

Realism and Relativism in Policy Analysis and Evaluation

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Abstract

Errol Morris' recent critique of Thomas Kuhn's philosophy of science is relevant to policy analysis and evaluation, indeed to public affairs more generally. How can scientific research designed to assist action in particular contexts bridge the gaps between Morris realism and Kuhn's supposed relativism? This paper provides examples of how "objective" analysis can respect and enhance differing "constructions" of reality.

Thomas S. Kuhn, or extensions of Kuhn, have raised hopes and hackles far outside his field of the history and philosophy of science.¹ “Like the choice between competing political institutions,” Kuhn wrote in *The Structure of Scientific Revolutions*, “that between competing [scientific] paradigms proves to be a choice between incompatible norms of community life... Each group uses its own paradigm to argue in that paradigm’s defense” (p. 94). Extreme Kuhnians apply these points more generally. Objectivity is an illusion; so are truth and progress. In public affairs, instead of talking about what will work given our world, the discussion quickly moves to radically changing that world. Scientific revolutions, Kuhn said; real revolutions, say the social Kuhnians.

“In these matters,” Kuhn noted, “neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced” (*Structure*, p. 151).

The big questions at stake, writes Errol Morris in his quirky, fascinating book *The Ashtray*,² include these: “Can we have knowledge of the past? Does science progress toward a more truthful apprehension of the physical world? Or is it all a matter of opinion, a sociological phenomenon that reflects consensus, not truth?” (p. 2). Morris sees a direct connection between Kuhn and postmodernism: “The essence of postmodernism, for me, is the social construction of meaning: If there are such things as reality or truth, they are, at best, socially constructed” (p. 20n12). And: “One could argue that the Kuhnian paradigm, whatever it might be, leads to intolerance. Intolerance toward any conception of science that is *not* sociological.” (p. 157).

Morris' book adds a personal dimension. When Morris was a graduate student, Kuhn threw an ashtray at him.³ He ordered Morris not to attend lectures by the visiting philosopher Saul Kripke, whose views often contradicted Kuhn's. Eventually, Morris says, Kuhn drummed him out of Princeton.

The personal dimension helps propel the reader through the book's sometimes arcane discussions of academic philosophy. Morris, a celebrated filmmaker, adds visual touches, too. In 185 pages of text, Morris intersperses 185 illustrations—photographs, paintings, diagrams. Footnotes are arrayed on the sides of the oversized pages. In an appendix is a “cast of characters” with epigrammatic, often humorous takes by Morris about everyone living and dead mentioned in the text.⁴ And Morris interviews famous philosophers about aspects of their work that is anti-relativistic. So, in the context of Morris' self-confessed vendetta against Kuhn's wrong thinking, the reader gets to meet among others Saul Kripke, Stanley Cavell, Noam Chomsky, and Hilary Putnam.

This article follows Morris's example, without airborne ashtrays or vendettas, using personal dimensions to advance the discussion. Policy analysis and evaluation exhibit the same tensions between realism and relativity: “speak truth to power” vs. “whose truth?” And, as it happens, variants of policy analysis and evaluation show how “objective” analysis can respect and enhance differing “constructions” of reality. These examples may help bridge the gap between realism and relativity more generally.

Model Explanations

In college I was a philosophy major. My senior honors thesis was about scientific explanation. How should scientists choose among conflicting explanations? According to philosophers such as Carl Hempel, prediction was the core. You *explained something* if you could use your explanation to *predict something like it*. Details intruded in many ways, including defining “something” and “something like it.”

My thesis advisor was the very same Hilary Putnam who appears in Morris’ book. In December of my senior year Putnam and I convened for lunch at a hamburger spot. I was excited but also troubled by a book I had just read, N.R. Hanson’s *Patterns of Discovery* (1958).⁵ Hanson pointed out that key terms in scientific discussions are theory-laden. The theory we hold influences the perceptions we make. Unlike Kuhn’s *The Structure of Scientific Revolutions* (1962), Hanson didn’t make statements from different theories “incommensurable” or render reality an illusion. Hanson did point to problems and challenges in understanding when a scientific explanation was convincing or should be convincing.

Putnam was not too helpful. “Physics is what physicists do,” was his aphorism. In other words, a *social standard* was at the core of scientific explanation, in the context of people trained a certain way, with certain values, and of course with big brains. To me, this left Hempel on the sidelines...and my senior thesis, too.⁶

Fast forward a few years. Now in graduate school, in a new program in public policy. One of the core courses was taught by Frederick Mosteller, the great statistician. I began to realize that the tools of probability and statistics helped address classic issues in philosophy about induction and causality. And what Mosteller called “confirmatory data analysis” could help a scientist decide between competing explanations.

