

**Thinking Like an Economist:
On Expertise and the U.S. Policy Process**

Elizabeth Popp Berman

The Occasional Papers of the School of Social Science are versions of talks given at the School's weekly Thursday Seminar. At these seminars, Members present work-in-progress and then take questions. There is often lively conversation and debate, some of which will be included with the papers. We have chosen papers we thought would be of interest to a broad audience. Our aim is to capture some part of the cross-disciplinary conversations that are the mark of the School's programs. While Members are drawn from specific disciplines of the social sciences—anthropology, economics, sociology and political science—as well as history, philosophy, literature and law, the School encourages new approaches that arise from exposure to different forms of interpretation. The papers in this series differ widely in their topics, methods, and disciplines. Yet they concur in a broadly humanistic attempt to understand how, and under what conditions, the concepts that order experience in different cultures and societies are produced, and how they change.

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Thinking Like an Economist: On Expertise and the U.S. Policy Process

Last week, Dani Rodrik opened his Thursday Lunch Seminar with a quote from the classic 1973 satire, “Life among the Econ,” by economist Axel Leijonhufvud. To jog your memory, it offers an imagined ethnography to point out the vast cultural gulf that exists between economics and the other social sciences. “[T]heir young are brought up to feel contempt for the softer living in the warmer lands of their neighbors, such as the Polscis and the Sociogs,” Leijonhufvud writes. “Despite a common genetical heritage, relations with these tribes are strained – the distrust and contempt that the average Econ feels for these neighbors being heartily reciprocated by the latter – and social intercourse with them is inhibited by numerous taboos.”¹

In the interest of historicizing this relationship a bit, I wanted to point out that this mutual mistrust dates back at least several decades before Leijonhufvud was writing. During World War II, the Office of Strategic Services, which was the precursor to the CIA, had a Research and Analysis Branch that brought together historians, economists, philosophers, political scientists, and others in support of the war effort. (Carl Kaysen, former director of the Institute, was one of those economists.)

Barry Katz, who wrote the history of the Research and Analysis Branch, describes its economists as being not terribly impressed by their colleagues from other disciplines. He wrote that they “were a restive lot, confident of the scientific foundations on which their own work rested and impatient with colleagues whom they regarded as mere fact-finders and whose approach they dismissed as literary, journalistic, or preoccupied with irrelevant historical detail.”² So the perception of economics as being a more scientific enterprise predates Leijonhufvud’s article considerably, and is, at a minimum, seventy years old. That’s a lot of history to overcome.

However, this story does have a happy interdisciplinary ending. While the economic approach remained distinct at the Research and Analysis Branch, Katz emphasizes that over the course of the war the different disciplines gained a broader appreciation of each others’ contributions, and he shows some of the ways that the economists of the Research and Analysis Branch carried the catholic approach that they adopted there throughout their careers, long past the war years.³ So there is hope.

I want to do two main things in this talk today. The first is to spend a little time setting up the motivation behind my project, and the puzzle it is trying to solve. I want to do that in part because I’m at a turning point in the writing, where I have plenty to say, but I’m no longer sure I’m still answering the same question that I started with. And I’d really like to get the collective feedback of the group here on that tension and possibly generate some ideas about how to resolve it.

I don't want to spend all my time talking about methodological issues, though, and so after I quickly set up that context, I'm going to focus mostly on one specific historical thread: how a particular group of economists and a particular set of analytical methods entered policymaking in the 1960s and became integrated into the policy process in the 1970s in ways that had lasting effects, some of them intended, others unintended. I will also use that history to make a couple of more general observations about some of the mechanisms through which economists and their tools can come to influence the policy process.

My interest in economists grew out of my first book, which started as my dissertation: *Creating the Market University: How Academic Science Became an Economic Engine*.⁴ In the book, I try to explain why U.S. universities, over the course of several decades, became much more involved in trying to use the scientific research conducted on campus to achieve specifically economic outcomes. This is partly about taking up activities like patenting, or faculty entrepreneurship, but it's also about coming to think of the purpose of science in a new way. In the past—forty or fifty years ago—people thought we did scientific research for defense purposes or to cure disease. If you were idealistic, it was for the purpose of expanding knowledge; it could even have been to solve particular problems in industry. But, we didn't see science as the motor for the economy.

