for a model to work.

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[13] Emanuel Derman, *A Guide for the Perplexed Quant*, 1 QUANT. FINANCE 476, 478 (May 2001).

[14] Derman, *supra* note 13, at 477.

[15] By design, statistical methods filter out most of the dynamical details by reducing virtually all systems to simple linear models and a small number of parameters. This is equivalent to the process of reducing the physical characteristics of people in a television cartoon to their most prominent features (*e.g.*, Marge's beehive hairdo or Homer's bald head on the Simpsons).

[16] Derman, *supra* note 13, at 478; Emanuel Derman & Nassim N. Taleb, *The Illusion of Dynamic Replication*, 5 Quant. Finance 323, 324-5 (2005).

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> Better to have market models with variables and factors you can name and whose nature you can grasp and opine about, than to have black-box models that dictate actions without a perceived structure.[17]

Derman's point is two-fold. First, because no financial model can be perfectly accurate, applying and using them will entail difficult interpretative judgments—model results cannot be read off mechanically. Second, analysts cannot interpret model results without having an intuitive understanding of its parameters and its functional properties. As such, models like Black-Scholes operate as useful conventions against which judgments are made and other models constructed, not as rigid formulas for determining actions.

Truth in this context is pragmatic, and the success of a theory, at least initially, owes as much to persuasion and consensus as it does to a model's putative efficacy.[18] In this mode, scientific methods function more as tentative forecasting tools, which must be used skeptically and wisely. Derman describes them aptly as generating "a collection of parallel thought universes you can explore. Each universe should be consistent, but the real financial and human world is going to be much more complex than any of them. You're always trying to shoehorn the real world into one of them to see how useful that approximation is."[19]

Models, like theorizing generally, can be taken to extremes or reified. The financial sector is as subject to such overreaching, or conceptual rigidity, as any field of human endeavor. This tendency was displayed spectacularly in 1998 with the near meltdown of Long-Term Capital Management ("LTCM"), a hedge fund run by an elite group of financial analysts and economists that included two Nobel Laureates. LTCM lost more than six billion dollars in six weeks following an unexpected economic retrenching of the Russian government.[20]

The story of the LTCM debacle is not a simple one. While there is certainly evidence that the principles had unfounded confidence in their models, it is by no means clear that similar lapses can be averted. Indeed, several hedge funds have lost huge sums of money in subsequent years.[21] These failures may be par for the course—the underlying dynamics are extremely complex, data are scarce, and systematic testing is either limited or impossible.[22]

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[17] Derman, *supra* note 13, at 478. *See also* Salih N. Neftci, PRINCIPLES OF FINANCIAL ENGINEERING 437 (2005) ("the Black-Scholes formula is *simple* and it depends on a small number of parameters. In fact, the only major parameter that it depends on is the volatility, σ. A simple formula has some advantages. It is easy to understand and remember. But more importantly, it is also easy to realize *where* or *when* it may go wrong. A simple formula permits developing ways to correct for any inaccuracies i*nformally* by making subjective adjustments during trading.").

[18] Derman, *supra* note 13, at 480.

[19] *Id.*

[20] Franklin R. Edwards, *Hedge Funds and the Collapse of Long-Term Capital Management*, 13 J. Econ. Perspectives 189, 198 (1999).

[21] Jenny Anderson, *After Loss, Hedge Fund Will Close*, N.Y. Times, Sect. C, p. 1 (Sept. 30, 2006).

[22] Jessica James, *A Little Learning Is a Dangerous Thing*, 1 Quant. Finance 380, 380-81 (Aug. 2001).

