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# A Failure to Communicate: What (If Anything) Can we Learn from the Negative Income Tax Experiments?

Karl Widerquist, *Tulane University*



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# A failure to communicate: what (if anything) can we learn from the negative income tax experiments?

Karl Widerquist\*

*Department of Politics, Lady Margaret Hall, Oxford University, UK*

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## Abstract

The U.S. and Canadian governments conducted five negative income tax experiments between 1968 and 1980. The labor market findings of these experiments were an advance for understanding the effects of a basic income guarantee, but their conclusiveness is often overstated. A review of nonacademic articles on the experiments reveals poor understanding of the results. One often overlooked cause of this misinterpretation was the failure of researchers to make clear that the experiments could not estimate the demand response and therefore could not estimate the market response to the program. Although the evidence does not amount to an overwhelming case either for or against the basic income guarantee, some important conclusions can be drawn, if they are drawn carefully.

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Between 1968 and 1980, the U.S. Government conducted four negative income tax experiments, and the Canadian government conducted one. The results of these experiments are extremely important to growing debate today about the basic income guarantee (BIG). Although the modern basic income guarantee discussion tends to focus on the basic income (BI) variant of the proposal rather than on the negative income tax (NIT) variant tested in the experiments, the two are similar enough that any conclusive findings from the experiments

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\* Tel.: +44 7747 864 6580.

*E-mail address:* karl@widerquist.com.

is of great value for the current discussion.<sup>1</sup> Although the NIT experiments had significant limitations, they yielded results that are extremely important to the current debate and that must be understood properly. This article reviews those results and clears up common misconceptions about them.

More than 200 scholarly articles on these experiments have been published in journals and books (see Bibliography B for an extensive list). Most of these articles were written in the 1970s and 1980s, but a few continue to come out today (O'Connor, 2001; Greenberg et al., 2003; Levine et al., 2004). The debate died down without a clear consensus on what the results of the experiments implied for policy, and the results were widely misinterpreted in the popular media (see Bibliography A for a list of nonacademic articles on the experiments). The experimental results continue to be cited both by supporters and opponents of the redistribution of income as evidence for the workability or the unworkability of a guaranteed income. The experimental results seem to be a political Rorschach test in which an observer's conclusions reveal more about the observer than about the observed.

For example, in 1993, long after the results were in and the initial flurry of articles was over, Hum and Simpson declared in the *Journal of Labor Economics*, "Few adverse effects have been found to date. Those adverse effects found, such as work response, are smaller than would have been expected without experimentation" (Hum and Simpson, 1993a). But in the same issue, Anderson and Block (1993) mused about why so many social scientists continue to support the negative income tax "in the face of an avalanche of negative results" provided by the experiments. The most important reason for this disagreement is that the general result of the experiment was what everyone expected: all else equal, the treatment group worked less than the control group. This agreed; the central question was how much less would the treatment group work? Along with many other statistics, the experiments provided numerical estimates of that answer. The estimates required not only quantitative evaluation of their accuracy, but also qualitative interpretation of their meaning and that inspires widely differing opinions. Perceptions of the experiments in the media and in the political arena have been confused and superficial; neither the results nor the disagreements about how to interpret the results were understood by politicians or the media.

This paper focuses on the labor market findings of the NIT experiments arguing that although the experiments were an advance for social science and for understanding the effects of a basic income guarantee, the conclusiveness of the labor-market results is often overstated. Researchers either presented their research as more conclusive than it was or failed to prevent the lay audience from making that misperception. One often overlooked cause of this misinterpretation was the failure of researchers to make clear that the experiments could not estimate the demand response and therefore could not estimate the market response to an NIT. Although the evidence does not amount to an overwhelming case either for or against the basic income guarantee, some important conclusions can be drawn, if they are drawn carefully.

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<sup>1</sup> I use the terms "basic income guarantee" and "guaranteed income" to mean any policy that ensures some minimum level of income for all citizens. "Basic income" ensures a minimum income by paying *everyone* regardless of their private income. The "negative income tax" ensures a minimum income by paying *anyone* whose private income slips below a certain level.

Part one summarizes the operation of the experiments. Part two discusses the limits of the experiments for drawing conclusions for a national policy. Part three discusses the labor market findings of the experiments in light of their limitations. Part four discusses the political and media perceptions of the experiments. Part five concludes with a summary of the lessons of the experiments both for the basic income guarantee and for the dissemination of statistical research to a lay audience.

## 1. The experiments

The five experiments conducted in the United States and Canada are known collectively as “the income maintenance experiments,” “the guaranteed income experiments,” or “the negative income tax (NIT) experiments.” They began at a time when the elimination of poverty was the stated goal of the presidential administration, when there was a growing movement for economic rights, and when many social scientists and policymakers believed that social policy reform was heading in the direction of a guaranteed income. But by the time all of the results were available the movement for eliminating poverty had dwindled and the idea of “welfare reform” was beginning to be associated with dismantling rather than rationalizing the welfare system.

The NIT experiments were the first large-scale social experiment to use the scientific method of randomly assigning human subjects into treatment and control groups just as medical researchers do when testing drugs. Some social scientists have called them, “experiments in how to conduct experiments,” and it is arguable that they had larger influence on future social experiments than in the examination of the policy they were designed to test.

The primary aim of the NIT experiments was to test the side effects rather than the effects of a basic income guarantee. The central goal of an income support program is to raise the welfare of the destitute, and that it can do that is something that does not need to be tested. Although the effect on poverty of most social policies (AFDC, TANF, EITC, job training, education, etc.) requires testing, the conclusion that an NIT with a guarantee rate at the poverty line can eliminate poverty is true by definition.

The effects of the negative income tax on health, homeownership, low-birthweight, school performance, and other indicators of the well-being of recipients were tested and reported in many studies (Avrin, 1980; Boumol, 1974, 1977; Bradbury, 1978, 1986; Cain, 1977; Elesh and Lefcowitz, 1977; Hanusheck, 1986; Kaluzny, 1979; Keeley, 1980c, 1980d; Kehrer and Wolin, 1979; Kerachsky, 1977; Knudsen et al., 1977a; Knudsen et al., 1977b; Ladinsky and Wells, 1977; Lefcowitz and Elesh, 1977; Mallar, 1977; Masters, 1978; Maynard, 1977; Metcalf, 1977a; Michael, 1978; Middleton and Allen, 1977; Murnane et al., 1981; Nicholson, 1977b; O’Connor et al., 1979; Ohls, 1980; Poirier, 1977a, Poirier, 1977b, Poirier, 1977c; Pozdena and Johnson, 1980; Rea, 1977; Robins, 1980b; Rossi, 1975; Thoits and Hannan, 1980; Weiss et al., 1980; Wooldridge, 1977). Most of these studies show positive effects, even for hard-to-change variables such as school performance and low birthweight, but discussion of these effects is beyond the scope of this paper. For an overview of some of these effects see Levine et al. (2004).