In the late 1970s, policymakers really got on board with an idea that economists had been talking about for a while, but that was new to policy circles. This was the idea that technological innovation drives economic growth. Today this is a familiar rhetoric. Neal Lane, for example, a physicist and former director of the National Science Foundation, argued in a *New York Times* editorial in 2012 that we should vote for Obama, because he understands this connection, unlike, apparently, Mitt Romney. "Mitt Romney said in all three presidential debates that we need to expand the economy. But he left out a critical ingredient: investments in science and technology. *Scientific knowledge and new technologies are the building blocks for long-term economic growth—the key to a 21st-century economy,*' as President Obama said in the final debate."⁵

The purpose of my book was to explain how this new way of thinking led to policy changes that reshaped what universities did. But I also kept wondering about the role of economics and economists in this process. On the one hand, there was no question that ideas about the importance of technological innovation traced back to Robert Solow's growth theory of the late 1950s, and to the emergence of an economics of innovation in the '60s.⁶ On the other, those ideas only had effects because they were promoted in the '70s by people who saw them as serving their interests, and who did so in a political context in which policymakers were desperate to do something about the economy.⁷

The shift toward thinking about policy areas in terms of their economic role continued into the 1980s and it was not limited to science policy, as recent scholarship highlights. Jal Mehta's book, *The Allure of Order*, shows that, in this period, education policy started to be discussed in terms of its economic effects.⁸ William Gormley has made a similar case for policy on children's issues: while arguments for children's healthcare used

to rest on a moral imperative to provide healthcare for children, they came to center around the economic payoff of investing in healthcare for children.⁹ Mark Smith's book, *The Right Talk*, tracks a shift toward economic arguments in multiple policy areas including education policy, but also around tax policy and other domains.¹⁰

So the puzzle that I set out to explore is why this turn toward the economic took place. You could say that it was prompted by the economic conditions of the '70s, but it didn't really disappear once the economy improved in the '80s or '90s.

The shift was interesting in other ways as well. It was clearly linked to the language of economics, yet it didn't, for the most part, cause politicians to start making decisions that economists would approve of. It was associated with changes that are often characterized as neoliberal, but the economic arguments behind them often came from center-left technocrats who wanted to use government to solve market failures, not from Chicago proponents of free markets.

My motivation was normative as well. While I didn't want the project to be anti-economics or anti-economists, I was concerned that the dominance of the economics discipline in government contributed to a process through which economic goals came to displace other important policy goals. Thinking about education as a human capital investment, for example, is fine in and of itself, but if education is conceptualized only in those terms, it becomes hard to talk about its importance to democracy.

The project was, and still is, structured around case studies of three U.S. policy areas in which 1) a turn toward economic goals took place, and 2) economists played a visible role, though I do not assume it was a causal one. I planned to keep looking at science and technology (S&T) policy, which I already knew something about; and I also wanted to look at antipoverty and antitrust policies.

I did have a working story about what was going on. In each of these domains, you see economists in competition with some other group of relevant experts: natural scientists, and particularly physicists, in S&T policy; other social scientists in antipoverty policy; and lawyers, primarily, in antitrust policy. In each of these areas you can also see that economists and other experts hold competing conceptions of what the purpose of policy in that area should be. So, for example, social workers and sociologists tended to see antipoverty policy as trying to achieve political empowerment of the poor, or disrupting the culture of poverty, while economists tended to see it as trying to increase people's income, or their capacity to earn.¹¹ I imagined telling a story in which an economic style of thought became more central to policymaking as economists gained influence relative to other groups of experts who had other ways of thinking.

I haven't totally abandoned that framework for thinking about the project. But it has proven to be problematic in some ways. In particular, I thought that I would see more common patterns across the three cases. Instead, it turned out that the story of "economization" was very different in each of them, and economists played a unique and different role in each one. This made it harder to develop a common narrative about how economics helps to push policy domains toward more specifically economic goals.

I also, as I wrote, found it harder and harder to tell a story about how economists came to matter in particular policy areas without first telling a story about how economists became more important in government in general. And so I'm a bit caught between writing a book that tries to explain a particular outcome—how economic goals became more important in particular policy areas—and one that tracks how economists and their intellectual tools entered the policy arena and circulated throughout it, and with what consequences. That latter story seems really important right now, but it doesn't line up very well with my original conceptualization of the project. So in the rest of my time today I'll delve into one strand of this history through which economists enter policy in the United States, and then perhaps at the end you will have some thoughts about whether I should be answering the first question, the second, or possibly both.