Another core course was taught by Howard Raiffa, one of the founders of decision analysis. You list your choices. You sketch out possible paths after those choices, most of whose consequences are probabilistic. You assign value or utility to each possible outcome in the decision tree. You can gather additional information, which will probably alter your prior probabilities through more powerful statistical inference; then you can assess the utility of that better information. You can then decide, given the costs, how much new information to gather before deciding. And then, you choose the alternative that maximizes expected utility.

I was imbibing pragmatic methods for thinking about choice in general. The methods could be applied to choosing between alternative scientific explanations as well as alternative policies.

But Mosteller and Raiffa also went beyond choosing between hypotheses. Raiffa taught an advanced module based on his new research in decision theory with multiple objectives. Part of the task was to figure out with decisionmakers what those objectives and their weights might be.

Mosteller taught an advanced course on a new area of statistics called “exploratory data analysis” (EDA). I remember the thrill of reading a long paper by Mosteller and John Tukey called “Data Analysis, Including Statistics.”⁷ Also the surprises evoked by the three type-written volumes of Tukey’s still-unpublished *Exploratory Data Analysis*.⁸ In contrast to the econometrics we were learning in other courses—where you began with a theory based on economics to generate testable hypotheses—EDA let the data speak. Its visualization techniques showed when the usual assumptions of confirmatory data analysis were violated. Variables could sometimes be “re-expressed” to clarify interpretation. Robust estimation could sometimes do a better job of discovering regularities and their importance. And identifying exceptional performers and not just average tendencies had exciting relevance for both science and policy.⁹

Let policymakers speak, said Raiffa. Let data speak, said Mosteller and Tukey. Use analysis to help them do so.

“I Am Not a Kuhnian”¹⁰

Thomas Kuhn attempted to answer his critics in his book of essays *The Essential Tension*, published 15 years after *The Structure of Scientific Revolutions*.¹¹ He professed to be baffled at hostile reactions.¹² He *did* believe in scientific progress, namely “the accumulation of concrete and apparently permanent problem solutions is a measure of scientific progress” (p. 36). In his essay “Objectivity, Value Judgment, and Theory Choice” (a lecture from 1973), Kuhn laid out five of what he called “classic” and

“objective” criteria for choosing among scientific explanations—and he said he cherished them.

...accuracy, consistency, scope, simplicity, and fruitfulness—are all standard criteria for evaluating the adequacy of a theory. If they had not been, I would have devoted far more space to them in my book, for I agree entirely with the traditional view that they play a vital role when scientists must choose between an established theory and an upstart competitor. Together with others of much the same sort, they provide *the* shared basis for theory choice. (p. 322)

Kuhn likened the five criteria to values in a scientific community. Everyone in the community shared the values, but in applying them to messy choices, people could disagree. Kuhn showed that in crucial moments of theoretical uncertainty, it was unclear how well each competing scientific explanation fulfilled the five criteria. Certainly, he argued, no algorithm exists to determine mechanically which is best.

And he went further. In those times of scientific conflict, scientists brought their own values, tastes, and something like languages. A result was difficulty in translation, therefore in understanding each other (this later work seemed to back down from an emphasis on “incommensurability” between theories, a key point in the first edition of *The Structure of Scientific Revolutions*).

Here is his description of how learning a new theory may take place.

“Communication between proponents of different theories is inevitably partial, that what each takes to be facts depends in part on the theory he espouses, and that an

individual's transfer of allegiance from theory to theory is often better described as conversion than as choice" (p. 338).

"Despite the incompleteness of their communication, proponents of different theories can exhibit to each other, not always easily, the concrete technical results achievable by those who practice within each theory. Little or no translation is required to apply at least some value criteria to those results" (p. 339).

And here is how Kuhn concludes the essay:

However incomprehensible the new theory may be to the proponents of tradition, the exhibit of impressive concrete results will persuade at least a few of them that they must discover how such results are achieved. For that purpose they must learn to translate, perhaps by treating already published papers as a Rosetta stone or, often more effective, by visiting the innovator, talking with him, watching him and his students at work. Those exposures may not result in the adoption of the theory; some advocates of the tradition may return home and attempt to adjust the old theory to produce equivalent results. But others, if the new theory is to survive, will find that at some point in the language-learning process they have ceased to translate and begun instead to speak the language like a native. No process quite like choice has occurred, but they are practicing the new theory nonetheless. Furthermore, the factors that have led them to risk the conversion they have undergone are just the ones this paper has

underscored in discussing a somewhat different process, one which, following the philosophical tradition, it has labeled theory choice" (p. 339).

Note how Kuhn uses the language of conversion. That is, a radical reinterpretation of one's world through a thoroughgoing change in worldview, with the characteristics of suddenness and a satisfying "aha!"