Much therefore depends on the wisdom of financial analysts. After all, no model can be taken literally and interpreting them will inevitably be part art and part science. Derman, once again, captures the spirit of and the difficult judgments inherent in the enterprise:

> The success of options valuation is the story of a simple, asymptotically correct idea, taken more seriously than it deserved and then used extravagantly, with hubris, as a crutch to human thinking . . . . But the catastrophes of options valuation are the obverse side of the same coin, when people pay more attention to formulae than ideas, so that extravagance evolves into idolatry . . . . Somewhere between these two extremes, north of hubris but south of idolatry, lies the wise use of models.[23]

The practice of financial modeling discussed above exposes a surprising truth—realism is of relatively limited value in complex decision making settings. Despite their high levels of sophistication, resources, and incentives, financial analysts use remarkably simple models to predict and understand the behavior of complex market dynamics. This counterintuitive strategy is driven by a very practical insight: it is much harder to interpret and test the validity of a complex model than a simple one. As Wall Street analysts have learned, the better part of valor is to build models around the few simple patterns evident in complex systems, otherwise the number of potential solutions and relevant variables rapidly overwhelm the available data and human cognitive capacities.

These limits should not be interpreted as implying that complexity can be ignored. To the contrary, experience in financial markets, as the LTCM case illustrated, reveals that tragic errors all too often follow from reifying simple models. Policymakers and analysts must remain cognizant of the limits of the models on which they rely and be vigilant in determining whether changing conditions require that they be reassessed. In general, however, consideration of subtler or rare influences will be secondary in most quantitative models and difficult *qualitative* judgments will dictate when they need to be factored into an analysis.

B. Bridging Newtonian and Darwinian Traditions in Environmental Science

Herbert Simon and Allen Newell long ago observed that scientific work is subject to two opposing pulls: On the one side, a powerful attraction is exerted by "good problems" . . . . On the other side, strong pulls are exerted by "good techniques."[24] They then went on to warn that when these two pulls fall out of sync,

> science is threatened by schism. Some investigators will insist on working on important problems with methods that are insufficiently powerful and that lack rigor; others will insist on tackling problems that are easily handled with the available tools, however unimportant those problems may be.[25]

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[23] Derman, *supra* note 13, at 480.

[24] Allen Newell & Herbert Simon, *Computer Simulation of Human Thinking*, 134 Science 2011 (1961).

[25] *Id.*

The difficult problems raised by environmental policy have promoted a similar schism in environmental science. Missing in the current debate is a clear conception of the reliable, though still contingent, center where scientific methods have sufficient power and rigor to be useful. Further, by failing to have a clear conception of good science, environmental science is made much more vulnerable to the political battles that dominate environmental law and policy.

Drawing on the preceding discussion, this section describes the basic contours of scientific practices required to address complex environmental problems effectively. A basic premise of scientific modeling is that unimportant details must be suppressed (or average out) because they obscure the few stable patterns that provide a conceptual foothold for understanding complex systems and predicting their behavior.[26] Put more simply, just as maps omit secondary roads and focus on primary routes for low scale, long distance travelers, so to do statistical methods, and scientific models more generally, focus on the variables with the greatest relevance and clearest associations.

Echoing Derman's comments above, Simon Levin, an ecological modeler and theorist, describes this approach with characteristic clarity:

> This is the principal technique of scientific inquiry: by changing the scale of description, we move from unpredictable, unrepeatable individual cases to collections of cases whose behavior is regular enough to allow generalizations to be made. In so doing, we trade off the loss of detail or heterogeneity within a group for the gain of predictability; we thereby extract and abstract those fine-scale features that have relevance for the phenomena observed on other scales.[27]

One implication of this approach is that not all levels of abstraction for analyzing a problem are created equal.[28] Just as it would be foolish to try to study the behavior of a gas by attempting to follow the motion of every single gas molecule, so too may it be futile to attempt to understand biodiversity by tracking populations of individual species. Consideration of scale matters for basic scientific understanding and for very practical problems of effective environmental management. In fact, the two are closely linked because identification of strong associations (i.e., patterns) through basic scientific work makes environmental management possible.

The work of John Harte, another prominent ecological theorist, exemplifies this approach. Similar to the views expressed by financial modelers, Harte is skeptical of the current "infatuation" with highly complex models because they become as "inscrutable as nature itself" and are immune to testing and refinement.[29] Like Derman, Harte believes that simple models

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[26] Simon A. Levin, *The Problem of Pattern and Scale in Ecology*, 73 ECOLOGY 1943, 1947 (1992) (observing that "At very fine spatial and temporal scales, stochastic phenomena (or deterministically driven chaos) may make the systems of interest unpredictable. Thus we focus attention on larger spatial regions, longer time scales, or statistical ensembles [collections of things], for which macroscopic statistical behaviors are more regular.").

