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R. A. Levine

H. Watts

Robinson G. Hollister

Swarthmore College, rhollis1@swarthmore.edu

See next page for additional authors

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Authors

R. A. Levine, H. Watts, Robinson G. Hollister, W. Williams, A. O'Connor, and K. Wilderquist

Chapter 5

A Retrospective on the Negative Income Tax Experiments: Looking Back at the Most Innovative Field Studies in Social Policy

Robert A. Levine, Harold Watts, Robinson Hollister, Walter Williams,
Alice O'Connor, and Karl Widerquist

Introduction, Karl Widerquist

The United States government conducted four negative income tax (NIT) experiments between 1968 and 1980. NIT is a form of basic income guarantee (BIG) that was popular in the 1960s and 1970s. It differs from basic income in that it gives money only to those with low incomes. However, both are basic income guarantees in the sense that they guarantee everyone a minimum income. Although the differences between the two policies are important, they have enough similarities so that what was learned from the NIT experiments can help us understand the consequences of either BIG plan.

The experiments began under the direction of the Office of Economic Opportunity (OEO) near the end of the Johnson administration and continued within the Department of Health Education and Welfare (HEW) after the Nixon administration abolished OEO. Their main goal was to determine the labor supply response to an income guarantee. That is, how much will work effort decline if a negative income tax is introduced? But as the experiments went on, many more questions were examined. The first experiments were conducted in New Jersey and Pennsylvania between 1968 and 1972, on a largely urban population of two-parent families. Two more experiments were soon added—one in Gary, Indiana to examine the effects of an NIT on single parents, and one in North Carolina and Iowa to examine its effects on rural populations. Finally the Seattle-Denver Income Maintenance Experiment (SIME-DIME) was added with a much larger experimental population.

These experiments were the first large scale social science experiment ever conducted, and they have become a model for social experiments. They employed the method (common in the natural sciences) of dividing subjects into a control group and an experimental group through random assignment. The experimental

group was given a negative income tax and the control group was not. Researchers collected income information and conducted interviews with both groups to determine how those receiving the NIT behaved compared to those not receiving it. The experiments eventually included thousands of subjects and collected data on variables such as time spent working (for all members of the family), school attendance, health, and marital status.

The experiment's results were widely discussed in policy circles and in the popular media at two times. In 1970, Nixon's modified version of the NIT, the Family Assistance Plan, was being debated in Congress. To help its policy cause, the administration pressed experimenters to release their findings long before they were ready to do so. While preliminary results showed very moderate reductions in labor supply due to work-incentive effects, Congressional opponents criticized the findings as premature.

The results were again discussed in the late 1970s during hearings for Jimmy Carter's Program for Better Jobs and Income. The finding that the work disincentive was not so large that it made the program unaffordable was overshadowed by two other findings. Although the experimenters expected to find some negative work incentive effects, and were pleased with how small they turned out to be, many newspapers reported the results as if the very existence of such effects was a crushing blow to the idea. Also, a controversial finding that the negative income tax increased the divorce rate caused a furor against the policy both in Congress and the media.

In the following years, hundreds of articles in books and scholarly journals debated the results of the NIT experiments. For a critical review and extensive bibliography, see Widerquist (2005).

In February 2002, the First Congress of the U.S. Basic Income Guarantee Network brought together four of the original experimenters and one historian to discuss the meaning of the experiments today. The session moderator was Robert Harris, former executive director of the President's Commission on Income Maintenance, and former vice president of the Urban Institute. The speakers were Robert Levine, senior economic consultant of the Rand Corporation and author of *The Poor Ye Need Not Have With You: Lessons From the War on Poverty*; Robinson Hollister, professor of economics at Swarthmore College and coauthor of *Labor Market Policy and Unemployment Insurance*; Harold Watts, emeritus professor of economics and public affairs at Columbia University, former director of the Institute for Research on Poverty, and coeditor of *The New Jersey Income Maintenance Experiment*, Volumes II and III; Walter Williams, emeritus professor of public affairs at the University of Washington, author of *Honest Numbers and Democracy: Social Policy Analysis in the White House, Congress, and the Federal Agencies*; and Alice O'Connor, associate professor of history at the University of California-Santa Barbara, author of *Poverty Knowledge: Social Science, Social Policy and the Poor in Twentieth Century U.S. History*. What follows is taken from their remarks.

