

Forty Years of Social Policy and Policy Research


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Shortly after his narrow victory over Richard Nixon in 1960, president-elect John Kennedy telephoned Professor James Tobin of Yale University and invited him to become a member of the Council of Economic Advisers in the new administration. Tobin had some doubts: "I'm an ivory-tower economist," he said. Kennedy topped him: "That's all right; I'm an ivory-tower president." The point of the story is that neither man was quite telling the truth.

There was never any ambiguity about where the Urban Institute stood. Lyndon Johnson was practically allergic to ivory. From its very birth, in fact even during pregnancy, the Institute's focus was on knowledge for policy. The incorporators, who were themselves experienced men of the world, had to sweet-talk President Johnson into accepting that the Institute could aim to bring anything useful that might come out of the ivory tower into the gritty world of live social policy. That is not always as simple as it sounds. My goal today is of course to heap well-deserved praise on the Urban Institute, but also to remind you of the complex interaction between policy research and policy action; that is the minefield where the Institute works.

The relation between economic and social policy and economic and social research is both indispensable and uneasy. (From now on, whenever I say "social policy" I mean social and economic policy.) It is an indispensable relation because most ideas proposing a new social policy are not very good ideas. You can think of this as just a universally observed fact of political life, but there may be fundamental reasons for it.

One important reason is that modern social life is highly complicated. Side effects and the side-effects of side-effects can be a very important part of anything that happens, but most of the policy ideas that turn up in the marketplace are fairly superficial: here is one obvious problem and there is one direct, obvious solution. The incidental implications are forgotten. Unless policy wheels are to spin forever, there has to be some way of weeding out the bad ideas fairly inexpensively. That is where research comes in.

Rigorous evaluation of social programs as they are activated and, even better, experimental testing of proposed social programs before they are legislated, are the only methods we know for finding out, in a serious way, what actually works and what merely seems plausible. The advantage of experimental testing comes from the deep embeddedness of social life: real life never holds all the extraneous forces constant, so the passive observer can never be sure whether any observed before-and-after differences come from the program or from all that background noise. Experiments are carefully designed to isolate the program effects by eliminating or randomizing the extraneous influences on outcomes. But that is often impossible. It takes a lot of ingenuity and courage to achieve that kind of isolation non-experimentally, but that is what the Urban Institute does. One way or the other, bad ideas are those that turn out to have no independent effects or to have perverse effects.

It is worth remembering that the Urban Institute began, in Bill Gorham's mind, as a vehicle for policy evaluation. In a history of the organization of the Institute, he is paraphrased as saying that "it is unknown what good is accomplished by public actions financed with tax money." He also emphasized the need for careful evaluation and the importance of gaining information from primary rather than secondary sources. The

focus then was on cost-benefit analysis, as it should always be, provided the analysis is done properly and comprehensively, especially on the benefit side. Measuring the benefits of some social policy intervention rests fundamentally on our ability to disentangle the true effects of the intervention from all the other things that are going on at the same time.

In the beginning, the Institute's intended terrain was specifically urban pathology, the highly visible problems of the nation's major cities. Many of its publications in the early years were aimed directly at the evaluation of state and municipal, as well as federal, programs. Some of them tried to convince and cajole governments to take the evaluation problem seriously. Most, however, were how-to-do-it studies, covering activities as diverse as public transportation, garbage collection and day care, every one of which needed serious evaluation.

Early in its history, the Institute undertook to examine how four cabinet-level agencies went about evaluating 15 of their social programs. The agencies, as they were then called, comprised the Departments of Labor, Health, Education and Welfare, and Housing and Urban Development, plus the Office of Equal Opportunity. The Institute's spade work led to the creation of what was probably their first comprehensive evaluation systems. I want to emphasize that this is a bigger deal than just getting a better fix on program effects, though it is that, too. The deeper point is that the good name of social policy as a public enterprise lives or dies with the perception that policymakers know what they are doing and can indeed make a convincing argument that policy choices are not merely shots in the dark.

As the scope of the Urban Institute's assignments has expanded far beyond the

narrowly urban during the past 40 years, the search for real understanding has played a larger role, inspired a major body of work, and lost none of its importance. The approach has naturally evolved along with the scope. Measuring the true effect of a single, limited social intervention is different from estimating the impact of a broad policy change or of a whole panoply of interventions. Then experiments or near-experiments have to give way to large surveys and massive data-bases. That is what happened when the Institute undertook broad analyses of the Reagan years and then of the "New Federalism." Major changes in the tax code pose similar analytical problems, and have been met in the same way..