Another side effect, the effect of the experiments on the divorce rate inspired a large amount of controversy (Bishop, 1980; Cain, 1986; Galligan and Bahr, 1978; Ellwood,

1986; Groeneveld et al., 1980a,b, 1983; Hannan et al., 1977, 1978; Hum and Choudry, 1992; Tuma, 1986, but these findings are also beyond the scope of this paper). See Hannan and Tuma (1990) and Cain and Wissoker (1990a,b) for two sides of this debate.

Table 1 summarizes the basic facts of the five NIT experiments. The first, the New Jersey Graduated Work Incentive Experiment (sometimes called the New Jersey-Pennsylvania Negative Income Tax Experiment or simply the New Jersey Experiment), was conducted from 1968 to 1972. The researchers originally planned to conduct the entire experiment in New Jersey, but they were unable to find enough poor whites there and had to open a second location in Wilkes-Barre, Pennsylvania to round out a racially representative sample. The treatment group originally consisted of 1216 people and dwindled to 983 (due to drop outs) by the conclusion of the experiment. The sample size consisted of black, white, and Latino, two-parent families with incomes below 150% of the poverty line, and with a male “head,” who was not approaching retirement.<sup>2</sup> Treatment group recipients received a guaranteed income for 3 years.

The Rural Income Maintenance Experiment (RIME) was conducted in rural parts of Iowa and North Carolina from 1970 to 1972. It functioned largely as a rural supplement to the New Jersey Experiment, which focused on an urban population. RIME began with 809 experimental subjects and finished with 729. The treatment group received a guaranteed income for 2 years. Subjects met the same criteria as the New Jersey Experiment except that single-parent, female-headed households were also included. Few, if any, Latinos were included in the sample. Both RIME and the New Jersey Experiment began under the direction of Office of Economic Opportunity (OEO) and were completed by the Department of Health, Education, and Welfare when OEO was abolished.

The largest NIT experiment was the Seattle/Denver Income Maintenance Experiment (SIME/DIME), which had an experimental group of about 4800 people in the Seattle and Denver metropolitan areas. The sampled included black, white, and Latino, families with at least one dependent and incomes below \$11,000 for single-parent families and below \$13,000 for two-parent families. The experiment began in 1970 and was originally planned to be completed within 6 years. Later, researchers obtained approval to extend the experiment for 20 years for a small group of subjects. This would have extended the project into the early 1990s, but it was eventually cancelled in 1980, so that a few subjects had a guaranteed income for about 9 years, during part of which time they were led to believe they would receive it for 20 years.

The Gary Income Maintenance Experiment (which is never abbreviated) was conducted between 1971 and 1974. Subjects were mostly black, single-parent families living in Gary, Indiana. The experimental group received a guaranteed income for 3 years. It began with a sample size of 1799 families, which (due to a large drop-out rate) fell to 967 by the end of the experiment.

The Canadian government initiated the Manitoba Basic Annual Income Experiment (Mincome) in 1975 after most of the U.S. experiments were winding down. The sample included 1300 urban and rural families in Winnipeg and Dolphin, Manitoba with incomes

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<sup>2</sup> Husbands were usually the primary income earners in a family, and researchers tended to describe this role with the status-implying term “head of household.” Women could not be “heads” unless they lived with children and without a husband.

Table 1  
Summary of the negative income tax experiments in the U.S. and Canada

Name	Location(s)	Data collection	Sample size: initial (final)	Sample characteristics	$G^*$	$t^{**}$
The New Jersey Graduated Work Incentive Experiment (NJ)	New Jersey and Pennsylvania	1968–1972	1216 (983)	Black, white, and Latino, two-parent families in urban areas with a male head aged 18–58 and income below 150% of the poverty line	0.5	0.3
					0.75	0.5
					1.00	0.7
					1.25	
The Rural Income-Maintenance Experiment (RIME)	Iowa and North Carolina	1970–1972	809 (729)	Both two-parent families and female-headed households in rural areas with income below 150% of poverty line	0.5	0.3
					0.75	0.5
					1.00	0.7
					1.25	
The Seattle/Denver Income-Maintenance Experiments (SIME/DIME)	Seattle and Denver	1970–1976 (some to 1980)	4800	Black, white, and Latino families with at least one dependant and incomes below \$1100 for single parents, \$13,000 for two parent families	0.75	0.5
					1.26	0.7
					1.48	0.7–0.025y
						0.8–0.025y
The Gary, Indiana Experiment (Gary)	Gary, Indiana	1971–1974	1799 (967)	Black households, primarily female-headed, head 18–58, income below 240% of poverty line	0.75	0.4
					1.0	0.6
The Manitoba Basic Annual Income Experiment (Mincome)	Winnipeg and Dauphin, Manitoba	1975–1978	1300	Families with, head younger than 58 and income below \$13,000 for a family of four	C\$3800	0.35
					C\$4800	0.5
					C\$5800	0.75

Sources: Robins et al. (1980a,b), Ferber and Hirsch (1978) and Hum and Simpson (1993a).

\*  $G$  = the guarantee level.

\*\*  $t$  = the marginal tax rate.

below C\$13,000 (Canadian) per year. By the time the data collection was completed in 1978, interest in the guaranteed income was seriously on the wane and the Canadian government cancelled the project before the data was analyzed. Fortunately, university-based researchers were eventually able to obtain and analyze the data, so that results are available today (Hum and Simpson, 1991, 1993a).

Two parameters are central to the design of any guaranteed income. The first is the guarantee level or the minimum income level ( $G$  in Table 1), which is the amount the recipient receives if she has no private income. Theoretically, the guarantee level can be any number between zero and per capita GDP. If  $G$  is too low, the NIT will not significantly reduce poverty or increase income security, if it is too high, it will have such strong work disincentive effects that the program would be unaffordable. The experiments intended to find out whether a guarantee level sufficient to seriously reduce or even eliminate poverty was feasible. For that reason guarantee levels between 50% and 150% of the poverty line were tested.

The U.S. experiments all defined the guarantee level relative to the poverty line, testing nine different guarantee levels: 0.5 (50% of the poverty level) was tested in the New Jersey and Rural Income Maintenance Experiments; 0.75 was tested in all four of the U.S. experiments; 1.0 (just enough to eliminate official poverty) was tested in all of the U.S. experiments except SIME/DIME; 1.25 was tested in only in the New Jersey Experiment, and 1.26 and 1.48 were tested only in SIME/DIME. Mincome, which defined its guarantee level in Canadian dollars rather than relative to the poverty level, tested guarantee levels of C\$3800, C\$4800, and C\$5800 per year. These levels were near the poverty line at the time.