The Political Background of the Experiments, Robert Levine

In the nineteenth century, economists were engaged in a great controversy over whether something called “value” was determined by supply or demand. Around the turn of the century some brilliant mind said “Why don’t we substitute ‘and’ for ‘or,’ and make it ‘supply and demand’?” The discussion today about jobs guarantees or income guarantees reminds me of that. The first formal proposal for a negative income tax (NIT) by the United States government was made in the five-year antipoverty plan of 1965. NIT was very quickly thought to be in conflict with a job guarantee. But the question of full employment or basic income guarantee strikes me as a non-argument. That was the way some of us thought of it then, and I still think that was the right way to think of it. But the negative income tax experiment came out of that debate.

Part of the political context is well known, at least in our esoteric circles. Alice O’Connor quoted me in her book as saying that when we brought the idea of a NIT to the high command of the Office of Economic Opportunity (OEO), the director of congressional relations said “that won’t be an experiment in negative income tax, that will be an experiment on how to kill a program on the Hill.” Sargent Shriver, to his credit, dismissed that, even though he was a politician to his fingertips. He said, “No, this is important, this is interesting, and we will go ahead with it.” That was the political birth of the experiments that we’re still discussing more than 30 years later.

Some of the political background to the NIT in OEO has not been commonly known. Because the money for the experiment was to be taken from the Community Action Demonstration Program (which had existed from the beginning of OEO in 1965), there was a substantial sum of tens of millions of dollars available. Some of the more controversial demonstration programs were in Chicago and Mississippi and a lot of the money was used to fund programs that were considered not just intellectually, but politically, radical; and they caused OEO much trouble at the time. Basically the accusation was made that the government was funding political power.

Then, we came in; we were the reactionary economists who wanted to do something else. Before I became assistant director of OEO, I was in charge of the division of research and planning. Robinson Hollister succeeded me, Walter Williams succeeded Rob. The only reason Harold Watts wasn’t in the sequence is that he went back to Wisconsin before we could catch him. We wanted to try *science* to find out something very specific. My colleagues will describe the specifics, but we believed that the basic political obstacle to anything like a negative income tax was the widespread belief that it would kill work incentives. We set out not to prove that it would not, but to find out whether it would. That was the very narrow, scientific focus of the experiment.

We set control groups to get information on that particular topic, not to prove anything to support an agenda. Community action people attacked from the left and congressional people attacked from the right. Those attacking from the left believed in “the culture of poverty” and didn’t think income had much to do with

why people were poor. Those on the right didn't know why we wanted to get this information since the program was impossible anyhow. With Shriver's aid, we got through these obstacles.

After Nixon's election in 1968, it was generally felt within OEO that he was going to kill the poverty program when he took over from Johnson. He didn't; he appointed a new head of the program named Don Rumsfeld who brought in an assistant named Dick Cheney. Rumsfeld systematically invited OEO folks to talk to him in his congressional office. My impression was that he attempted to preserve the program by shifting it in a Republican direction—experimentation rather than action. This put a focus on the NIT experiment.

The Makeup of the Experiments, Harold Watts

I understand that Sargent Shriver said "We've got this institute up in Wisconsin and they aren't good for anything else, so why don't we have them experiment?". We took that as a serious recommendation. We took very seriously the question of whether a set of programs would produce a generation of layabouts. That was the hypothesis that needed to be examined. If you're worried about the layabout possibility, you really want to start out with people who are working. And so the first experiment did not look at the welfare population but at the working poor.

A negative income tax can be looked at as having two parts—a lump-sum grant (G) and a reduced wage (Y-tY). From the standpoint of static economic theory, both of those things should reduce the tendency to work. The lump sum grant should produce more leisure and more non-wage work. The reduced price of leisure, which comes from the reduced wage, should also produce more leisure: "Time off is cheaper, let's buy some more of that." There was no question of the direction of the effect of NIT on hours worked, and there was no lack of confirmation of that in the experiments. But the question was quantitative: How much would NIT reduce work time? The same theory that says that people would definitely be inclined to work less also says that they are made better off, because the NIT expands the alternatives available to them. This was confirmed because almost no one refused to take part in the program. That part of economic theory works fine.

But do we need an experiment to answer these questions? There are different people at various levels of unearned income and different levels of net wage running around loose in the streets. Why don't we just see what they do? The experimental problem with using these people is that nothing is imposed from the outside; there is no exogenous change. The experiment tried to make the change in income and net wage rate exogenous, so that the behavior of recipients would be representative of a national program.

The size of the work response was important, because if work effort declined sufficiently it would largely vitiate the strategy of using the NIT as a means of increasing family income. If recipients used their entire NIT to buy more leisure, it succeeds in making them better off, but it wouldn't be a good antipoverty program.