* * *

The story I want to tell you is about how a family of analytical methods championed by a group of economists entered and circulated throughout the policy process in the 1960s and '70s. This is a loose group of methods that was talked about as systems analysis and the Planning-Programming-Budgeting System (PPBS) during the 1960s; later the language shifted to a variety of other terms including “policy evaluation,” and “cost-benefit analysis.” I'll refer to it as policy analysis, meaning quantitative analysis focused on using rational methods with roots in economics to optimize policy decisions. These methods are not *only* associated with the discipline of economics, but economists played a key role in moving them into policy, and economists have always been high-status participants in their use.

The family of methods I am calling policy analysis had its origins in the operations research that was conducted during World War II. Mathematical economists, working closely with applied mathematicians, developed methods to solve optimization problems. These were solutions to fairly well-defined tactical problems. For example, U.S. B-52s were laying mines in Japanese waters. Given that, how could losses be minimized without impairing the bombers' effectiveness? At what time of day do you fly? At what altitude? In what formation?¹²

After the war, operations research continued to develop, particularly at the RAND Corporation, the Santa Monica think tank which is probably known best for its representation in *Dr. Strangelove*. But while RAND is most closely associated with military strategy, it was also, as Herbert Simon put it, one of “the places...to see and to be seen” in postwar quantitative social science.¹³ Its Economics Department was a *Who's Who* of the discipline; RAND's employees and summer consultants in the '50s included Armen Alchian, Kenneth Arrow, James Buchanan, Robert Dorfman, Albert Hirschman, Tjalling Koopmans, Paul Samuelson, Thomas Schelling, Theodore Schultz, Robert Solow...the list goes on. To put it another way, RAND had more than its fair share of future Nobel Laureates.

While systems analysis, which developed out of operations research, was an interdisciplinary endeavor, RAND's Economics Department was, in many ways, its

intellectual home. Like operations research, systems analysis focused on quantitative methods and tried to optimize decision-making. But systems analyses focused on situations somewhat less well-defined than the mine-laying example given above. It tried to clarify options in situations where objectives might conflict, or the environment might be uncertain. So, for example, it might compare the costs and benefits of two possible weapons systems when there were still questions about how hard each would be to develop and then produce, what the range of military needs might be by the time they reached production, and so on.¹⁴

While RAND economists were optimistic about the potential of systems analysis to rationalize policy, they were quite aware that applying it would be challenging in practice. Over and over in the 1950s, RAND scholars wrote anguished internal papers about what they called the criterion problem. What, exactly, should RAND's systems analyses be optimizing for? What was the scientific answer? If anything was left out, the solution could be quite obviously wrong.¹⁵ An example, not from RAND, that nevertheless illustrates the point is the problem of the Stigler Diet, named after economist George Stigler.

Stigler asked how much of 77 different foods should be eaten by a 154-pound man so as to meet the recommended allowances of nine nutrients for minimal cost. This problem had a clear answer. Given the costs of food in 1939, he should eat the following each year: 370 lb. wheat flour, 57 cans evaporated milk, 111 lb. cabbage, 23 lb. spinach, and 285 lb. of dried navy beans.¹⁶

This solution demonstrates the gap in translation between “healthy diet at minimal cost” or, even more so, “healthy diet that people will be willing to eat at minimal cost,” and “meeting the recommended allowance of nine nutrients at minimal cost.” While being able to solve the problem was valuable, identifying the right criteria was key to a systems analysis that could succeed in the real world. Some thought the criterion problem could not be solved. Political scientist Charles Lindblom, who spent a summer at RAND in 1954, argued that

despite the mathematical sophistication...the criteria upon which RAND systems analysts based their conclusions were heavily value-laden and were typically selected according to the personal preferences of the analysts. The search for optimal solutions to complex policy questions was thus a trap since the more complex the systems analysis became, the more deeply biased the analysis became.¹⁷

RAND's economists remained optimistic, though. And in 1961, a group of them got the chance to put their theories into practice when President Kennedy appointed Robert McNamara Secretary of Defense. McNamara, who had himself worked to rationalize the Ford Motor Company with considerable success, was immediately drawn to systems analysis when he began to learn about defense questions. He hired Charles Hitch, chair of RAND's Economics Department, to serve as his comptroller, and Alain Enthoven

and Henry Rowen, both RAND economists, as part of the inner circle of Whiz Kids. They quickly began to implement what was known as the Planning-Programming-Budgeting System, or PPBS, also sometimes called program budgeting. This was a systems analytic approach to identifying military priorities, using those to develop programs, and then creating budgets that maximized the military's ability to meet centrally defined priorities.