The other central parameter of any guaranteed income system is the marginal tax rate ( $t$  in Table 1), also known as the “take-back rate:”<sup>3</sup> the rate at which benefits are reduced as the recipient makes private income.<sup>4</sup> In other words, the marginal tax rate is the effective income tax rate per dollar of private income for recipients of the negative income tax. A higher marginal tax rate is associated with a lower overall tax-cost of program<sup>5</sup> but also with greater the work-disincentives, and a greater potential “poverty trap.” A lower marginal tax rate is associated with a greater redistribution of income towards people with incomes above the poverty line. Redistribution to this group might be desirable in terms of equity (as a reward for low-wage workers), but to do so would increase the cost of a program primarily conceived as an anti-poverty policy.<sup>6</sup> For these reasons, it is important to know what kinds of take-back rates are feasible and the work-disincentive effects of each. The experimenters tested nine different values of  $t$ : 0.3 (30%) was tested in the New Jersey and Rural Experiments; 0.35 was tested only in Mincome; 0.4 was tested only in Gary; 0.5 was tested in all of the experiments except Gary; 0.6 was tested only in Gary; 0.7 was tested in the New Jersey Experiment, RIME, and SIME/DIME; 0.75 was tested in Mincome.

<sup>3</sup> The practical working of the marginal tax rate is slightly different if the guaranteed income is administered as a basic income rather than as a negative income tax.

<sup>4</sup> Private income could include interest, dividends, capital gains, etc. But for the participants in these experiments it was overwhelmingly wage income.

<sup>5</sup> Higher marginal tax rates could be associated with higher taxes costs if the supply of labor had a very high elasticity of substitution, but this was not expected and did not prove true in any of the experiments.

<sup>6</sup> The basic income movement today puts less stress on the issue of poverty reduction and more stress on broader equity goals that make the issue of spending money on those already above the poverty line is less important.

SIME/DIME tested two nonlinear income functions with marginal tax rates of 0.7 minus 0.025 times income and 0.8 minus 0.025 times income. The effect of these two nonlinear functions was to impose higher marginal tax rates on lower levels of income and lower marginal tax rates on higher levels of income.

The use of so many different rates of  $G$  and  $t$ , reduced the numbers of subjects receiving each type of treatment, and therefore reduced the statistical reliability of the results for each. Some of this tradeoff is worthwhile to allow for testing of a greater variety of potential parameters, but the experiments might have benefited from more coordinated effort to test a uniform group of widely spaced parameters.

Table 1 summarizes the configuration of the experiments.

## 2. What the experiments could and could not measure

Within the context of the work–effort response, there were conceptual questions about which parameters and which effects deserved most concern. Results were reported for income and substitution effects of various levels of  $G$  and  $t$ , but the most discussed statistic was the simple question of the overall effect of the various treatments on the hours of work of the average recipient, and so I will focus on that here as well. There were also conceptual questions about how findings on work hours should be used: were they important because they represented the shift in the labor supply curve, because they had implications for the tax cost of the program, or because they had implications for the efficiency cost of the program? Overwhelmingly, the concern came to be the overall change in work hours and their effect on the tax cost of an NIT. Economists focused on this issue, even though only the work disincentive effects of the marginal tax rate (not the guarantee rate) represent a true cost in terms of economic efficiency (Hall, 1980a and Hall, 1980b).

The experiments produced many precise and technical estimates for the effect on hours of work, but what we learned from these estimates is small in comparison to what we simply do not know about the effects of a national program on work hours. Three obstacles (that make it difficult to draw conclusions about national policy) can be understood with reference to Fig. 1. First, there was no stated agreement about what level of work disincentive would be considered acceptable. How much of a decrease in  $H$  in Fig. 1 is too much? Second, there were problems with the fallacy of composition. That is, how well the response of the treatment group to the experiment represented the response of a wider population to an actual program. How well does the experimental shift from  $A$  to  $B$  represent the true shift from  $A$  to  $B$ ? Third, the experiments measured the supply response to an NIT, but they were incapable of measuring the demand response, which made them incapable of determining the market response to an actual program. How much did the estimated shift from  $A$  to  $B$  differ from the shift from  $A$  to  $C$  that would determine the final effect on hours and costs?

The first two of these problems have been well discussed by the scholars who wrote about these results, but were not well understood in media reports on the experiments. The third received only minor discussion by academics and virtually no discussion in the media or in Congressional testimony. The rest of the section discusses these three problems in more detail.



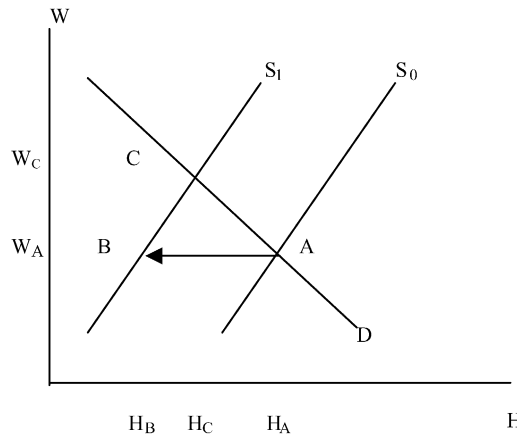


Fig. 1. The vertical axis shows the wage ( $W$ ), the horizontal axis shows the hours worked ( $H$ ). The work disincentive effect causes the supply among the experimental group to shift from  $S_0$  to  $S_1$ . Because the experimental group is small in comparison to the size of the market, the results would reflect a fixed-wage shift in hours worked (at  $W_A$ ) from point  $A$  to point  $B$ , which involves only a decline in hours and no increase in the wage. If all workers in the market received the NIT, there would be a movement along the supply curve. The market outcome would go from  $A$  to  $C$  instead of  $A$  to  $B$ , increasing the wage to  $W_C$  and partially offsetting the decrease in hours worked by difference between  $H_C$  and  $H_B$ .

### 2.1. *The lack of an agreed acceptable level of work-disincentive*

Many of the authors who have written on these experiments have complained that there was no criteria laid down for what decline in work–effort would be considered acceptable. Although this fact allowed sides could claim that the results vindicated their beliefs, there are two reasons why this criticism of the experiment is overstated: The experiments did give conclusive answers to several objective questions, and the goal of the experiments was inquiry; they were not expected to be a precursor to immediate implementation if work effort declined by less than a percentage. The NIT was simply a policy that Congress was interested in learning more about, and in that respect there was no need for a simplistic yes-or-no result.

There were, in fact, three objective yes-no questions about the work–effort response that the experiments answered quite well, all of which are very important to the BIG debate: First, would a large number of people respond to an NIT by withdrawing entirely from the labor force? The experiments found no evidence of such behavior. Some of the experimenters said that they were unable to find even a single instance of labor-market withdrawal (Levine et al., forthcoming). Second, would the work–effort response be large enough to threaten the financial viability of an NIT? The experiments found no such evidence. Third, would there be any work–effort response? The experiments found that there was a non-negligible work–effort response.