[27] *Id*. at 1947.

[28] *Id*. at 1960 ("That there is no single correct scale or level at which to describe a system does not mean that all scales serve equally well or that there are not scaling laws.").

[29] John Harte, *Toward a Synthesis of the Newtonian and Darwinian Worldviews*, 55 PHYSICS TODAY 29, 31 (October 2002) (arguing for an approach "based on models that capture the essence of the problem, but not all the

have the virtue of being readily interpretable, which ensures that scientists will have an intuitive sense of when and how they are likely to go wrong.

Identifying the stable patterns and laws that exist in complex systems is just the start of a much longer process, however. Simple models on their own are of limited value if scientists do not also develop an understanding of the underlying mechanisms.[30] In the biological sciences, Harte argues that the primary means for obtaining this information is discrete field studies that "combine the natural-history component of ecology[] with the experimental manipulations that are essential to testing putative mechanisms."[31] Once confirmed, this mechanistic knowledge can then be fed back into general models to improve their reliability and predictive power, as well as to enhance scientists' ability to interpret them effectively.

Scientific efforts to resolve the primary drivers of lake eutrophication provide a concrete example of this approach. In the 1970s, it was unclear whether the explosive algal growth in lakes throughout the Midwest was part of a natural cycle or caused by runoff from farms and cities (e.g., phosphates from fertilizers). Scientists from opposing sides of the debate constructed fantastically complicated models, some with literally hundreds of parameters, to support their opposing claims. Data, of course, were available for only a few of the model parameters, so scientists in each camp had virtually free reign to adjust the remaining parameters to conform to whatever position they were predisposed to believe.[32]

A breakthrough occurred towards the end of the decade when scientists discovered an association between algal levels in certain lakes and phosphorous levels in the rivers feeding them. The association revealed by the data was then used to construct a simple mass-balanced model for the Great Lakes system, *i.e.*, one that accounted for all of the flows into and out of the lakes. This model produced estimates of phosphorous levels for each of the Great Lakes with admirable accuracy, and predicted substantial benefits from reducing runoff into them. Importantly, "[b]ecause the model's handful of parameters were all readily measured, its output could not be fudged"; this gave scientists an independent basis for confidence in its predictions and a sound basis for policymakers to address the problem.[33]

In more complex settings, the tradeoffs between tractability and accuracy can become much more acute. The use of average global surface temperature in climate change research, for example, is a useful fiction that obscures a great deal of local variability that is of paramount importance to individuals living in regions likely to be hit the hardest. It is nevertheless a useful metric to start with because global averaging is less subject to the large, chaotic fluctuations that obscure evidence of changing climactic conditions at the regional level.

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details, might get us farther. We need to develop simple, mechanistic models. They will, perforce, be caricatures of the Earth system, but they must be falsifiable.")

[30] Harte, *supra* note 29, at 32; Levin, *supra* note 26, at 1948 ("Once patterns are detected and described, we can seek to discover the determinants of pattern, and the mechanisms that generate and maintain those patterns. With understanding of mechanisms, one has predictive capacity that is impossible with correlations alone.").

[31] Harte, *supra* note 29, at 32.

[32] *Id.*

[33] *Id.*

These tradeoffs merely highlight the fact that environmental problems must be analyzed at multiple levels because their dynamics may span many scales, whether temporal, spatial, or organizational. Science must consequently be viewed as an iterative process of learning and refinement, where conceptually simple models with broad generality are used in concert with localized studies (and models) that ensure important details are not overlooked. Notably absent in this vision of environmental science are rigid thumbs-up-thumbs-down standards of validity.