I want to emphasize a couple of ways the experiment tended to maximize the size of the work-effort response. First, we applied no additional treatments that would tend to prod them into the labor market. We had to check what they earned to determine how much to pay them, but we weren't critical if they didn't work. Second, it was a short-term experiment. In our case, essentially leisure was on sale for a three-year period. When laundry soap is on sale, what do you do? You buy a lot of it. You might expect people in the experiment to act the same way. Not everyone will; someone who has a good job (it may not pay much, but it's stable) may not want to mess around with that by working less. That kind of rigidity could be built into their job. But by and large the poor families we were looking at didn't tend to have terribly steady jobs. There were a few with stable janitor jobs at Princeton, but for the most part, that wasn't the case, and there was a disincentive. People did work less, but percentage-wise it tended to be in the single digits for men in particular. Some of the work response came from taking more time to look for work. Some of it came from cutting down hours, say from 65 to 60 hours a week, which doesn't seem like a tragedy. I don't remember finding anyone (on an anecdotal basis), who as soon as they got the grant, left the labor market and sat on the porch and whittled for three years.

That's what we found and those are the tools we used to find it. The whole flavor of the OEO at that point was something rather new to the government: a willingness to look at evidence, to do some examination, to check some facts out. That hadn't always been there, and in that sense, this idea of doing some experimentation fell on fertile ground.

The Findings of the Experiments, Robinson Hollister

My part of this discussion covers three points. First, I review the results of the experiments with respect to the labor supply, which was the central issue driving the design of all these experiments. Second, I talk about the non-labor supply outcomes that have largely been ignored and that covered a territory that was ahead of its time in many ways. These bear heavily on what other benefits come from having a basic income guarantee. Third, I map the findings of the experiments to what we have found since then.

Labor supply results showed about a 13 percent reduction of work effort for the family as a whole starting from an initial work effort of 35 hours per week for the entire family. One-third of the response came from the primary earner, one-third from the secondary earner, and one-third from the tertiary earner in the family. In most cases, the primary earner worked more hours than the secondary and tertiary earners, and therefore, when measured in percentage terms, there were relatively small responses from the primary earner. Percentage term responses were much bigger from the female spouses in the family and from the third workers in the families. The biggest response overall came in reduction in the female labor supply and that mostly took the form of slower reentry to the labor market after absence. This labor supply response added about 25 percent to the static costs of a national

program with a guarantee level approximately at the poverty line. You could look at these results as either half empty or half full. You could say that 25 percent is too much or not too much.

The most common non-labor supply result mentioned was an erroneous finding by some sociologists (from an initial analysis of the Seattle–Denver Income Maintenance Experiments) that the marriage-dissolution rate for black families in the experimental groups was 57 percent greater than the control group, and 53 percent greater for white families. When these results came out in congressional hearings, Senator Moynihan, who had been a backer of Nixon’s Family Assistance Plan, and who had written a very controversial report about instability in the black family, recanted his support for the guaranteed income. Those particular findings greatly contributed to killing the Carter administration’s guaranteed income scheme. In the 1980s, Glen Cain carefully reanalyzed the data from the Seattle–Denver experiment. The results were technically quite complicated, but there was basically no family dissolution effect. Some of the results were suspect from the beginning, because the effect seemed to occur in the sector of people with the lowest guarantee rate, the lowest incentive to strike out on their own—those who had the least to gain from breaking up showed the largest amount of marital breakup. Cain’s study appeared in the *American Journal of Sociology* in 1990, with a rebuttal by the authors of the original findings, but subsequent studies (and those from the other NIT experiments) also found no effects on marital stability.

The rural experiment in North Carolina and Iowa collected data on educational attainment. In North Carolina there were significant positive influences in grades 2–8 in attendance rates and teacher rating, and on test scores. The literature on education shows that it is nearly impossible to raise test scores through direct intervention. Yet, BIG had large positive effects on the test scores of children in the worst-off families in the rural South. The New Jersey experiment didn’t collect data on test scores, but there was a very significant effect on school continuation; that is, BIG was an effective anti-dropout program. Again, if you look at programs that are trying to reduce dropouts directly, it’s a pretty dismal scene. In Gary, there were positive test score effects for males in grades 4–6. In Seattle–Denver, there was a positive effect on adults going on in continuing education.