There is a large range between a negligible work-disincentive and one that is so large that it makes the experiments unaffordable. Most researchers who worked on the experiments were not surprised that the results fell into that range, and it simply means that anyone who

reads the results must make a judgment about them. That judgment is a matter of an opinion, about which people are likely to disagree. Therefore, the experiments gave both sides the ability to judge the results favorably.

## 2.2. *The fallacy of composition and the representativeness of the experiments*

The representativeness of the experimental results was affected both by sampling and by the extent to which the experiments could replicate an actual policy change.

The experiments did not draw a random sample of data. Only low-income families were tested; most of the experiments sampled only families with incomes below 150% of the poverty line. Gary and SIME/DIME sampled higher income participants but only in small numbers. Because only low-income families were tested, most of the experimental families did not have the kind of jobs that gave them a reason to stay committed to the labor force. Such families have a greater incentive and a greater ability to withdraw from the labor force than families with better paying, more secure jobs. This method of drawing the sample does not make the experiments “wrong” it merely means that they focused on the reaction of the poorest segment of the labor force, and must be read accordingly. Moffitt (1979b) estimated that the labor supply response of eligible low-income individuals would be  $-4.5\%$  but the response of the labor market as a whole would be only  $-1.6\%$ . However, it should be noted that a response by higher-income people, if there is one, has greater effect both for the efficiency cost and the tax cost of an NIT.

Participants were not randomly assigned to treatment groups. In order to reduce the costs of the experiments, the researchers tended to assign those with higher pretax incomes more generous programs (higher levels of  $G$  and lower  $t$ ). This strategy enters an important bias into the estimated responses to these parameters.

Many of the results are not attributable to the NIT per se but to the fact that most of the NIT plans tested were more generous than the existing welfare programs that the control group was eligible for (Robins and West, 1980b). Burtless (1986) observed that the average tested program was much larger than anything likely to be introduced and therefore overstated the work–effort response. The question of whether an NIT system or conditional welfare system or a similar size would have a larger work disincentive is still unanswered.

Few if any single, childless individuals were sampled. This is the group might have a larger work–effort response, because (aside from Food Stamps) they were not eligible for any non-work-based benefits, as parents were at the time.

The experiments measured the short-run response to a temporary change in policy, but we really want to know the long run response to a permanent change in policy. This problem could mean that the experiments either overestimated or underestimated the work-disincentive effect. As Harold Watts described it, an experimental plan that recipients know will be in place for only a few years, is the equivalent of putting leisure time on sale: When laundry soap is on sale, people buy more of it, and we can expect a similar response when leisure is on sale. People, who might want to take a few weeks or months off work sometime in the next 10 years, might as well take it while the experiment is going on (Levine et al forthcoming). On the other hand, because the experiments were only temporary, recipients knew that they had to return to the workforce eventually, and might have been less likely to drop out for fear of losing work experience or losing their place in line for promotion. It is

questionable whether many of the recipients had jobs that elicited such loyalty to the labor market, but arguably a permanent NIT could give workers a disincentive toward building the kind of attachments to the labor force that might lift them well out of the bottom of the income distribution later in life. The possibilities for biases in either direction do not necessarily cancel each other out, but they do show that those who make claims that the long-run effect is certainly larger than the experimental effect (Burtless, 1986; Anderson and Block, 1993) are making claims that are not supported by evidence or theory.

Metcalf (1974), Ashenfelter (1978) and Robins (1984) discussed the problem of limited-duration experiments and efforts to solve it. The best evidence on this issue provided by the experiments comes from the SIME/DIME “20-year” recipients. It is unclear whether these recipients believed the experiment would last for 20 years, and they would have been wise not to, as it was cancelled after 9 years. These recipients did not behave terribly different from other experimental group (Robins, 1984), but even if the experiment had gone on for the full 20 years it could not have estimated everything we want to know about long-term and cultural effects of an NIT.

Other problems included Hawthorne effects, complicated experimental rules, attrition, and underreporting of income by the experimental group. Hawthorne effects are changes in behavior that resulting from being watched and/or from trying to influence outcome of an experiment. Ferber and Hirsch (1978) argued that many participants did not seem to understand the eligibility rules. Attrition is likely to lead to bias towards exaggerating the value of the work-disincentive effects because those who worked the least had the most to gain by remaining in the experiment. Underreporting is important because the control group had no incentive to misrepresent their private income, while the experimental group did (Greenberg et al., 1981). They may also have had a greater ability to get away with underreporting than they would if an actual policy were in place. Ashenfelter (1986) speculates that underreporting might have been the main cause of the difference in reported income between the control and experimental groups, which would greatly bias the results toward over estimation of the work-disincentive effects.

### 2.3. *The inability of the experiments to measure the demand response*

The researchers involved were clearly aware of the absence of a demand response and of its theoretical importance, but with few exceptions (such as Browning, 1971; Greenberg, 1983) it received little attention in the literature. To determine the market effect, researchers would have to know the elasticities both of labor supply (which the experiments estimated) and of labor demand (which the experiments could not estimate). The following analysis assumes no unemployment. If unemployed workers replace the work reductions for NIT recipients, the effect of an NIT on total labor hours, output, and the efficiency cost of an NIT will be mitigated, but the effect on the labor hours of recipients and on the tax cost will not be mitigated.<sup>7</sup>

Examining the extreme cases reveals the range of possible outcomes. Fig. 2 shows the effects of a completely inelastic demand for labor. In this case, firms need a fixed

<sup>7</sup> See Greenberg (1983) for a more detailed discussion of this issue in the context of unemployment.

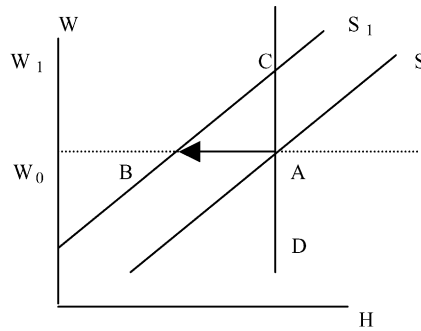


Fig. 2. If demand is completely inelastic, there is no equilibrium reduction in work hours.

amount of workers and will pay whatever they must to get it. If so, no amount of labor-disincentive effect will cause any long-run decrease in work effort; the entire result of the work-disincentive effect would be to raise wages; and there would be no equilibrium decline in hours worked and no efficiency cost. Fig. 3 shows that, if the demand for labor is perfectly elastic (if firms will hire any amount of labor at the going wage, but won't pay even a cent more for it), the market equilibrium will be entirely determined by the horizontal shift in the supply of labor just as measured by the experiments.