In the preface to *Essential Tensions*, Kuhn describes his own conversion as a young scientist, fifteen years before the 1962 publication of *Structure*. He was studying Aristotle. He was surprised by how clumsy, how weird, were Aristotle's statements about the theory of motion, especially in contrast to Aristotle's subtlety and lasting wisdom in matters of morality. How could this be, Kuhn puzzled. The answer came to him in a flash of insight. "I have to read Aristotle as if I were inside his time and place, indeed as empathizing with his whole way of looking at the world" (my paraphrase). When Kuhn did so, suddenly what seemed absurd made sense, in terms of the way "science" in Aristotle's time talked about and experimented with the physical world. *Talked about*: "qualities" and "changes in qualities" were a preoccupation. *Experiments*: they were mostly in one's head, or in everyday experience.

Kuhn draws useful conclusions. "When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer...then you may find that more central passages, ones you previously thought you understood, have changed their meaning" (p. xii). Before dismissing someone's views or theories, try to understand

where they are coming from. Therefore, *translations* deserve more attention, both in understanding the language-like qualities of theoretical frameworks and in appreciating how learning and change may nonetheless occur.¹³

These points resonate more generally. In a world of diverse people, ideologies, religions, and the like, it is important for understanding what they say and do to try to inhabit their worlds and worldviews. And yes, in many senses people's theories, preferences, and customs are socially determined. They may not be *incommensurable*, but issues of translation and mutual comprehension are salient.

Noting these points does not imply that objectivity is illusory or scientific progress is impossible. Back to Kuhn's aside: "the accumulation of concrete and apparently permanent problem solutions is a measure of scientific progress." In an appealing passage, Kuhn admits he can't explain scientific progress even though he acknowledges it:

Even those who have followed me this far will want to know how a value-based enterprise of the sort I have described can develop as a science does, repeatedly producing powerful new techniques for prediction and control. To that question, unfortunately, I have no answer at all... The lacuna is one I feel acutely, but its presence does not differentiate my position from the tradition (pp. 332-333).

And recall Kuhn's five criteria for assessing a theory: accuracy, consistency, scope, simplicity, and fruitfulness. Prediction matters, and it matters so to speak across

theories. When predictions fail—when “anomalies” occur in one’s theoretical expectations—they can trigger thoughtful reconsideration. (Sometimes.)

So, reading Kuhn this way, we can acknowledge both

(1) The “social construction” of theory and how understanding another theory is abetted by putting oneself so to speak inside its construction—its epoch, its *problématique*, its protagonists.

(2) The push and pull on theories of predictions about a real world that people inhabiting different theoretical constructs can jointly appreciate.

Policy Analysis and Evaluation

The issues Kuhn and Morris “debate” are even more pronounced in policy analysis and the human and social sciences. Here objectivity and reality, truth and progress, are even more contentious. Ways of life may seem “incommensurable.” And yet, at least for issues we care about, we talk about impacts, consider costs, cite social benefits. We allude to evidence as if it were, well, *true*. How can we negotiate between taking a theory or worldview as socially constructed AND at the same time discovering what works where and when, in what senses, for whom?

Kuhn asked us to go beyond thinking about social scientific progress in terms of a choice among theories. What about a choice among policies? Instead of policy analysis as the science of choice, might we think bigger and better? And more usefully?

In my junior year of college, I began studying economics, including a remarkable course on game theory by Thomas C. Schelling. In this and my senior year seminars with the economist Richard Zeckhauser and the philosopher John Rawls, I discerned that game theory might help address classic issues in philosophy, such as Kant's generalizability problem and the conflict between act- and rule-utilitarianism. Then in graduate school, I had the good fortune to study again with both Schelling and Zeckhauser and to be a teaching assistant for Rawls. Let me dilate here on Schelling, who later won the Nobel Prize in economics. I believe he can help us recast policy analysis—and perhaps go beyond some of the brouhaha around Thomas S. Kuhn.

Schelling looked at big issues. Foreign aid. Nuclear deterrence. Racial discrimination. Organized crime. Nonviolence. The value of a human life. Addiction. Global warming. In every case, Schelling immersed himself in the details of real problems and real choices, even as he provided models of general applicability.

He was fascinated by both concrete examples and abstract theory. "In my own thinking," he wrote, "they have never been separate. Motivation for the purer theory came almost exclusively from preoccupation with (and fascination with) 'applied' problems; and the clarification of theoretical ideas was absolutely dependent on an identification of live examples."¹⁴ In the long, discussion-paper version of his essay "Binary Choice with Externalities," an index shows readers where to locate the many examples he uses along the way because, Schelling noted, they are what many readers most want to find.¹⁵ And he also sought enriching principles, and "principles rarely lead

straight to policies; policies depend on values and purposes, predictions and estimates, and must usually reflect the relative weight of conflicting principles.”¹⁶

Through the combination of simplifying theory and elegant example, Schelling forced his readers to realize that there are not one but a multiplicity of, say, military strengths, public goods, types of discrimination, nonviolent behaviors, ways to value a human life. “My conjectures,” he says in his analysis of various kinds of organized crime, “may at least help to alert investigators to what they should be looking for; unless one raises the right questions, no amount of hearings and inquiries and investigations will turn up the pertinent answers.”¹⁷

“I am drawing a distinction, not a conclusion,” Schelling writes, prototypically, in an article on corporate governance. He goes on to distinguish standards that impose costs from those that do not, costs arising from an act from those prompted by the fear of that act, wanting to do the right thing from figuring out what the right thing is, discouraging what is wrong from doing what is right, and the firms of economic abstraction from businesses as “small societies comprising many people with different interests, opportunities, information, motivations, and group interests.”¹⁸ The result, as so often with his work, was a new, deeper understanding of a big, important subject.