Some of the experiments collected data on low birth weight, nutrition, and other quality-of-life variables. Low birth weight is associated with very serious deficits later on in life, and programs that try to reduce the incidence of low birth weight have been largely ineffective; but the Gary experiment found that NIT reduced low birth rates in the most at-risk categories. The rural experiment showed significant effects in various categories of nutritional adequacy. Homeownership showed significant effects in New Jersey, in the rural experiment, and in the first year of the Gary experiment.

It is important to map these results into more recent experience, both experimental and non-experimental. Later experiments such as the Minnesota work-welfare reform (MFIP), SSP in Canada, and New Hope in Milwaukee tended to be work related with strong financial incentives. People who wanted to get benefits had to work a minimum of hours and, as you would expect, these experimental

programs elicited greater work effort. But across all the experiments, secondary earners used some of the benefits to buy more time in the home. Non-experimental studies using income tax returns also found effects similar to the NIT. Two-parent families receiving an Earned Income Tax Credit (EITC) used some of the extra income to increase time at home; this was especially true for secondary earners. The order of magnitude of the labor supply elasticity is essentially the same in more recent experiments. The Minnesota experiment found positive effects for marital stability and reduced domestic abuse. The Canadian experiment found an increase in marital stability in New Brunswick and a decrease in marital stability in British Columbia. The New Hope experiment found some long-term effects on the educational performance of males (in the experimental group) in elementary school.

The Use and Misuse of Experimental Information, Walter Williams

I'm greatly concerned about the growing misuse of policy information in the current political environment. Elliot Richardson (1980: 105), a distinguished secretary of several United States government departments wrote, "in a sense, all of the abuses of Watergate have been abuses of information: its theft, distortion, misuse, fabrication, misrepresentation, concealment and suppression." Today's efforts are not new, but these activities, with the exception of theft, are much worse today than in earlier times. And the growing abuses of information undermine informed consent by the people and ultimately American democracy itself.

I will argue that the negative income tax experiment set a standard in seeking reliable information, which should be current practice, and that the Office of Economic Opportunity (OEO) policy analysis staff of which I was a member exemplified sound analytic practices. This did not come about because the analysts involved had greater personal integrity than current practitioners, but because the political environment facilitated such efforts. It is the deterioration of political institutions that is the problem, not the skills and standards of today's policy analysts and researchers.

The policy analysts at OEO were not public relations types but academically oriented social scientists. They understood that their one comparative advantage was to go after hard evidence on the negative income tax. It is true that the OEO analytic staff hoped that families receiving negative income tax payments would not significantly reduce their work efforts. However, and this is critical, they sought a carefully designed state-of-the-art field experiment to provide a rigorous assessment of the extent to which negative income tax payment recipients changed the labor supply response. And the social scientists at the University of Wisconsin at Madison who were administering the study were even more concerned about meeting the highest research standards.

The OEO analytic unit had a basic commitment to increasing the supply of sound, relevant social policy information and undertook an extensive research program to develop it. For example, the analytic unit set up and fully funded the

University of Wisconsin's Institute for Research on Poverty, and supported in its initial stage a critically important longitudinal study at the University of Michigan following 5,000 American families and that has continued for 35 years. OEO launched the first major, rigorous social policy evaluations and large-scale field experiments. As to the latter, the New Jersey negative income tax experiment was funded largely because the OEO analytic office, in summer of 1965, sold agency director Sargent Shriver on a negative income tax plan, and he recommended it to the president in that year's agency submission to the budget bureau. Then in October 1965, the office sent the budget office a more detailed, more accurate estimate for the cost of a negative income tax aimed at ending poverty by 1976—the 200th anniversary of the Declaration of Independence.

The United States has experienced a radical change in the political environment since the 1965–1968 period. During that period, the OEO analytic staff could engage in sound analyses of the pros and cons of policy options to support agency decision-making. The commitment to good information at OEO certainly did not run throughout the government. But, between then and now, the changes that have come about have been negative. Although emphasis on sound data remains essential for reasoned policymaking, I have found over the years that there is more and more distortion of information and policy analyses. Over time, the willful use of deceptive statistics and misleading analyses has increased materially, with the current administration using distorted evidence as its main weapon in misleading the public about its major policies.

Our political system has been deteriorating because people in senior positions, including the highest officials in the White House and Congress, have been propagandizing citizens who often do not perceive the nature and extent of the subterfuge. Take President George W. Bush's 2001 tax bill, where the top one percent of the income distribution got thirty percent of the tax cuts and the bottom forty percent got only about fifteen percent. Yet, the Bush administration was able to pass the tax legislation by engaging in an extended propaganda campaign claiming falsely that those at the bottom benefited the most.