The more general results are that the equilibrium level of work effort will be somewhere between the initial equilibrium (point A) and the horizontal shift in supply (point B), and that the equilibrium wage will be as high or higher than the initial wage. In other words, the market equilibrium will be somewhere in the shaded area in Fig. 4. Without information on elasticities, it is impossible to say precisely where in this region the equilibrium would be. Thus, instead of estimating the equilibrium outcome of a negative income tax, the experiments estimated *the boundary of a region of possible outcomes*.

It should be noted that it is theoretically possible for the equilibrium point to be in the region to the upper left of point B if the labor supply is backward bending. However, backward bending requires that workers' demand for goods is so inelastic that a decrease in

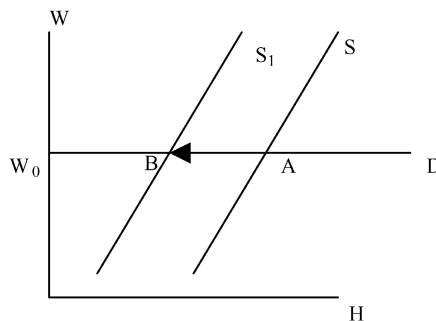


Fig. 3. If demand is completely elastic, there is no change in the wage, and the full reduction in work hours in the experiments would occur in the market.

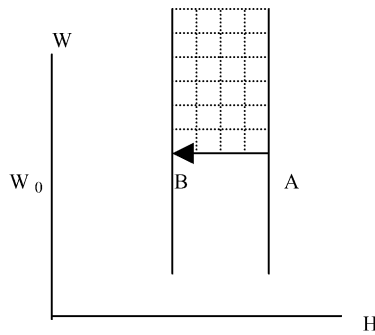


Fig. 4. The range of possible market responses to a given horizontal shift in the supply of labor.

wages will cause them to work more hours to maintain their level of consumption. That is quite reasonable for someone whose labor is the primary or the only source of income. But if a generous guaranteed income is in place, a lower wage reduces the portion of income attributable to work. It becomes unlikely that workers will work more and more to maintain the level of a smaller and small part of their income. Therefore, it is unlikely that labor supply would backward bend for workers in the low wage market when a substantial NIT exists. Also, if it did exist it would be likely to lead to a very large increase in wages as the backward bending supply forced the price farther up the supply curve.

If a backward bending labor supply is ruled out, the lack of ability of the experiments to estimate the market response to a guaranteed income has several important effects on the estimates:

- The reduction in labor hours would be smaller than estimated by the experiments.
- The increase in income of recipients (and therefore) the effect of the program on poverty would be larger than estimated (via increased wage rates).
- The cost of the program in terms of tax dollars would be smaller than estimated.
- The efficiency loss of the program would be smaller than estimated.
- The increase in wages would create a cost to firms that the experiments could not estimate.

In other words, the experiments found upper-bound estimates for the decline in hours worked, lower-bound estimates for the effect of the program on the income of recipients, upper-bound estimates for the cost of the program in terms of tax dollars and efficiency loss, and no estimate of the cost of the guaranteed income in terms of higher wages.<sup>8</sup>

Given this inherent limitation of the experiments, there are two reasonable ways to present results: One is to obtain the best available estimates for the elasticities and simulate the outcome (Betson et al., 1980, 1981; Betson and Greenberg, 1983; Greenberg, 1983). The other is to present them as what they were: estimates of the boundaries of a range of possibilities. Instead, as shown in Section 3, demand effects were sometimes ignored and often treated with a small caveat. When treated with a caveat it was often included on a list of things that could bias the estimates, such as factors mentioned in Section 2.2, but

<sup>8</sup> This is not an economic cost, of course. But it is a cost to an interest group that might interest policymakers.

few brought attention to the important difference between those biases and the difference between a point estimate and an estimate of the boundary of a range.

### 3. The work-disincentive results of the experiments

Nearly half of the scholarly articles on the negative income tax experiments deal in some way with empirical results for work incentive effects, and many of those present original estimates. Table 2 summarizes the findings of several of the studies on the work–effort response to the NIT experiments, giving the difference in hours worked by the experimental group relative to the control group in hours per year and in percentage terms. Results are reported for three categories of workers, husbands, wives, and “single female heads” (SFH).<sup>9</sup> Data was also collected for the work effort of youths, but is omitted from this table in the interest of brevity.<sup>10</sup> The five experiments found a range of work–effort reduction from –0.5% to –9% for husbands, which corresponds to a reduction of about 0.5–4 h per week, 20–130 h per year, or 1–4 fulltime weeks per year. The three studies averaging the results from the four U.S. experiments (Robins, 1985; Burtless, 1986; Keeley (1981a) and Keeley (1981b)) found work reduction effects of 5%, 7% and 7.9%, respectively.

The response of wives and single mothers was somewhat larger in terms of hours, and substantially larger in percentage terms because they tended to work fewer hours to begin with. Wives reduced their work effort by 0–27% and single mothers reduced their work effort by 15–30%. These percentages correspond to reductions of about 0–166 h per year. The labor market response of wives had a much larger range than the other two groups, but this was usually attributed to the peculiarities of the labor markets in Gary and Winnipeg where particularly small responses were found.

Robins (1985), Robins and West (1980a,b), and Moffitt (1979a) all clearly present their findings as the difference between the labor supply of the treatment group and the control group, which should avoid any confusion with broader labor market findings to anyone who understands the difference, and one would expect everyone who reads technical articles is likely to understand. Others added a simple caveat (Keeley et al., 1978a; Moffitt, 1979b), but some were not as careful to avoid confusion. Orcutt and Orcutt (1968) claimed that the experiments could produce unbiased estimates of the disincentive effects and earnings effects of an NIT, when the lack of a demand response clearly makes this impossible (Browning, 1971). Ferber and Hirsch (1978, p. 1385) referring to the “labor supply response” as the “labor market response” despite explaining the difference later in the article. Kelly and Singer (1971) write, “No experiment paper should be complete without mention of possible response bias,” but do not mention the experiment’s inability to measure demand response as a source of bias. West (1980b, p. 642) mentions three ways NIT can affect wages without mentioning the demand response. Most of these slips are small, but the omission of demand is more significant when researchers attempt to carry the results over to the cost of a national program.

<sup>9</sup> Meaning women with children and no husband.

<sup>10</sup> Youths tended to have work–effort responses comparable in percentage terms to wives and single mothers. It was not correlated with an increase in school attendance, but was correlated with an improvement in school performance.