Schelling assimilated various costs and benefits and put them in context. Take climate change.

What is in dispute is its magnitude over the coming century, its translation into changes in climates around the globe, and the impacts of those climate

changes on human welfare and the natural environment. These are beyond the professional understanding of any single person. The sciences involved are too numerous and diverse. Demography, economics, biology, atmospheric chemistry, oceanography...

—and Schelling lists a dozen more fields before saying “and that is not all.”

But he goes on: “There are expert judgments on large pieces of the subject, but no single person clothed in this panoply of disciplines has shown up or is likely to. So, I venture to offer my judgment.”¹⁹

Beyond Choice to Enrichment

In 1985, Schelling reviewed “the non-accomplishments of policy analysis” in fields from defense to energy to health to education. Policy analysis as customarily practiced had made so little difference because the usual paradigm was wrong.

If policy analysis is the science of rational choice among alternatives, it is dependent on another more imaginative activity—the invention of alternatives worth considering...

The point I would make is that policy analysis may be doomed to inconsequentiality as long as it is thought of within the paradigm of rational choice...

[P]olicy analysis may be most effective when it is viewed within a paradigm of conflict, rather than of rational choice ... Analyzing the interests and the

participants may be as important as analyzing the issue. Selecting the alternatives to be compared, and selecting the emphasis to be placed on the criteria for evaluation may be what matters, and the creative use of darkness may be as much needed as the judicious use of light.²⁰

What is the paradigm of policy analysis that Schelling rejected? He didn't say so, but consider the decision tree mentioned earlier. The policy analyst is given the utility function and alternative choices. The analyst then assesses the likely effects of the various actions. The analyst calculates which alternative maximizes utility, and from a prescription for action is derived.

This rejected paradigm conceives of the analytical problem as the leap from givens to prescriptions, for the "if" to the "then." This conception borrows from economics. Under idealized assumptions, economic science is able to derive powerful statements about optimal courses of action. Seduced, the analyst may accept a lot of unrealistic restrictions on the "if" for the thrill of an unassailable "then." But as Schelling pointed out, in real policy making the intellectual problem is often a different one: how to discover, how to be more creative about, the objectives, the alternatives, and the constraints. In other words, how to understand, expand, and enrich the "if."

The rejected paradigm says that the policy maker's problem is deciding among many given courses of action. Schelling's version turned this around. The problem is understanding, indeed generating, the objectives and the range of alternatives. Once

policymakers have done that, they usually do well at making decisions. They are already pretty good at the “then” part; they may need help on the “if.”

Schelling recast the predominant paradigm of policy analysis. He was an enricher of the “if,” a catalyst for one’s own creativity. In what he wrote and how, he was aware of the importance of intangibles like perceptions, inclinations, and will—in the policy maker and in the reader as well.²¹ Policy analysis in the Schelling style tries to unpack the concept under discussion, even an emotively loaded one; one disaggregates and reclassifies. One approaches a sensitive subject by using a simplifying theory to obtain, not an optimizing model under restrictive assumptions, but a framework that stimulates the creativity of policy-makers and managers in their varied and unique circumstances. One supplies examples to clarify theory and inspire.

So, how can scientists and policy analysts kindle “another more imaginative activity—the invention of alternatives worth considering”? Recall Kuhn again. He recommends entering as best we can into the world of the scientists he studied—for social and human scientists, into the world of specific people and settings. We should try to understand their goals and constraints. Learn from salient examples of confusion and conflict. See how change involves both a confrontation with “concrete technical results” and something like learning a new language. These points seem to me applicable to policy analysis as well, as I hope one final autobiographical vignette may illustrate.

Learning from Success

In the summer after my first year of graduate school, I worked for three months in Peru's Institute of National Planning. The revolutionary government wanted to evaluate the foreign aid it received. Over 200 aid projects were underway. They ranged from rural roads to immunization to environmental protection to the development of the fishing industry. One project enumerated the monkey population in the Amazon.

The Minister of Planning explained what he was seeking. "We want to tell the donors what we want and need, not just take what they are giving. Last week, someone from UNIDO was here and said, 'Would you like this project? We have funding for it. But if you don't, we'll take it to Ecuador.' We need evidence to be able to say, 'This kind of project works well, this kind doesn't—and given our national plan, here's what we'd like you donors to do.'"