The overriding problem is that the public is fed distorted information and false assertions based on it; yet, the politicians lack either the political will or the institutional capacity to restore integrity to national politics. As I observed in *Reaganism and the Death of Representative Democracy*, "The extent to which deceptive propaganda has been employed in [President George W.] Bush's first three years to sell major policy proposals makes the Bush administration radically different from any earlier presidency" (Williams 2003: 259). Ultimately, the issue is whether the public receives sound policy information and interpretation—prior to the making of major public decisions—for there to be informed consent. If not, democracy withers. Policy analysts are accused of aggrandizing the importance of valid information so let me turn finally to an impeccable source, James Madison, the father of the Constitution: "The people who mean to be their own Governors must arm themselves with the power knowledge gives" (Hunt 1910: 103). In sum, American democracy requires the informed consent of the people on major policy choices; and such informed consent can come only when the needed relevant

policy information is available to citizens in time for them to consider the policy at issue and assent to it.

Political Ramifications of the Experiments, Alice O'Connor

The period we're talking about seems like ancient history; not only is there now less integrity in the inquiries behind policy changes, but also antipoverty is now easily dismissed as a serious policy objective. Sometimes when I tell my students that Lyndon B. Johnson made speeches about ending poverty in America, they laugh. That to me is extraordinarily sad commentary.

The NIT experiments were not just fundamentally scientific undertakings, but fundamentally political undertakings as well. Within a broader political context, we need to understand them as experiments whose design, implementation, and ultimately whose meaning, were all shaped by the volatile and rapidly changing politics of social provision, social welfare, and social citizenship. We can also see the experiments as a form of political advocacy—they sought to establish the legitimacy of the NIT in the absence of widespread political awareness or support.

As a scientific undertaking, the experiments were highly successful, but as a political undertaking the experiments had the opposite effect. They were used to undermine the NIT/BIG concept. More importantly, they show us some of the fundamental weaknesses of framing the BIG idea narrowly, as a highly targeted antipoverty measure as opposed to a more universal citizenship right, framing it as policy with labor-market effects as opposed to an intervention that actively tries to reshape labor markets. I also want to discuss what the experiments tell us about the limitations of a style of policy making that looks to these experiments as a source of policy innovation.

The politics at the time affected the experiments. The guaranteed income was talked about in the late 1960s as an idea whose time had come, but there was no significant mobilization in terms of any grassroots or major constituency groups like labor behind the idea. Instead, guaranteed income came to the fore with extremely diverse advocates. There was a group of free market economists who saw it as an antidote to the burgeoning welfare state. Some in the civil rights movement and the growing welfare rights movement talked about the NIT as a response to the problems of structural unemployment in the labor market (as well as to racism in the labor market and gender bias in the labor market), and activists attached it to an expanded notion of citizenship rights. Most important of all in terms of getting these experiments going were the Keynesian economists within the Johnson administration, specifically within OEO, who came to embrace the negative income tax as a key to eliminating poverty by 1976 as laid out in the five-year plan developed by OEO analysts. This group saw the income guarantee as a supplement to the overriding full employment growth strategy embraced in the war on poverty.

The experimenters were determined not to advocate something that would violate the basic principles of a market economy. That is to say, they didn't insist that

BIG was a response to market failure so much as to the incapacity of certain segments of people in the labor market to earn adequate wages. The experiments, therefore, were concerned with proving NIT's efficacy as a tool for raising incomes above the poverty line, and to prove the hunch (that had been based on some econometric studies) that the NIT could eliminate poverty without a massive work disincentive and within the boundaries of liberal social policies at the time. Those who held this view saw the NIT as emphasizing growth over redistribution. They were reluctant to regulate labor markets explicitly, and considered the antipoverty measures not to be about changing the dynamics of inequality, but about expanding opportunities to be part of the economic system. As a result, the experiments were highly targeted. They focused on the poorest people (up to 150 percent of the poverty line), not a broad segment of the population. They were not concerned with some of the broader labor market effects such as how a basic income guarantee affects the choice and power of workers to go elsewhere, and did not attempt to look at the impact on racial or gender segmentation of the labor market. These problems were not acknowledged within the framework of these experiments. This leads me to say that the experiments were narrowly focused on individual behavior and predicting it, not on the structural impact of the NIT.