Table 2  
Summary of findings of work reduction effect

Study	Data source	Work reduction* in hours per year** and percent			Comments and caveats
		Husbands	Wives	SFH	
Robins (1985)	4 U.S.	-89	-117	-123	Study of studies that does not assess the methodology of the studies but simply combines their estimates. Finds large consistency throughout, and "In no case is there evidence of a massive withdrawal from the labor force." No assessment of whether the work response is large or small or its effect on cost. Estimates apply to a poverty-line guarantee rate with a marginal tax rate of 50%.
Burtless (1986)	4 U.S.	-5% -119	-21.1% -93	-13.2% -79	Average of results of the four US experiments weighted by sample size, except for the SFH estimates, which are a weighted average of the SIME/DIME and Gary results only.
Keeley (1981)	4 U.S.	-7% -7.9%	-17%	-7%	A simple average of the estimates of 16 studies of the four U.S. experiments.
Robins and West (1980a)	SIME/DIME	-128.9	-165.9	-147.1	Estimates "labor supply effects." It goes without saying that this is different from "labor market effects."
Robins and West (1980b)	SIME/DIME	-7% -9%	-25% -20%	-15% -25%	Recipients take 2.4 years to fully adjust their behavior to the new program.
Cain et al. (1974)	NJ	-	-50	-	Includes caveats about the limited duration of the test and the representativeness of the sample. Notes that the evidence shows a smaller effect than nonexperimental studies.
Watts et al. (1974)	NJ	-1.4% to -6.6%	-	-	Depending on size of $G$ and $t$ .
Rees and Watts (1975)	NJ	-1.5 hpw**	-0.61%	-	Found anomalous positive effect on hours and earnings of blacks.
Ashenfelter (1978)	RIME	-0.5% -8%	-27%	-	"There must be serious doubt about the implications of the experimental results for the adoption of any permanent negative income tax program."
Moffitt (1979a)	Gary	-3% to -6%	0%	-26% to -30%	No caveat about missing demand, but careful not to imply the results mean more than they do.
Hum and Simpson (1993a)	Mincome	-17	-15	-133	Smaller response to the Canadian experiment was not surprising because of the make-up of the sample and the treatments offered.
		-1%	-3%	-17%	

\*The negative signs indicate that the change in work effort is a reduction; \*\*hours per year except where indicated "hpw;" hours per week. NJ, New Jersey Graduated Work Incentive Experiment; SIME/DIME, Seattle/Denver Income Maintenance Experiment; Gary, Gary Income Maintenance Experiment; RIME, Rural Income Maintenance Experiment; Mincome, Manitoba Income Maintenance Experiment; SFH, single female "head of household".

Table 3 reports some of the labor market findings other than the simple difference between the hours worked by the treatment and control groups. Robins et al. (1980a) and Robins et al. (1980b) and Tuma and Robins (1980) found that the percentages are much larger if labor response is considered in terms of the increase in the length of spells out of work or the rate at which people who aren't working return to employment. These results largely reflect the fact that the reduction in labor hours was not primarily caused by workers reducing their hours of work each week but by remaining nonemployed longer if and when they became nonemployed. Increased periods of nonemployment might have an efficiency benefit if they lead to better matches between workers and firms.

Several studies estimating the additional tax cost caused by the work–effort response found widely divergent results. Rees and Watts (1975) estimated it would add 5% to 10% to the tax cost of the program. Ashenfelter (1978) estimated that the cost of the program without labor market effects would be 78% of cost with labor market effects, which is equivalent to saying that the reduction in work effort would increase the tax cost of the program by 28%. Keeley et al. (1978a) estimated that the labor supply response would account for 23–55% of total program costs (equivalent to an increase of 30–122%). Burtless (1986) estimated that work disincentive would nearly triple the tax cost of the program. All of these studies neglect the demand response, implicitly assuming that demand is completely elastic. Rees and Watts's conclusion is that the costs are small and so apparently don't think it necessary to say that a demand response might make the costs even smaller. Only Keeley et al. (1978a,b) explicitly make the assumption of perfectly elastic demand. They admit that this reduces the accuracy of the results, and justify the assumption by speculating that employers could easily replace NIT recipients with workers who are not covered by the program.

Most of the studies that did include a demand response used data from the NIT experiments to examine particular changes in policy such as Carter's Program for Better Jobs and Income (Betson et al., 1980a,b; Betson and Greenberg, 1983), and so are not very useful for correcting cost estimates of an NIT for demand responses. Only Greenberg (1983) applied a microsimulation model with a demand effect to the cost of an NIT as examined in the experiments. He found that a wage response could slightly mitigate the effect on hours and costs but the general pattern remained in which a dollar spent on poverty reduction raises the incomes of the poor by less than a dollar,<sup>11</sup> but his results are tentative because they depended on assumptions about the elasticity of demand, the level of unemployment and the substitutability between NIT recipients and other workers (Greenberg, 1983). Bishop (1979) used a general equilibrium framework to examine the impact of several antipoverty programs including NIT on efficiency. The focus on efficiency rather than tax cost means that his results are not directly comparable to the others, but he finds that the NIT would produce a demand response that would increase wages and therefore it would reduce both the efficiency loss and the tax cost of the program. Unfortunately there do not seem to be any articles employing a demand response in otherwise comparable models that generate comparable estimates of tax cost, hours worked, efficiency lost, and impact on inequality.

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<sup>11</sup> Personal correspondence.



Table 3  
Labor market findings other than simple work–effort reduction

Study	Data source	Findings	Comments and caveats
Robins et al. (1980a,b)	SIME/DIME	Increase in length of spells out of employment: husbands: 9.4 weeks, 27%; wives: 50 weeks, 42%; single females: 56 weeks, 60%	The experimental group was somewhat more likely to leave employment and substantially more like to remain nonemployed for longer spells than the control group.
Tuma and Robins (1980)	SIME/DIME	Change in rate of entering employment: husbands: –22.2, wives: –39.6, single female heads: –35.4	Conditional having become nonemployed. This reflects the fact that the labor-hours reductions were attributable more to longer spells of unemployment than to reductions in weekly hours of work.
Hall (1975)	NJ	Opt out rate: 125-50 plan: 13%; 100-50 plan: 25%; 50-50 plan: 94%	These are the percentages of participants in the study who received no benefits. But the results depend substantially on the participants pre-experimental income.
Robins (1984)	SIME/DIME	Does not find evidence that 3-year and 5-year studies were biased relative to the response of the 20-year treatment group.	The available evidence is limited.
Cogan (1983)	NJ	Husbands reduce labor effort by –5 to –7 h per week, conditional on participation	This estimate was only for the sub-sample of that actually received payments and so is not directly comparable to the estimates of labor response in Table 2.
Moffitt (1979b)	Gary	Eligible low income population: –4.5%	Simulation model, does not take demand into account, but warns, “Assuming the labor-supply curve is forward-sloping, which it probably is at low age rates, the experimental estimates over-state the final impact on employment (due to a demand response).”
Keeley et al. (1978b)	SIME/DIME	Total population: –1.6% The effect of an NIT on labor supply could be offset by unemployed workers if there is sufficient slack in the labor market. Predicted labor supply response of a national program: husbands: –5.3%, wives: –22.0%, SFH: –11.2%	Applies the experimental parameters for labor supply functions to a national data base to obtain estimates of the nationwide aggregate labor effect and so these findings are not directly comparable to those in Table 2. Finds that the results vary wide with the generosity of the program.
Greenberg (1983)	SIME/DIME	Response of the demand for labor had a small mitigating effect on hours.	Results depended on assumptions on the level of unemployment and the elasticities of demand and supply of labor and the substitutability and availability of workers making similar wages to those eligible for NIT.