Sure, I said in effect to the Minister of Planning. Can do.

After my graduate courses in welfare economics, statistics, and modeling, I imagined answering the Minister's question this way. We would begin with a social benefit-cost analysis of each project. Then we would create a multivariate statistical model with the benefit-cost ratio on the left-hand side and, on the right-hand side, variables about sector, donor, project design, budget, degree of popular participation, and who knows what. Something like equation (1):

$$(1) \text{ Social } b/c = f(\text{project characteristics, sector, donor, budgets, ...})$$

Our office in the Institute of National Planning had only three professionals and few funds, but we got to work. We examined the files on each project. There were “evaluations” but only in terms of this happened then or this money went there. We found virtually no data on social benefits and costs.

Distraught, I sought in Peru’s newly published national plan its social welfare function. To my surprise, nothing there corresponded to the mathematical constructs I had been learning in my economics courses. After two weeks, I met again with the Minister of Planning and asked him about Peru’s social welfare function. His response: “Social welfare what?” (my translation). After a couple of weeks, I realized the dream equation (1) was more like a nightmare.

And so, I wrote to Frederick Mosteller. I described the analytical task and its impossibility, and I implicitly laid that impossibility on the model of policy analysis and evaluation I thought I had been learning from him and the other professors.

He wrote back and said, in effect: “People can never agree on what benefits and costs are. But they can and do agree on specific examples of outrageous success and outrageous failure. Find these among your projects. Study them. Compare them. Share your results, and learn some more.”

We followed his advice. My Peruvian colleagues and I went from ministry to ministry, asking for outstanding examples of aid projects that had succeeded. (We left aside outrageous failures.) Eventually, we studied six of the successes. At the same time, we put together data on all the projects—how much money was being spent each year

in which ministries and from which donors. And we reviewed academic hypotheses about why some sorts of foreign aid projects work better than others.

At the end of the summer, the Minister of Planning called a meeting. He invited the ministers and the donors. He asked me to facilitate. We brought a stack of printouts with all the data on all the projects, and we presented a summary. The participants had received brief descriptions of six successful projects, which we quickly reviewed. Then I asked the participants, “Why do you think these projects succeeded?”

One of the ministers said local participation was key. But another minister cited a project that had failed despite participation. One of the donors related his experiences in other countries with participation of various kinds.

Soon participants were in an animated, full-blown discussion of what works where and why. Peruvian wisdom and experience were evoked, and so was international knowledge. Every once in a while, I would ask a question based on the development literature. By the end of the session, many good ideas had been shared, and there was even some movement toward priorities for the development projects Peru needed next.

Frederick Mosteller’s advice succeeded, in several senses. Many donors were impressed and subsequently cut the Peruvians more slack in deciding what projects would be funded and how the projects could be structured. Ministers worked better with donors and better together. One minister told the Planning Minister it was the best cabinet meeting they had ever had. The head of my office got promoted and then was plucked away by the United Nations Development Programme, which two years later

asked him (and thankfully me, as a consultant) to replicate this process in two Central American countries.

Note that my dream study never happened: I did not provide anything like equation (1) to say what worked where in Peru. But a combination of data about all the projects plus examples of success plus a room full of expertise led to creative thinking about what works where. The participants ended up with better knowledge, better relationships, and the beginning of collaborative problem solving.

Examples can illustrate principles, pedagogically anyway. But what do examples show or prove? They can disprove a universal (“all swans are white”). In my experience, the universal is sometimes “there’s nothing you can do about that issue” or “it’s impossible to get a government agency to collaborate with a private business” or “incentives don’t work in the public sector.” Confronted with a successful example, we may react with “I never would have thought of that. And that makes me wonder about my situation.” We may be inspired: “Wow, she did that! And look how she emphasized this, put this other thing aside, dismissed her past failures this way...”

The point is far from proving a theory or deducing that because of such-and-such a model, you should do this. It’s not a science of choice. Not testing hypotheses. Not a declaration of truth and consequences.

Theory as Useful and Theory as True

Think for a moment of theories as mental models. Metaphors are useful to understand what mental models do. A lens. A map. A tool. They affect what we “see” in many senses, and they affect how successful we are in coping with challenges and opportunities.

Kuhn was once asked if his own ideas were true or not.

“Look,” Kuhn responded with even more weariness than usual; obviously he had heard this question many times before. “I think this way of talking and thinking that I am engaged in opens up a range of possibilities that can be investigated. But it, like any scientific construct, has to be evaluated simply for its utility—for what you can do with it.”²²

Some theories have wide utility. Others are highly context- and individual-dependent. A theory can be false and yet useful, to you and to society. And a theory can be true and yet harmful. Using a theory can have negative, unintended consequences.

These points conflict with a second notion of a good theory, namely a true theory. What a true theory unambiguously states—its words and concepts and mathematical relationships—corresponds to “reality.” What is reality? That which exists regardless of your or my beliefs about it.