Another kind of politics that shaped the experiments was the politics of social provision for poor people. From the standpoint of the economists who designed the experiments, one of the appeals of income guarantees was that they were efficient and would cut through some of the inefficiencies of the Aid to Families with Dependent Children ("welfare") program, and especially would be fairer to the working poor because they wouldn't be targeted to women with children. This led to a political decision in the New Jersey experiment to focus on two-parent, male-breadwinner families, assuming that welfare mothers (and their potential work falloff) wouldn't be a major political issue at the time. (This despite the fact that in 1967 Congress passed the WIN "work incentives" legislation that tried to put more welfare recipients to work.) Similarly, the assumption was that the potential work falloff among wives in two-parent families would not become a political issue. In fact, however, Nixon later played very heavily on the two-parent/single-parent divide in promoting his family assistance plan and in his more dedicated efforts to break up the political coalition behind the Great Society. By pitching his plan as a matter of fairness for the forgotten two-parent families who were ineligible for welfare, he drew a sharp, artificial distinction between the popular, stereotypically white, two-parent working poor/working-class family, on the one hand, and the "welfare poor" on the other hand, who were increasingly imagined in the public mind as predominantly black, drug using, etc.—none of which was true.

These divisive, racial politics quickly came to determine the political meaning of these experiments. They became political fodder in the Nixon-era wars over welfare reform. At first, it actually looked like this was going to be a moment of congruence between research and political priorities: Nixon's Family Assistance Plan (FAP), after all, was a version of the NIT with a work requirement attached. Before the results were in, they were able, under extreme pressure from White House advisor Daniel P. Moynihan, to put together a report that was used in

testimony in favor of FAP, saying there were no work disincentive effects whatsoever.

The moment of congruence quickly passed, however. The use of the experiments in favor of FAP drew public and political attention to the experiments. And Senator Williams from New Jersey, an opponent of FAP, used this as an occasion to sic the General Accounting Office on the experiment, claiming that the families were double-dipping and should be prosecuted for welfare fraud. He tried to get Congress to invade the office of the experiments and look into the files of the experimental families. David Kershaw, who was running the experiments, essentially camped out to prevent congressional investigators from ruining the confidentiality agreement with the families. Thus, after looking like there was some congruence between social science and politics, as the war on welfare turned into a war on dependency, the findings of these experiments were actually used to undermine the very idea of an income guarantee. As others on the panel have indicated, the initial rosy scenario from the experiments changed once the longer-range results were in: there was, after all, some measurable work disincentive from the guaranteed income, albeit relatively modest and partly due to reduced hours among secondary as well as primary household earners. There were also the subsequently challenged findings linking the NIT to family breakup. By the late 1970s, when the Carter administration attempted to revive a version of the NIT, even some of its former advocates turned against it. Moynihan, in a very public and I can't help but think, strategically timed manner, said to Congress, "I am shocked to look at these findings and say we scientists were wrong." Meanwhile, the right wing mobilized, in the form of Charles Murray and others, to use these findings to say that these experiments proved that an income guarantee was impossible.

A final political dimension to the NIT experiments is that they were considered highly innovative, not just because they were testing this "idea whose time had come," but also because they represented a new approach to policy making. It was thought that an experimental design would give definitive proof that an idea can work. I think it led to consequences that were unanticipated. The experiments ushered in a time of increasing rigor, increasing emphasis on experimental design in program planning and evaluation, but they also helped raise the bar especially for innovative antipoverty policies, which now had to prove their value before passage. Antipoverty and welfare policy has been subjected to a scrutiny that is not applied to other areas of social policy, certainly not to military policy even though the military costs far more.

To conclude, looking at the NIT experiments as a political undertaking shows us how politics can confound efforts to inform policy with scientific knowledge. Even as social scientists were sorting through and debating the meaning of the experimental findings, political opponents were using those findings to tell a simple story of lazy poor people and family decline. I would point to the importance of using the experimental findings to tell a different story, and the importance of working harder to change this prevailing narrative with a more complex alternative. And yet, those of us who know better have let the simpler

narrative rule the day. This also points to the limitations of narrow antipoverty justifications for an income guarantee. The experiments, like the welfare reform debate that followed and distorted their meaning, turn on the individual behavior of poor people; and when we frame this as a behavioral issue, we rarely get the outcome that progressives want. Finally, I think the experiments point to the political limitations of a style of policy making that doesn't pay enough attention to the need to articulate research with the needs of social movements at the same time.

Note

* Special thanks to Robert Harris who moderated the session on which this chapter is based and gave extensive comments on the written version.

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