But I also described the connection between social research and social policy as uneasy; and here is what I had in mind, another disconcerting fact of policy life. It is a fair empirical generalization from years of social research that, even for those policy measures that pass tests of statistical significance, the ones of which we can honestly say that they have real effects on social outcomes of the kind that they are designed to have, even for them, almost always those correctly measured effects seem to the policy community to be disappointingly small.

Social policies are created, and sold to the electorate and their representatives, as cures for acute or nagging social problems, not as ways of reducing the incidence of something unpleasant by one-twentieth or improving long-run outcomes by something between six and nine percent. We have had a War on Poverty and a Great Society; you don't win hearts and minds by promising a Small Guerilla Incursion on Poverty, or a Slightly More Equitable Society. There is inevitably a feeling of letdown when the small

statistical truth comes out. Is *that* what the fuss is about? Remember: these are the successful social interventions. One has the feeling that, if exactly this degree of success had been accurately promised at the beginning, the policy intervention would have been stillborn, incapable of generating the enthusiasm needed to get it passed, financed, up and running. So social policy research seems often to be courting the danger of throttling what it is intended to improve.

You could try to explain this away. Maybe the standard research methods tend to be biased against strongly effective policies. Or maybe we just haven't hit the bull's eye yet and, somewhere out there, fantastic wonder-drug-like cures are waiting to be discovered but have been unaccountably overlooked so far. I don't think that there is a good case to be made for either of these excuses. It is not a matter of Wait until next time. My personal conviction is that the behaviors we perceive as social (and economic) problems are messily intertwined with each other and with normal behaviors: crime with poor housing, poor housing with bad education, health care and unemployment with all three, income volatility with just about everything. Piecemeal social policies, the only kind we are likely to have in a stable democracy, are generally likely to have small effects because they reach only one or two parts of the tangle.

This thought leads to an even deeper reason why progress on fixing social problems comes in such small pieces. Social and economic pathologies are not just plagues that are inflicted on us by original sin, bad luck, or previous mistakes. They are in part--I emphasize *in part*--shaped by the (less than iron-clad) laws of economics. There are some fundamental principles involved that are going to work themselves out one way or another. If we try to correct one such pathology by addressing it directly, it may

manifest itself in another form, in another place, affecting other people, perhaps because the law of supply and demand doesn't give up, or because if you stop me from doing this, I will respond by doing that. If you squeeze the balloon in one place, the displaced air pops up in another. This fact of social life is what underlies the often-cited law of unintended consequences.

If that is so, then one way or another we have to teach ourselves and our representatives to understand that this is the way the world works, and that promoting unrealistic expectations about favorite policies is going to make things worse, not better. In these circumstances the role of careful, unbiased social research becomes even more essential: if progress depends on the cumulation of small favorable effects, it is really vital not to waste money, effort and good-will on basically ineffective ideas, no matter how plausible and tempting they look. Research has to be able to detect and diagnose the modest successes. And this is on top of the other function I mentioned: helping to weed out the really bad ideas, or at least to give fair warning of potholes in the road.

This all sounds pretty pessimistic, but Gorham didn't promise you a rose garden, and neither did his sober and judicious successor Reischauer. The very difficulty is why specialized institutions like the Urban Institute are needed. And in actual fact, despite these intrinsic obstacles, there have been some outstanding successes.

The first successful example I want to mention may seem slightly atypical, but actually does fit the pattern: the Tax Policy Center. It is also an organizational novelty, a joint venture of the Urban Institute and the Brookings Institution, with Len Burman of Urban as Director and Bill Gale of Brookings as Co-Director. Tax policy is always with

us, even when nothing new is happening; it is the ubiquitous social policy. If you think in terms of Richard Musgrave's famous conceptual division of the government into an Allocation Branch, a Distribution Branch and a Stabilization Branch, tax policy involves all three. It involves the Allocation Branch because every tax, intentionally or not, creates incentives for people and businesses to do certain things and to avoid doing other things: to work more or less, to invest more or less, to drive more or less. So taxes always affect the way productive resources are used. It involves the Distribution Branch because every tax has an incidence, even if that is not its main purpose; it is paid, in the last analysis, by some group of individuals or institutions and not by others, depending on their status and behavior. It involves the Stabilization Branch because taxes influence spending decisions and spending decisions promote or discourage production and employment somewhere in the economy.

So it is a necessity for sound democratic government that, whenever tax policy is being discussed, which is almost always, there should be unbiased, quantitative, state-of-the-art information about any current proposal: whose behavior it is likely to affect, and how, whose incomes will ultimately be increased or decreased, and how much revenue it will gain or lose for the federal--or state and local--governments. You can not answer those questions just by reading the text of a proposed piece of tax legislation, or by listening to the stated intentions of its sponsors. The answers come out of the detailed working of the economic and social system. Masses of data have to be processed. The lessons of history have to be inferred statistically. Systematic calculations--what we call models--have to be run and re-run and interpreted.