A challenge in both the philosophy of science and in the applied human sciences is how to square these different approaches to and uses of theory. The issues are

complicated by heterogeneity and backwardness, scientifically and in human conditions. People's identities are involved; hurt feelings and worse are at risk. Translation is difficult, misunderstanding is easy.

One idea is to force ourselves (and others) to understand and apply different mental models. Let me give an example from public policy analysis.

Any policy implies a theory of change: these inputs (rules, procedures, treatments...) given this context will have such-and-such effects for these subjects. These are statements about agreed-upon facts. For example, an increase in foreign aid will stimulate economic growth rates in recipient countries. Or in education, a lower student-teacher ratio will raise achievement test scores. These can be considered objective questions; well-known if sometimes controversial analytical techniques can answer them, within bounds. But when those answers emerge, especially if they have consequences, different mental models emerge to "explain" them.

Imagine (not unrealistically) that an evaluation finds that increases in U.S. foreign aid budgets did not lead to increases in economic growth in recipient countries. Suppose, plausibly, that research in California public schools discovers that smaller class sizes are not associated with increases in student achievement scores. In these and many other policy areas, people almost reactively recoil to one of four competing "scientific explanations" (with apologies to Hempel).

(1) The Conservative Explanation

What I will call in this context the conservative mental model questions the validity of the theory of change for this setting or population. “With those people (students, patients, countries...), these inputs (development aid, more teachers...) do not lead to improvements in those outcomes.” The conditions belie the assumptions of the policy.

(2) The Liberal Explanation

The liberal mental model does not question the theory of change for “those people.” The problem is in the particular mix of inputs. Foreign aid will work, but it has to be done differently. Reducing class sizes obviously helps, if we can only get the right kinds of teachers matched with the right schools and students. We need more experiments and evaluations.

(3) The Professional Explanation

The professional mental model questions the outcome measures. What do you mean, “disappointment”? Your outcome measures are incomplete and biased, and your expectations are unrealistic. For example, economic growth is at best a partial measure of development as we professionals see it. And achievement test scores—we educators care about so much more for our students. Moreover, these are terribly difficult problems. Only we professionals can discern exactly what inputs will lead to all sorts of

qualitative and interacting “outcomes.” Only we can adjust inputs to varying local conditions and populations.

(4) The Radical Explanation

The radical mental model questions the theory of change in a different way. Your “program logic” is not really what is going on. For example, schools are not designed to raise academic achievement; they are designed to teach compliance, obedience, and one’s proper place in the so-called meritocracy. Foreign aid is not motivated by development outcomes. It has a macropolitical agenda—vote for us at the UN, help us against the terrorists and the drug merchants, give us access to your resources. Those undertaking foreign aid have personal and bureaucratic agendas: let me get promoted for getting an impressive-sounding project underway (where “me” can be someone in an aid agency or a local government).

Our being aware of these four varieties of explanation can have positive results. We may recognize that we tend to rely on one or another, and that this reliance has limitations as well as economies. We may see that each explanation has a point. We may admit that each may be valid in one or another policy domain, population, or setting.

Bracketing our preferred explanatory model and forcing ourselves to consider others may lead us to ask new questions. And the answers may help us design and implement better policies.

More generally, opening our minds in this way involves four steps:

1. Realizing that we may prefer a particular mental model in the sense of lens, map, and tool. Understand the uses of our models. And their possible misuses.
2. Appreciating other models on their own terms. What do they say and mean? What are their strong features, in our outside view and in the views of insiders (and other, different outsiders than us)? What is their intended range of application? What seem to be their blind spots?
3. Playing a choice or issue through many mental models. Hedgehogs have one big mental model, foxes have many. Be foxes, at least for a moment.
4. Considering what Kuhn called “impressive concrete results” can create opportunities for new thinking. What are the data about inputs and outputs? What are the salient examples of success and failure? Here, as Kuhn says, there is a role for classic science; and, as he emphasizes, the learning may be as difficult as learning a language, perhaps as profound as experiencing a conversion.

How the learning takes place matters. What uses and misunderstandings lurk?

Whose identities and status may be affected by them? And—who has to act upon them, if changes are to occur creatively and sensitively to context? Enter here a process I call “convening.”

Convening

Many policy problems have too many causal loops and intervening variables to infer that what works in one setting can be transferred straightaway to a seemingly similar problem in a quite different setting. A blueprint is unlikely to succeed. Speaking analytically: we often face theory uncertainty, measurement uncertainty, and modeling (or econometric) uncertainty, coupled with numbers of countries and ministries and cities and projects that are too small to enable us to explore potentially important interaction effects among policies and contexts

What to do? Locals have tacit and explicit knowledge about local settings. But they may lack access to some of the best data and techniques of data collection to calibrate their challenges. They may not be familiar with examples of what has worked elsewhere, not so much to copy as to inspire. They may benefit from exposure to models and theories that can be used to suggest frameworks for policy analysis. The challenge becomes how to combine forces: how to bring what they know best (local contexts and possibilities) with what outsiders may offer (facts, examples, frameworks).