While McNamara liked program budgeting partly because rational, quantitative techniques appealed to him, he also liked its political implications. PPBS required that budget decisions be tied to centrally defined goals. So in practice it was a method of centralizing control over the strong military branches. Thus, in addition to the political judgments that had to go into a systems analysis, the process of implementing the method had political effects of its own, since budgets were explicitly linked to McNamara's priorities under the guise of rationalization. And it was these political effects, in part, that encouraged PPBS to circulate beyond the Defense Department in the years that followed.¹⁸

In particular, McNamara's Whiz Kids worked closely with the Bureau of the Budget (BOB) to implement PPBS. The Budget Bureau was the executive agency that coordinated budgets for all the departments in the executive branch. It was itself going through a decade in which it was led by a series of PhD economists, and in 1964 they decided that program budgeting was the solution to another political problem: the problem of the Community Action Program.¹⁹

The Community Action Program was created by the Economic Opportunity Act in August 1964, as part of the War on Poverty. It aimed to democratize welfare policy by creating in cities organizations that would give poor people a political voice to express their needs. The idea was that the poor knew, better than any bureaucrat or social worker, what they needed, and would benefit most from having a voice in the political process.

Almost immediately, Community Action started causing political headaches for the Johnson administration. Most significantly, it was the Democratic mayors of big cities who were the objects of these new political demands the poor were making. The mayors were not happy that Washington Democrats were creating these problems for them. So the Bureau of the Budget almost immediately sought to regain control of the poverty program and rein in Community Action.

Both the director of the Budget Bureau, Kermit Gordon, and its assistant director, Charles Schultze, were PhD economists. Gordon had recently brought Henry Rowen to BOB from the Defense Department, where he had been implementing program budgeting. With Rowen, Gordon and Schultze developed a plan to bring PPBS to the Office of Economic Opportunity, which ran Community Action. Again, the explicit goal was not just to rationalize policymaking, though that was appealing, but to regain control.

This led to the creation of a policy analysis department in the Office of Economic Opportunity. Two former RAND economists, Joseph Kershaw and Robert Levine, were hired to run it. As had worked in the Defense Department, by centralizing and rationalizing budgetary decisions, they were able to regain control over the direction of the War on Poverty, and move it away from Community Action and toward their preferred

strategy, which was a negative income tax. The goal of the War on Poverty was shifting, from the more radical political empowerment of the poor, to the still ambitious but less politically threatening goal of eliminating income poverty.²⁰

While the Office of Economic Opportunity was one site that gained a program budgeting operation in 1965, it was far from being the only one. At the recommendation of the Budget Bureau economists, President Johnson signed off on an executive order that required all federal agencies to implement PPBS. Every department would establish a systems analysis office, which would develop program recommendations based on specific objectives and conduct cost-benefit analyses that would then make their way to the Budget Bureau for central budget decisions.²¹

While PPBS had a substantial impact in the Defense Department and in the War on Poverty, its broader roll-out was largely a disaster. Agencies responded with some combination of confusion and hostility.²² A report from the U.S. Geological Survey gives a sense of a typical agency reaction. It said that the agency:

was required to respond to [Johnson's] directive but, like many other Executive agencies, was ill-equipped to carry out such implementation. The [USGS] was handicapped by the lack of well-defined objectives which could readily be translated into plans amenable to Planning-Programming analysis. The agency was staffed, as well, by professional geologists and scientists without experience in economics, quantitative analysis, or the related disciplines needed for installing systems analysis....Its most difficult problem has been the identification of 'output', and measurement of that output's value to the users. Without estimates of the demands for the information produced by the Survey, calculation of benefits from the present activities of the Survey and economic comparison of these with alternative activities has been virtually impossible.²³