This is what the Tax Policy Center can do, and has done, as a sort of intellectual

public utility. Its value is enhanced by its earned reputation for telling it like it is. Often its findings confirm the observations I made earlier: the summer gas tax holiday was a bad idea; most compromise stimulus packages have small effects. So we have an indispensable tool for policy evaluation, even if it does not, because it can not, work by direct observation of before-and-after.

The Tax Policy Center is only the latest and, perhaps, best known modeling enterprise of the Institute. An earlier example was the Institute's very detailed Transfer Income Model. What it did--and still does--is to digest any proposed change in federal transfer programs and tax rules, and tell you how it will affect average households at different income levels and with different demographic composition. The third generation of this model provides the kind of information without which any attempt to reform social programs is mostly guesswork and extrapolation.

Now I have to return to an important issue that arose earlier. The fundamental difficulty in the evaluation of any complex social program is somehow to eliminate the influence of "external" factors, so that what is left is the "true" effect of the program. Experiments do this by creating statistically identical experimental and control groups. Sometimes a valid "comparison" group can be found in nature, so to speak. But often, and always when the program itself is large and inclusive, these devices are ruled out. The only way to isolate the effects we want to measure is to use statistical methods to purify the observed events of those external influences.

For the Urban Institute and its siblings, the most important and ubiquitous influence is the course of the national economy itself. A period of strong national

economic growth can make even an ineffective intervention look good, and hard times can make an effective program look helpless. To take a recent example: the welfare reform legislation of 1996 was supposed to "end welfare as we know it." The welfare rolls did diminish substantially after 1996; but those were also years of unusually rapid non-inflationary growth, so welfare enrollment would have fallen in any case. So how much difference did welfare reform make? There is a prize example of an important policy question that can not be answered just by looking. A complicated process of inference can not be avoided.

A good deal of hard work by Institute researchers and others led to the conclusion that welfare reform accounted for a lot--not nearly all, but a lot--of the reduction in the size of the welfare population. Exact numbers are inevitably uncertain, and unsuitable for dinner conversation anyway, so I will leave it at the qualitative statement. I think it is more interesting that a by-product of the research was the understanding that it was in a way the wrong question. Ending welfare as we know it was not the real goal; that could have been accomplished just by ending welfare. Presumably the real goal was to reduce the incidence of poverty, and there the results were much more problematic.

It turned out that while some of those who left welfare--mostly single women with children, of course--found and kept jobs, many others simply disappeared from the data. And a large number of the welfare-leavers who could be tracked, some with and many without jobs, remained poor, especially the ones with the deepest personal, educational and circumstantial disadvantages. Keep in mind that this was all in a period of exceptional national prosperity. So ending welfare as we know it is not the same thing as ending poverty as we know it. That problem remains on the policy agenda. If it

remains on the policy agenda, then, by the Institute's standards, it remains on the research agenda.

The Institute's ten-year project entitled "Assessing the New Federalism" expanded this kind of research across a much larger canvas. The devolution to the states of decisive responsibility for a vast assortment of health, employment, income maintenance and social-service programs for low-income families amounted to a complex social experiment without controls. It was a massive and costly job just to document what in fact happened: what was done state by state, and what became of the families who were the ultimate subjects of the experiment. While the project stops short of the inherently more difficult task of imputing causality, it provides the indispensable observational base without which no deeper analysis is even thinkable. And sometimes, just sometimes, the data do speak for themselves.

Now I want to finish with some reflections on measured poverty rates. Poverty is certainly a central concept in any broad discussion of social policy. Many of the problems and pathologies that are the focus of social policy are consequences of poverty, many are causes of poverty, and many are both. It is of course exactly the both-ness that creates complexity for both policy and research. When I had arrived at this point in the drafting of this lecture, I came upon the Census Bureau's announcement that the poverty rate in 2006 and 2007 was stationary at 12.5 percent. We will probably discover that it is higher in 2008 when the data for this year are in. I was right to say, a moment ago, that poverty as we know it is still with us. Two questions arise immediately for anyone interested in social policy and social research. The first one is: what do we mean by

poverty or, less grandiosely, how do we or should we measure it? The second question is: assuming that we are measuring the poverty rate in a reasonable way, how can it have remained roughly flat during the past 40 years (after having fallen sharply between 1960 and 1968) while the economy was getting richer, income per person was doubling, and even median family income was rising by some 20 percent?