The particular process I call “convening” tries to bring together these capabilities in a safe space through a pragmatic process. Those convened have different if overlapping objectives, different if sometimes overlapping capabilities, and different if overlapping information about the state of the world and about if-then relationships. The stakeholders are strategically connected, in the sense that what one party does often affects the outcomes of what other parties do. They are not fully aware of each other’s

objectives, capabilities, or information sets; they do not fully understand their strategic interrelations. Convening combines their strengths and inspires them to address their challenges with new information, examples, and frameworks.

This process provides stakeholders with:

1. Data, especially data that help people “get on the same page” about the nature of the problems. Data-rich discussions help build trust, particularly about controversial issues.
2. An example of success in similar problem areas, which spotlight what was done and how. The example is conveyed through a part A, part B case study. Participants analyze the issues and make recommendations, then learn what happened, then discuss.
3. A framework for understanding goals, alternatives, if-then relationships, and strategic interdependence. The framework can provide a new way of framing the problem.
4. An imaginary news story of success five years hence. Participants read it aloud, then ponder what steps could lead from now to then.

The data help everyone contextualize the challenges.

The success story from another country provides inspiration.

The framework conveys principles.

The imaginary news story stimulates creative problem-solving.

In short, convenings engage:

- Key decisionmakers and stakeholders, sometime from outside government,
- In a safe setting (no recordings, Chatham House rules, etc.),
- For a day, sometimes more,
- Away from the office,
- With the goal of stimulating new, practical ideas for improvement.

More on Mental Models

The psychologist Brian Little writes, “Generally speaking, the more numerous the lenses or frames through which you can make sense of the world, the more adaptive it is. Having too few constructs or insufficiently validated ones can create problems, particularly when life is moving quickly and you are trying to make sense of it.”²³

Note both “too few” and “insufficiently validated.” The former acknowledges that theories are multiple and usefully multiple. The latter acknowledges that theories can vary in their validity.

Kuhn said something similar about interpretations of a scientific theory. “First, there are many ways to read a text, and the ones most accessible to a modern are often inappropriate when applied to the past. Second, that plasticity of texts does not place all ways of reading on a par, for some of them (ultimately, one hopes, only one) possess plausibility and coherence absent from others” (*Essential Tension*, p. xii).

We have talked about learning, even across theories, political systems, and cultures. More recently, I have become interested in heroes and how we might learn from them. My interest is different from reading biographies of successful people. Most of the

heroes in myth and religion are reconstructed. (Ambrose Bierce defined a saint this way: “A dead sinner, revised and edited.”) And yet, it has been argued by people as different as psychologists, historians, and scholars of ancient Greece that many heroes follow a pattern. Shall we say, a model? Only at a very high level of abstraction. The pattern is this. Heroes struggle. They find or receive a calling. Their calling is associated with a big insight, which combines a personal challenge they face with a problem all or most humans face. And then heroes share and serve...in contrast to putting up their feet and enjoying their wisdom in leisure.

As I developed these ideas, I tried to present them with two overlapping but distinct points. First, we can be inspired by heroes, even if we are ordinary people. Second, we can apply the heroic pattern in our lives, including suggestions about how to find a calling, how to obtain and manage big insights, and how sharing and serving might be kindled through gratitude and then become love.

The ligatures of similarity with the earlier points are the combination of a story—a case study of success, if you will—and a model—a framework that draws attention to key principles in ways that can guide us on different problems, perhaps even in our own life choices. The logic is also similar. It is not deducing action from a true theory, as in my college understanding of scientific explanation. Nor is it what the anthropologist Clifford Geertz decried as “size-up-and-solve social science.” Not the powerful if-then of an economic model: “if you assume X, Y, and Z, then I can prove the optimal policy choice is alternative B.” The communication is not scientific in the usual senses of that

word. It is personal and perhaps artistic. The desired response is something like “aha!”—perhaps even what Thomas S. Kuhn described as a conversion.

Endnotes

¹ Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962)

² Errol Morris, *The Ashtray (Or the Man Who Denied Reality)* (Chicago: University of Chicago Press, 2018).

³ Morris felt Kuhn's hostility first-hand. "If paradigms are really incommensurable, how is history of science possible?" the graduate student Morris asked his Professor Kuhn. "Wouldn't we be merely interpreting the past in the light of the present?"

I thought, "How can any of us know what Maxwell was thinking?"

I said, "...except for someone who imagines himself to be God?" A megalomaniac who creates a philosophy but doesn't believe he has to abide by it.

He put his head in his hands and muttered, "He's trying to kill me. He's trying to kill me."

Then he looked up and threw the ashtray at me. And missed.