Keeley et al. (1978a)	SIME/DIME	Labor Supply response accounts for 23–55% of programs with a positive net cost. That is, cost before labor supply response is 45–77% of total cost.	Range depends on the size of $G$ and $t$ . Justifies the assumption of perfectly elastic demand on employers' ability to substitute high-wage, high-skilled workers for workers who are likely to be affected by an NIT.
Robins (1980a)	SIME/DIME	Replacement of the 1974 welfare system with an NIT would have cost an additional \$2.2 billion to \$30 billion (\$55 to \$97 in 2004 dollars). The work–effort response would add \$0.2–\$7.0 billion (\$0.6–\$23 in 2004 dollars) to cost.	Range of responses depends on the size of $G$ and $t$ . Demand response not included.
Rees and Watts (1975)	NJ	Increase tax cost due to supply response: 5–10%	Demand response not included.
Ashenfelter (1978)	RIME	Estimates that the cost before the labor supply response would only 78% of the cost after the labor supply response.	Demand response not included. Findings could be restated to say that the work–effort response adds 28% to the transfer cost.
Burtless (1986)	4 U.S.	\$3 in transfers raises the income of recipients by only \$1. Poverty among all families with children could be eliminated for an additional cost of \$61 billion (\$98 in 2004 dollars).	Demand response not included.
Maxfield	SIME/DIME	Labor supply response is highly correlated to the generosity of the NIT program.	Demand response not included.
Bishop (1979)	SIME/DIME	“Reduction in labor supply produced by these programs does tend to raise low-skill wages, and this improves transfer efficiency.”	General equilibrium model focusing on efficiency effects, and so results are not directly comparable to those focusing on tax cost. Results are sensitive to assumptions.

These results are not extremely divergent or controversial, and they are not terribly conclusive on the issue of whether the government should introduce a basic income guarantee, but they can be spun to make an apparently strong case either for or against it. Most of the scholarly works did not seem to consciously spin the results with a few exceptions such as [Burtless \(1986\)](#) and [Anderson and Block \(1993\)](#). Although Burtless displays knowledge of the difficult issues involved in the experiments, he betrays an effort to nudge the conclusion in direction. He declares a 7% decline in work effort to be “large.” He discusses various biases in the estimation of labor supply that point in both directions, but hastily concludes that the balance the labor supply effects are overestimated, and fails to recognize the significance of underreporting bias ([Ashenfelter, 1986](#)). He does not mention that his cost estimate is substantially larger than any of the others, and he does not mention that it is biased by the omission of a demand response. [Anderson and Block \(1993\)](#) seem to use [Burtless \(1986\)](#) as their primary source, but make a one-sided representation even of his account, omitting many of his caveats and clarifications. They go farther than Burtless by attributing poverty to a lifestyle “choice” on the part of recipients because so many people in poverty do not work, ignoring such a basic economic concept as *unemployment*. They ignore the demand side of the labor market, failing to note that poverty also represents the “choice” of employers in the low-wage sector who pay wages that leave workers in poverty even if they work fulltime. Anderson and Block’s normative and positive arguments are both one-sided and therefore not very valuable.

Despite these two exceptions, the presentation of the data in the official reports and in most published works was good science and not political spin. But as Section 4 shows, once that data made its way into the public arena, it was spun anyway.

#### 4. Political and media perceptions of the experiments

Hopefully, Sections 2 and 3 have demonstrated that the findings of the NIT experiments are far more complex, subtle, and ambiguous than one might be led to believe by findings such as an X% decline in hours worked. But as this section shows, the complexity of the results was largely lost on politicians and members of the media to whom the findings were reported. Bibliography A contains a survey of about 50 articles from the popular media on the experiments.

The experiments gained significant attention in the press only twice. In 1970–1972, when Nixon’s Family Assistance Plan (FAP) was under debate in Congress, and in 1977–1978 when Carter’s Program for Better Jobs and Income (PBJI) was under consideration. Both plans had elements of a negative income tax; neither was a pure guaranteed income, although FAP was considerably closer to it than PBJI. In 1970, the first experiment had only been under way for 2 years and researchers believed that they were at least 3 years away from being able to produce meaningful results, but at the insistence of the administration and some members of Congress, the researchers released preliminary reports showing no evidence of any work disincentive effect.<sup>12</sup> Some other members of Congress (rightly) could not

<sup>12</sup> The reason that the preliminary reports so greatly underestimated the work–effort reduction was probably that workers took several years to adjust their behavior to the new policy (see [Robins and West, 1980b](#)).

believe the result, and commissioned a review of the results from an independent auditor that concluded the results were “premature,” which was just what the researchers had initially warned.

Results of the fourth and largest experiment, SIME/DIME, were released while Congress was debating PBJI. Dozens of technical reports with large amounts of data were simplified down to two statements: It decreased work effort and it supposedly increased divorce. The small size of the work disincentive effect that pleased so many of the researchers hardly drew any attention. Never mind that everyone going into the experiments agreed that there would be some work disincentive effect; members of Congress were appalled; and columnists across the country responded with a chorus of negative editorials decrying the guaranteed income and ridiculing the government for spending millions of dollars to find out whether people work less if you pay them not to work.

The [United Press International \(1977\)](#) simply got the facts wrong saying that the SIME/DIME study showed that “adults might abandon efforts to find work.” The UPI apparently did not understand the difference between a decline in work hours while continuing to work, and abandoning the labor market. The *Rocky Mountain News* claimed that the NIT “saps the recipients’ desire to work.” Jones (1977) writing for the *Seattle Times* presented a relatively well-rounded understanding of the results, but despite this, simply concluded that the existence of a decline in work effort was enough to “cast doubt” on the plan. Similarly Rich (1978, November 18) implied that evidence showing the NIT “might cause recipients to work less” is enough to disqualify the program from consideration. [Raspberry \(1978\)](#) declared the experiments a failure simply because people worked less.

Senator Daniel Patrick Moynihan who had written a book in support of the guaranteed income a few years early and who had been one of the architects of FAP, recanted his support for the guaranteed income as a result of the SIME/DIME findings. He is a sociologist and would be expected to have a sophisticated understanding of statistical data, but he implied in a letter to William F. Buckley later published by the *National Review* that the mere existence of a work disincentive effect was an important factor in his recantation. He stated, “But were we wrong about a guaranteed Income! Seemingly it is calamitous. It increases family dissolution by some 70%, decreases work, etc. Such is now the state of the science, and it seems to me we are honor bound to abide by it for the moment.” He held Congressional hearings on the results in November of 1978 to discuss the evidence. Although a large amount of good information was presented ([U.S. Senate, 1978](#)), media reports and politicians’ comments on the experiments did not betray a real understanding of the findings.