III. Conclusions: Scientific Bias Versus Social Value

Many critics of environmental science are likely to be unsatisfied with the model of science described in the preceding sections. Two potential classes of critics standout in this regard: those concerned about perceived informational gaps and those who object to these methods on moral grounds. Methodological critiques are all of a piece—they question the realism of the analysis, with the most common variety challenging starting assumptions for their disregard of a problem's complexity. Moral objections focus on the biases inherent in scientific methods. Cost-benefit analysis, for example, is criticized for its failure to consider non-market goods (e.g., aesthetic values), which systematically tips the scales against regulation.

It should be clear by now that all scientific methods can be criticized for what they leave out, as all models sacrifice realism for tractability. Yet, one of the standard criticisms of scientific methods is that they disregard so called "soft variables," that is variables that are difficult to measure or quantify. According to this view, the rhetorical power of quantitative assessments "dwarfs soft variables" and biases environmental policies in favor of factors, particularly standard economic ones, that can be most readily measured.[34]

This characterization is perfectly accurate so far as it goes. However, it ignores an important point, namely, that scientists' inability to represent complex systems accurately is not unique their methods.[35] Verbal representations may be equally deficient or even less effective than standard scientific methods. Similar to conceptual framework generally, "the problem of choosing the model is that of choosing the human point of view. One is going to be driven insane if one tries to find a nonhuman point of view from within a hopelessly human problem."[36] Blanket objections to ignoring soft variables risk being little more than demands for an unattainable nonhuman point of view.

This is not to say that scientific and linguistic methods are interchangeable. Clearly, there will be times when it is more appropriate to describe things using words than numbers, and vice versa. My point here is only that if the characteristics of an environmental problem make it difficult to represent using scientific methods, these same impediments will often make it hard to reduce them to a tractable verbal form. Put another way, scientific laws and verbal metaphors are both reliant on the existence of stable patterns and associations; without them, nature—like history—simply becomes "one damn thing after another."

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[34] Laurence H. Tribe, *Policy Science: Analysis or Ideology*, 2 PHIL. & PUB. AFFAIRS 69, 97 (1972); Wagner, *supra* note 5, at 122.

[35] Donald N. McCloskey, *History, Differential Equations and the Problem of Narration*, 30 *HISTORY AND THEORY* 21, 35 (1991).

[36] *Id.*

I worry that those of us engaged in debates over environmental policy frequently fail to take this next step to examine whether the alternatives to existing scientific methods do any better or, at the very least, that they offer a different perspective on the issues that is sufficiently coherent that it can be used meaningfully to inform decision making processes.

Moral objections clearly overlap with methodological ones. To the extent that a methodological gap implicates an important social value, it will also raise significant moral questions. Moral considerations thus may necessitate methodological compromises, such as sacrificing predictive accuracy to align a model with social values. Three points are relevant here. First, balancing qualitative factors (e.g., precision, generality, accuracy) is a routine part of scientific work, as models are context dependent, not absolute. Second, the fact that—in *aggregate*—a model may be more accurate is of little value to those for whom it gets things wrong when it counts most.[37] Third, the predictive accuracy of environmental models is typically modest, so a further loss in accuracy may be of marginal significance.

These observations suggest that there is room in scientific methods to accommodate moral considerations. Insofar as they do not demand information or knowledge that is inaccessible, social values can be treated as an additional dimension of the context and questions that already inform the development of scientific models. Clashes will nevertheless persist where scientific theories and evidence are misaligned with prevailing social values, although it may be the case that no systematic means exists for factoring certain values into a decision.

The challenges posed by environmental problems put policymakers in a seemingly paradoxical position. By undermining the apparent objectivity of science and foreclosing simple benchmarks for trustworthiness, they deprive environmental science of the authority and rhetoric needed to sway public opinion. An important implication of the model of science presented here is that this dilemma is illusory, for the only way to take advantage of environmental science is to accept its contingent nature. On balance, it is far better to accept the loss of this dubious rhetorical authority for a much more expansive—and human—view of scientific methods.

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[37] This is the key point to appreciate because evaluating methods is not simply a question of whether bias exists, as all models or methods will be biased in some way. Beyond the actual magnitude of the bias, which often will be difficult to determine, the more important point is that not all biases are created equal. Tribe, *supra* note 34, at 101.