In 1970, President Nixon quietly killed PPBS.²⁴ But its influence extended well beyond its modest lifespan in several ways. First, while PPBS did not last, the offices established to carry it out did. PPBS installed quantitative social scientists, primarily economists, in high-level offices around the federal bureaucracy. The Department of Health, Education, and Welfare, for example, created the Office of the Assistant Secretary for Policy and Evaluation. It was first led by RAND and Defense Department economist William Gorham, and by the mid-1970s, long after PPBS was gone, it employed 150 people to do policy analysis.²⁵ Although the term "systems analysis" gradually fell out of favor, the family of methods that made it up—particularly, but not only, cost-benefit analysis—became an institutionalized part of the policy process.²⁶

Second, by requiring economic analysis, PPBS helped launch an entire new industry of policy research organizations. Economists were only one of several professions represented in these, but were typically the first among equals within them. In 1966, the

RAND Corporation diversified into social policy analysis; in 1968 the Urban Institute was founded; that same year Mathematica Policy Research was started in Princeton; the Manpower Research Development Corporation was created in 1974; and so on. This launched a whole new ecosystem that Alice O'Connor talks about as the poverty research industry, but that really applied to social policy much more broadly. It addressed not only welfare policy but education policy, health policy, urban policy, and other areas from a broadly economic perspective.²⁷

Third, the sudden acceleration of demand for economic policy analysis led to the creation of a new set of academic programs in public policy. There was already a discipline of public administration, which had a more practical, managerial focus, but the first public policy school was not founded until 1967. The new public policy programs were interdisciplinary but took their inspiration from economics, which has been called their “overarching intellectual framework” and their “dominant mode of thinking.”²⁸ Twelve public policy programs were founded between 1967 and 1972, mostly at elite institutions like Michigan, Berkeley, Harvard, Yale, and Penn.²⁹ They had a different curriculum from public administration programs, with a core focused on microeconomics and quantitative methods.³⁰ And economists often founded and led these new schools: Thomas Schelling helped develop the Kennedy School curriculum; William Niskanen, Berkeley's; Alain Enthoven, who was one of the Whiz Kids, Stanford's. Graduates of these programs found ready employment, given the increased demand for policy analysis, and, while they were certainly not economists themselves, they were shaped by a different sensibility than their peers who had come from the older public administration programs.³¹

Finally, beyond the executive branch, the advance of policy analysis there also provoked a compensatory reaction in Congress. The General Accounting Office, for example, really was an accounting organization until the early '60s. It focused on the financial auditing of government agencies. But in 1966, Elmer Staats, who had helped implement PPBS at the Budget Bureau, was appointed to lead the GAO, and he began shifting its focus toward program evaluation—that is, toward evaluating how cost-effective government programs were at achieving their stated goals.³²

Similarly, as Congress felt the need for more analytical capacity to counterbalance an aggressive White House, it established the Congressional Budget Office in 1974. The CBO's founding director was Alice Rivlin, a PhD economist who had led the policy analysis office at Health, Education, and Welfare, and who had written a book on systems analysis. She appointed as her deputy another RAND alumnus, Robert Levine, who had been one of those who introduced PPBS to the War on Poverty.³³ Rivlin intentionally designed the Congressional Budget Office as an organization that would not just provide budget numbers, but that would “help frame difficult policy choices by providing analysis of the budget implications of various program and policy options.”³⁴ That is, it would effectively conduct systems analyses. The CBO was, from its early days, the most economist-dominated of the congressional agencies.³⁵

Ranging even further afield, back in the executive branch, the deregulatory movement was starting to encourage more use of a cost-benefit approach by the mid-1970s. The deregulation story links several groups of economists, but most relevant here, it particularly promoted a more formal role for economic analysis of regulations.³⁶ President Ford required agencies to prepare economic impact statements for major legislative proposals. President Carter required cost-benefit analysis of regulations beyond a certain size. President Reagan required that *all* regulations be subject to cost-benefit analysis and established a central office to oversee the process. Each of these steps further institutionalized cost-benefit analysis more deeply into policymaking.³⁷