Headlines such as “Income Plan Linked to Less Work,” and “Guaranteed Income Against Work Ethic” appeared in newspapers following the hearings. The [Knight News Service \(1978\)](#) quoted Jodie Allen of the Labor Department commenting on Spiegelman’s cost estimates saying, “It could easily turn out that the government might spend billions of dollars on benefit payments and have little effect on the families’ incomes. Instead, most of the (government) expenditures would offset reductions in earnings.” Only a few exceptions such as Carl Rowan for the *Washington Star* (1978) considered that it might be acceptable for people working in bad jobs to work less, but he could not figure out why the government would spend so much money to find out whether people work less when you pay them to stay home.

Spiegelman, one of the directors of SIME/DIME, defended the experiments in the *Washington Star* (1978), saying that the experiments provided much needed costs estimates that demonstrated the feasibility of the NIT. He said that the decline in work effort was not dramatic, and could not understand why so many commentators drew such different conclusions than the experimenters. Demokovich (1978) was one of the few popular writers who considered the work–effort reduction to be small, but the more common reaction was given by Senator Bill Armstrong of Colorado Citing only that a work disincentive effect existed, Armstrong said the experiment was, “An acknowledge failure. Let’s admit it, learn from it, and move on” (Brimberg, 1980).

The scientists who presented the data were not entirely to blame for this misunderstanding, as Burtless (1986) remarked, “Policymakers and policy analysts . . . seem far more impressed by our certainty that the efficiency price of redistribution is positive than they are by the equally persuasive evidence that the price is small.” It may be an impossible task to communicate such complexities to an audience interested only in sound bytes or in the bottom line, but social scientists have a responsibility to do a better job than we did in this instance. The understanding of the NIT experiments displayed in the popular press was superficial and obviously the result of spin. Few commentators kept figures like 5–7% in perspective. None of the articles in the popular media that I was able to find betrayed any understanding that the experiments measured only the horizontal shift in the labor supply function. None seemed to understand the elementary economic principle that a change in supply necessitates a demand response that can greatly affect the equilibrium outcome.

## 5. Conclusion

It would be very easy to spin on the results in either direction. A positive spin would focus on the size of the work disincentive effects. The experiments clearly contradicted two of the most common arguments against a basic income guarantee: The experiments found *no evidence* that a negative income tax would cause some segment of the population to withdraw from the labor force, and the experiments found *no evidence* that the supply response would increase the cost of the program to the point that it would be unaffordable (even ignoring the mitigating demand response). Certainly, some level of  $G$  would make an NIT untenable, but the results implied that a guarantee level as high as 150% of the official poverty level would be well within the bounds of financial feasibility. Also, the experiments predicted that the full labor market response in the work hours of primary income earners would fall into a range of about 0–5% or 0–7% and where in that range it fell would depend on the elasticity of demand for labor. The reduction in work hours could be called “small,” and it could be mentioned that it would have the side benefit of increasing wages, further reducing poverty and inequality.

A negative spin would require a focus on three facts: First, there was a statistically significant work disincentive effect, allowing willing laypersons to draw the fallacious conclusion that there was therefore a substantively significant work disincentive effect. Second, work reductions of 5–7% among primary earners in two-parent families and reductions of up to

27% for other earners could be called “large.” Third, the work disincentive increased the cost of the program over what it would have been if work hours were unaffected by the NIT. Estimates of the added cost vary from 10% to 200%, and it is not difficult to focus on the larger estimates.

Even if the public had been made to understand more of the complexities of results, as long as there is a significant political block believing that any work disincentive is unacceptable, the NIT experiments were bound to give ammunition to NIT opponents. To that extent it was a mistake for any guaranteed income supporters to agree to the experiments in the first place. Reichauer (1986) asked what would have happened if the introduction of Social Security had been preceded by a similar experiment? It would certainly have shown that people saved less for their retirement, retired sooner than they otherwise would have, and relied less on traditional feelings of family responsibility for elders. Such findings would have challenged prevailing norms and would have given considerable ammunition to Social Security opponents. But there is a danger in focusing too much on the strategic value of the experiments to supporters and opponents. There is more to scientific inquiry than political advantage. The experiments were not a propaganda device, and although what we learned from them was tentative and limited, it is worth knowing.

Why was the limitation of a missing demand response treated so lightly? Perhaps, as a general trait, scientists like to focus on the results of their research, not its limitations. Perhaps, those presenting the data might have assumed this fact was too obvious to be bothered with among social scientists or too difficult to be dealt with by a lay audience. Perhaps, opponents didn’t want to bring it up because it waters down their argument that the work disincentive is “large” and the costs are “high.” Perhaps, supporters didn’t want to bring it up because it is easier to make the case that the work-disincentive is “small” than to make a case that a work disincentive would have a desirable effect on wages. Using the small argument requires only an objective look at empirical evidence—if one can objectively define small. But using the desirability argument requires not only empirical data that the experiments could not produce, but also a much more complex normative argument. It affronts those who want to keep wages low to keep profits high and those who espouse the extreme version of the work ethic stating that everyone without property must at all times even at poverty wages.

To those who believe that low-wage workers need more power in the labor market, the NIT experiments demonstrated the feasibility of a desirable program. To those who believe all work-disincentives are bad, the experiments demonstrated the undesirability of a well-meaning program. These normative issues separate supporters from opponents of the basic income guarantee, and therefore, the NIT experiments, as long as they are discussed, will always mean different things to different people. Either side can spin the results, but that’s not how science should be used. It is better to understand that the NIT experiments were able to shed a small amount of light on the positive issues that affect this normative debate. They were able to indicate only that a basic income guarantee is financially feasible at a cost of certain side effects that people with differing political beliefs may take to be desirable or disastrous. To claim more would be to overstate the evidence.

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<sup>13</sup> I’m sure I missed some. There is some repetition of papers published both as journal articles and as book chapters, and there was some subjectivity in the judgment of what constitutes “largely” and “published”—my apologies for any omissions. In addition to the published articles, there are at least 200 more unpublished memorandums, reports, discussion papers, and other unpublished works on the experiments as well. Many (but not all) of the unpublished articles were simply early version of later published works. For a bibliography including many of the unpublished articles on the NIT experiments, see the working paper version of this article: USBIG Discussion Paper No. 38, “A Failure to Communicate: The Labor Market Findings of the Negative Income Tax Experiments and their Effects on Policy and Public Opinion” at <http://www.usbig.net>.

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