Most of us know that the "official" poverty threshold in the U.S. was defined in the mid-1960s by Mollie Orshansky, who was then an economist with the Social Security Administration. It was a miracle of ingenuity: simple, plausible, and workable. Orshansky observed that indubitably poor families spent about a third of their income on food. So she priced out an "Economy Food Plan" designed by the Department of Agriculture--devoid of all luxury, monotonous but nutritious--and multiplied the cost by three. Except for inflation-adjustment and some allowance for family size and composition, that has been the official standard ever since. You are officially "poor" if your cash income, including cash transfers but before taxes, is below that threshold. In other words, you are poor if, after paying for that minimal diet, you do not have enough left over to pay for the other things--housing, clothing, transportation, etc. that families at your low income level normally buy.

There are obvious problems with this calculation of the poverty rate, the most glaring being the omission of direct taxes and transfers in kind. The problems are clearly fixable, and this is not the occasion to consider them. Nor will I discuss some more subtle issues, like the proper treatment of assets, the appropriate time unit--how long must you be below the threshold to be considered poor?--or the appropriate family unit and allowance for its particular needs. Those are all worth discussing and, indeed, they have

been discussed. The most exhaustive treatment is a 1995 report by an eminent panel convened by the National Academy of Sciences' National Research Council (*Measuring Poverty: A New Approach*). A full five years earlier, you should know, the Urban Institute published an excellent volume by Patricia Ruggles, covering essentially the same ground more compactly (*Drawing the Line: Alternative Poverty Measures and their Implications for Public Policy*). There is even talk of maybe, some day, improving Mollie Orshansky's inspired concoction.

But I want to focus on more general issues. One outstanding advantage of the official definition of the poverty line is that it has remained essentially unchanged for more than 40 years. Of course one outstanding disadvantage of the official definition of the poverty line is that it has remained unchanged for more than 40 years. In the meanwhile, many relevant things have changed. To take a concrete example, poor families today spend much less than a third of their incomes on food. If Orshansky had had the same bright idea today, she might be multiplying the cost of the Economy Food Plan by something closer to six or seven, and that would double the income corresponding to the poverty line. If that sounds drastic, well, there are other necessities besides food. You could build an Orshansky-like measure around minimal shelter costs, for instance. But housing costs have been increasing as a share of family expenditure, so the opposite of the food problem would arise: the multiplier would be falling and the poverty line with it.

The deeper point is that expenditure patterns change, and the commodity-meaning of poverty changes too. In the long run, and 40 years is a long run, poverty is a relative

concept, not an absolute concept. For instance, the World Bank defines the poor as those living on a dollar or so a day, but that is simply meaningless in a developed-world context. If you were going to revise the Orshansky concept--revise the concept, not just fix the details--you could start where she started in 1965, and then, say, adjust the poverty line periodically according to the change in median family income. If the median four-person family is 10 percent better off since the last revision, then the poverty threshold for a four-person family goes up by 10 percent. But then it would be simpler and more understandable just to *define* the poverty threshold as a fixed fraction of median income. One-half is a round number, though nothing else. You are poor if your income is less than half that of the median family. Something like that would at least take the relative nature of poverty seriously, and it would pose the policy problem squarely: how much inequality below the median are we prepared to tolerate?

By the way, the current official poverty line is about 40 percent of median family income. If we shifted to one-half, the measured poverty rate would be higher. In Europe, $\frac{2}{3}$ is a more common figure, but the meaning of any such difference is complicated by the fact that there health care and education are not privately funded. For social-policy purposes, the location of that threshold is a quintessentially political question. How should one think about it? I can not discuss that now, for two reasons: there is not enough time, and I can't think of anything interesting to say. But I have one more observation.

I mentioned earlier that the official poverty rate in the United States has been essentially flat since 1968. We can now see what that must mean. The poverty line itself has been fixed; but median family income has increased by about a quarter, though not uniformly. So the poverty threshold has been falling as a fraction of median income; we

are looking further and further out along the lower tail of the income distribution. The proportion of families still further out in that tail has stayed more or less constant. What must have happened is that the degree of inequality below the median has been increasing just about fast enough to keep the poverty population fairly constant. That is not a pretty thought. The poorest among us have been losing ground compared with the median just about as fast as the fixed poverty line has been falling behind the median family income.

Social policy in the past has been patchwork: see a problem, fix a problem. Maybe. That is not going to change; it is the way our system of narrow majorities works. One vital role for social research is to identify the patches that actually work; they are not obvious. That is where I started this talk. I want to end it by pointing out another role for social research: to find ways to make effective social policies more "inclusive," so that the forces that give us a rising standard of living above the median more or less automatically spread to those far below it. It could take another 40 years, and give the Urban Institute plenty to do.