Morris, *The Ashtray*, p. 13.

⁴ For example, here is Isaac Newton: "Often preoccupied with a theory of universal gravitation, as well as other sundry things, when he proposed marriage to his putative beloved, he took her hand, essentially forgot what he was doing, and used her thumb to

tamp down the ash in his pipe. Needless to say, he ended life as a bachelor. (This much repeated story, like that of Newton's dog, Diamond, may be apocryphal. Nonetheless, both deserve to be repeated)." (Morris, *The Ashtray*, p. 182)

⁵ Norwood Russell Hanson, *Patterns of Discovery: An Inquiry Into the Conceptual Foundations of Science* (Cambridge and New York: Cambridge University Press, 1958).

⁶ In Morris' book, Putnam says, "What we count as being the essence of something—and I still would say this—depends on a tradition of scientific investigation. Kripke disagreed: 'No, the science is about discovering the essence. You've got it the wrong way around'" (Morris, *The Ashtray*, p. 59). Like Morris, my naïve thinking was more aligned with Kripke's position, even though Kripke's book wasn't published until 1977.

⁷ Frederick Mosteller and John W. Tukey, "Data Analysis, Including Statistics," in Gardner Lindzey and Elliott Aronson, eds., *Handbook of Social Psychology, Vol. 2*. (Reading, MA: Addison-Wesley, 1968).

⁸ It finally appeared in 1977 in one volume of 688 pages and a shipping weight of almost 2½ pounds. Reading, MA: Addison-Wesley.

⁹ Later, in my doctoral dissertation and a number of subsequent papers, I focused on exceptional performers. How could one identify unusually effective schools, say, after taking account non-school factors that affect performance? If one identified them, what were their characteristics? How might policymakers and practitioners learn from them?

¹⁰ “I once attended a meeting of historians at which the disciples of Kuhn were presenting an extreme and exaggerated version of his views. Kuhn interrupted them by shouting from the back of the hall with overwhelming volume, “One thing you people need to understand: I am not a Kuhnian.” Freeman Dyson, *The Scientist as Rebel* (New York: New York Review of Books, 2006), p. 208.

¹¹ Thomas S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977).

¹² “With respect to theory choice, I have never thought my departures large and have been correspondingly startled by such charges as ‘mob psychology’” (Kuhn, *The Essential Tension*, p. 336).

¹³ He concluded the preface: “Proponents of different theories (or different paradigms, in the broad sense of the term) speak different languages—languages expressing different cognitive commitments, suitable for different worlds. Their abilities to grasp each other’s viewpoints are therefore inevitably limited by the imperfections of the processes of translation and of reference determination. Those issues are currently the ones that concern me most, and I hope before long to have more to say about them” (Kuhn, *The Essential Tension*, pp. xxii-xxiii).

¹⁴ Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1960), p. vi.

¹⁵ Thomas C. Schelling, "Hockey Helmets, Concealed Weapons and Daylight Saving: A Study of Binary Choices with Externalities" Discussion Paper No. 9 (Cambridge, MA: Kennedy School of Government, 1972).

¹⁶ Thomas C. Schelling, *Arms and Influence* (New Haven: Yale University Press, 1966), p. vii.

¹⁷ Thomas C. Schelling 1971. "What Is the Business of Organized Crime?" *The American Scholar* 40 (1971): 649.

¹⁸ Thomas C. Schelling, "Command and Control" in *Social Responsibility and the Business Predicament*, ed. James W. McKie (Washington, D.C.: The Brookings Institution, 1974), pp. 82, 30, 83.

¹⁹ Thomas C. Schelling, "Some Economics of Global Warming" *American Economic Review*, 82 (1992): 1-14.

²⁰ Thomas C. Schelling. "Policy Analysis as a Science of Choice," in *Public Policy and Policy Analysis in India*, eds. R.S. Ganapathy *et al.* (New Delhi: Sage, 1985), pp. 27-28.

²¹ A military example of this theme: "[W]e are necessarily dealing with the enemy's intentions—his expectations, his incentives, and the guesses that he makes about our intentions and our expectations and our incentives ... This is why so many of the estimates we need for dealing with these problems relate to intangibles. The problem involves intangibles. In particular, it involves the great intangible of what the enemy thinks we think he is going to do" (Thomas C. Schelling. 1964. "Assumptions about

Enemy Behavior” in *Analysis for Military Decisions*, ed. E.S. Quade (Santa Monica: The RAND Corporation, 1964), p. 216.)

²² John Horgan, “What Thomas Kuhn Really Thought about Scientific ‘Truth’” *Scientific American*, 23 May 2012. <https://blogs.scientificamerican.com/cross-check/what-thomas-kuhn-really-thought-about-scientific-truth/>

²³ Brian R. Little, *Me, Myself, and Us: The Science of Personality and the Art of Well-being* (New York: Public Affairs, 2014), ch. 1.