One challenge, of course, during all this was that while conducting policy analysis required economic expertise, even the best-intended efforts did not necessarily resolve political questions. Quite the contrary. This could be seen over and over again as policy analysis spread through the bureaucracy. The earliest evaluations of Head Start, for example, showed significant gains for the children served by the program. But they also showed a “fade-out” of those gains over the following years. This prompted a lively debate over the best way to evaluate government programs, but gave no easy answers about Head Start’s effectiveness.³⁸

The New Jersey Negative Income Tax experiment, which was a major effort run by Mathematica, ran into a variety of troubles. It gave cash to an experimental group while also following a control group of low-income families. The question was whether families would reduce their labor force participation if they were given additional income. Among other things, the Mercer County prosecutor subpoenaed Mathematica’s payment records because he suspected welfare fraud, and the head of Mathematica had to declare his willingness to go to jail rather than break client confidentiality. But even beyond these practical issues, the experiment did not answer the question once and for all of whether giving people money would lead them to stop working. It just led to a decade of arguments over what the experiment’s findings really meant.³⁹

Microsimulations proved equally unhelpful when they became part of the standard policy toolkit in the 1970s. Microsimulations estimated what the effects of various policy changes would be. So, for example, if a change was made to the food stamp program, how would that affect such things as the number of participants, the total cost of the program, or the behavior of those receiving food stamps, and so on. Debates over which model was better quickly took on political tones. Various models were accused of having political biases, and while honest analysts knew that the models provided very rough estimates, nonexperts often took the numbers at face value.⁴⁰ As one modeler later recalled, “It got to the point where I was scared to think that these guys [in Congress] were actually believing what I was telling them despite all the qualifiers we told them. It was really sort of awesome....The numbers took on a life of their own. It was really sort of frightening to me.”⁴¹

By the end of the 1970s, the systems analysis revolution had not managed to rationalize the budgetary or policymaking process, but it had created a whole new

infrastructure for a particular type of economic policy analysis. It included new academic programs and new research institutes, and it was institutionalized in new government organizations. In all of these arenas, economics was typically the dominant discipline, even though many non-economists were employed as well.

So what does this narrative tell us about how economics might matter to policymaking? And how, if at all, does it connect back to my initial question about how policy areas become more “economic,” meaning more focused on economic goals and outcomes as the legitimate targets of policymaking?

First, it should be pretty clear that this story does not imply that economics matters mostly because economists give advice that policymakers take, at least not very often. Nor, as the examples of Head Start and the negative income tax illustrate, does economics typically provide definitive answers that can resolve policy debates.

But economics does matter in at least two ways in this story. First, the process of integrating economic analysis into policymaking itself has had important political effects. PPBS was implemented partly because it promised to rationalize policymaking, but more immediately because it solved a political problem: it helped BOB and parts of the Office of Economic Opportunity to reassert control over the Community Action Program by imposing top-down budgeting. That is not to say that all economic methods have the same political effects. Cost-benefit analysis is a prime example. Because it tends to bring attention to costs of regulation that would otherwise go unmeasured, cost-benefit analysis has often been championed by conservatives who want to limit regulation. Ronald Reagan cut a lot of policy analysis budgets, but he created a whole new office to conduct cost-benefit analysis of regulations because he expected it to serve his political interests.⁴²

It is also clear from this story that economics does direct policy attention toward goals that can be understood with the tools of economics to the exclusion of other goals defined as “social.” For example, Harold Watts, an economist associated with the War on Poverty, made this choice explicit in a 1968 paper titled, “An Economic Definition of Poverty.” He contrasted what he called the “narrow economic definition” with the “culture of poverty” approach and argued that “[a] program aimed at eliminating economic poverty will measure its success by the increase in command over goods and services that is induced by the program. A program aimed at eliminating the culture of poverty will measure its success by changes in the complex of attitudes and behavior patterns characteristic of that culture.”⁴³ While pursuing one goal may incidentally advance the other, you cannot effectively pursue both, he argues: there is a tradeoff. And he claims that it is the economic goal that should be pursued, largely because it is more tractable.⁴⁴

Yet the tradeoff between more and less achievable often meant that what seemed less achievable was simply ignored. During the PPBS boom, for example, the Department of Health, Education, and Welfare was tasked with creating a “social report” to develop indicators of the social, as opposed to the economic, well-being of the population. Somewhat ironically, the effort was led by economists Alice Rivlin and Mancur Olsen, although other social scientists, including Daniel Bell, participated. But while a document

was published that highlighted questions of social mobility, health, crime, the physical environment, and even alienation, the effort never really took hold.⁴⁵ The normative quality of such measures were much more obvious, and this was politically problematic. As such, there was no commitment to building an infrastructure for measuring them.

Finally, it is important to note that while economics has political effects both in the goals it emphasizes and in the implementation of its methods, these can vary across time and place. For example, it can be argued that we didn't notice that income gains were going mostly to the top 1% in the U.S. partly because economists weren't looking for it. Simon Kuznets had "shown" that inequality declined as societies became more developed, so distributional issues seemed to be of limited concern. But it was also economics that put distributional issues back on the table in the '90s—at first through the Congressional Budget Office, which highlighted the distributional consequences of tax policy.⁴⁶ Interestingly, the CBO did this for bureaucratic reasons. The staff of the Joint Committee on Taxation had a monopoly on providing tax analysis to Congress, and the CBO wanted a piece of the action. It carved out a niche for itself by providing data on the distributional effects of tax policy, which the Joint Committee had not been doing.⁴⁷

But while the policy dominance of economists may promote a range of political positions, the possibilities are not endless. One can imagine worlds in which economists give more or less emphasis to growth or distributional concerns. But it is harder to imagine how economists as a profession might tackle a question like political empowerment, however much individual economists might value it. If the dominant policy role of economics encourages attention to one family of goals while necessarily ignoring others, that still has political effects, if only by constraining the range of options that can be considered.

* * *

I want to return very briefly to the research question I began with. I have been focusing on how economists championed one particular family of methods that then became integrated into policymaking in a variety of ways. But what about the other end of the question: how particular policy domains, like antipoverty policy, become more focused on economic goals? In antipoverty policy, at least, economists were able to impose their definition of the problem by leveraging their academic and political status, the perception of science that was attached to their analyses, and by the political appeal of their methods at a particular moment in time. This did not by any means determine which policies would be followed, but it did shape the range of policies it made sense to consider, as well as the criteria on which policies were judged successes or failures.

In antipoverty policy in particular, however, this story still feels like it's missing something. Despite the proliferation of rational policy analysis, our national conversations about poverty still retain a strong moralistic flavor that often trumps economic expertise. Economists may have been able to impose their definition of the problem on other experts, but the broader public often cared more about making sure the poor were working hard enough or were punished for making bad decisions, not simply about bringing people

above the poverty line.⁴⁸ So there is a danger in overstating even these indirect effects of economics when a larger public doesn't care about rationality at all.

Finally, a puzzle remains about the relationship between the rhetorical power of economics and its practical application. In antitrust policy, economists gained control over a fairly technical set of decisions and exercised that influence to redefine antitrust policy in ways that were logically consistent with the goals of economics.⁴⁹ But in science policy, the rhetorical appeal of using science to drive innovation and achieve growth has been enormously powerful, although economists themselves have had very limited direct influence. They are the source of a powerful myth that is legitimate in part because economists themselves have policy legitimacy, but play little role in determining actual policy.⁵⁰ I am still convinced that it matters to politics that economists are so visible in the policy process. But there is much work to do in understanding when economists shape policy and when they are irrelevant, even to the policy conversations they care most about.

ENDNOTES

1. Axel Leijonhufvud, "Life among the Econ," *Western Economic Journal* 11, (1973): 327.
2. Barry M. Katz, *Foreign Intelligence: Research and Analysis in the Office of Strategic Services, 1942-1945* (Cambridge: Harvard University Press, 1989), 97-98.
3. *Ibid.*, 131-136.
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6. Robert M. Solow, "Technical Change and the Aggregate Production Function," *Review of Economics and Statistics* 39, (1957); Richard R. Nelson, "The Simple Economics of Basic Scientific Research," *Journal of Political Economy* 67, no. 3 (1959); Kenneth Arrow, "Economic Welfare and the Allocation of Resources for Invention," in *The Rate and Direction of Inventive Activity*, ed. Richard R. Nelson (Princeton: Princeton University Press, 1962); Edwin Mansfield, "Rates of Return from Industrial Research and Development," *American Economic Review* 55, (1965).
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