



ELSEVIER

The Journal of Socio-Economics xxx (2004) xxx–xxx

---

---

**The Journal of  
Socio-  
Economics**

---

---

www.elsevier.com/locate/econbase

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

Karl Widerquist\*

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

---

7 **Abstract**

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

16 © 2004 Published by Elsevier Inc.

17 *JEL classification:* I3; J2; J3

18 *Keywords:* Basic income; Negative income tax; Experiment; Redistribution

---

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

---

\* 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.

---

<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.

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

## 71 1. The experiments

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

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

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

93 The effects of the negative income tax on health, homeownership, low-birthweight,  
94 school performance, and other indicators of the well-being of recipients were tested and  
95 reported in many studies (Avrin, 1980; Boumol, 1974, 1977; Bradbury, 1978, 1986; Cain,  
96 1977; Elesh and Lefcowitz, 1977; Hanusheck, 1986; Kaluzny, 1979; Keeley, 1980c, 1980d;  
97 Kehrer and Wolin, 1979; Kerachsky, 1977; Knudsen et al., 1977a,b; Ladinsky and Wells,  
98 1977; Lefcowitz and Elesh, 1977; Mallar, 1977; Masters, 1978; Maynard, 1977; Metcalf,  
99 1977a; Michael, 1978; Middleton and Allen, 1977; Murnane et al., 1981; Nicholson, 1977b;  
100 O’Connor et al., 1979; Ohls, 1980; Poirier, 1977a,b,c; Pozdena and Johnson, 1980; Rea,  
101 1977; Robins, 1980b; Rossi, 1975; Thoits and Hannan, 1980; Weiss et al., 1980; Wooldridge,  
102 1977). Most of these studies show positive effects, even for hard-to-change variables such  
103 as school performance and low birthweight, but discussion of these effects is beyond the  
104 scope of this paper. For an overview of some of these effects see Levine et al. (2004).

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

107 1986; Groeneveld et al., 1980a,b, 1983; Hannan et al., 1977, 1978; Hum and Choudry,  
108 1992; Tuma, 1986, but these findings are also beyond the scope of this paper). See Hannan  
109 and Tuma (1990) and Cain and Wissoker (1990a,b) for two sides of this debate.

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

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

130 The largest NIT experiment was the Seattle/Denver Income Maintenance Experiment  
131 (SIME/DIME), which had an experimental group of about 4800 people in the Seattle and  
132 Denver metropolitan areas. The sampled included black, white, and Latino, families with  
133 at least one dependent and incomes below \$11,000 for single-parent families and below  
134 \$13,000 for two-parent families. The experiment began in 1970 and was originally planned  
135 to be completed within 6 years. Later, researchers obtained approval to extend the experiment  
136 for 20 years for a small group of subjects. This would have extended the project into the  
137 early 1990s, but it was eventually cancelled in 1980, so that a few subjects had a guaranteed  
138 income for about 9 years, during part of which time they were led to believe they would  
139 receive it for 20 years.

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

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

---

<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
					0.75	0.4
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
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	1.0	0.6
					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.

185 SIME/DIME tested two nonlinear income functions with marginal tax rates of 0.7 minus  
186 0.025 times income and 0.8 minus 0.025 times income. The effect of these two nonlinear  
187 functions was to impose higher marginal tax rates on lower levels of income and lower  
188 marginal tax rates on higher levels of income.

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

194 **Table 1** summarizes the configuration of the experiments.

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

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

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

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

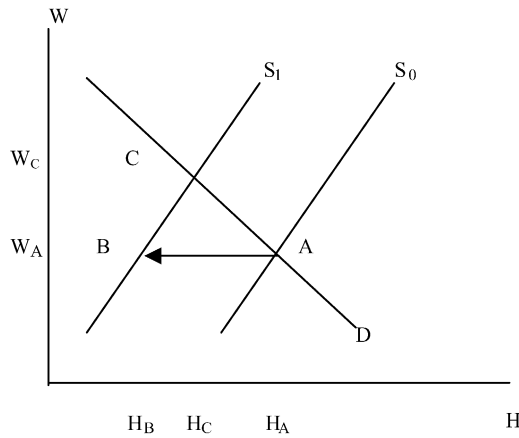


Fig. 1. The vertical axis shows the wage ( $W$ ), the horizontal axis shows the hours worked ( $H$ ). The work disincentive effect causes the supply among the experimental group to shift from  $S_0$  to  $S_1$ . Because the experimental group is small in comparison to the size of the market, the results 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

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

## 251 2.2. *The fallacy of composition and the representativeness of the experiments*

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

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

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

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

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

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

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

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

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

### 319 2.3. *The inability of the experiments to measure the demand response*

320 The researchers involved were clearly aware of the absence of a demand response and  
321 of its theoretical importance, but with few exceptions (such as Browning, 1971; Greenberg,  
322 1983) it received little attention in the literature. To determine the market effect, researchers  
323 would have to know the elasticities both of labor supply (which the experiments estimated)  
324 and of labor demand (which the experiments could not estimate). The following analysis  
325 assumes no unemployment. If unemployed workers replace the work reductions for NIT  
326 recipients, the effect of an NIT on total labor hours, output, and the efficiency cost of an  
327 NIT will be mitigated, but the effect on the labor hours of recipients and on the tax cost will  
328 not be mitigated.<sup>7</sup>

329 Examining the extreme cases reveals the range of possible outcomes. Fig. 2 shows  
330 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.

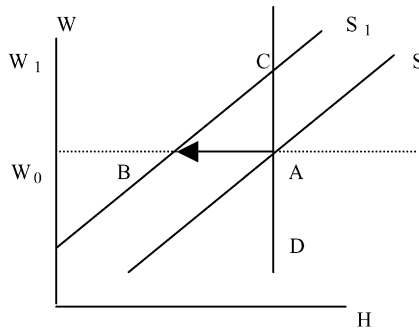


Fig. 2. If demand is completely inelastic, there is no equilibrium reduction in work hours.

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

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

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

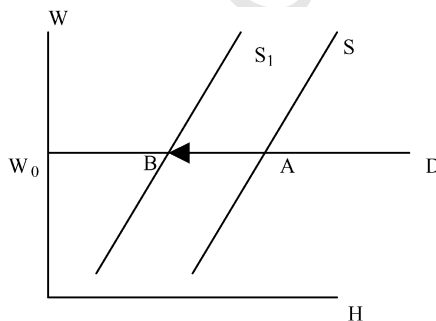


Fig. 3. If demand is completely elastic, there is no change in the wage, and the full reduction in work hours in the experiments would occur in the market.

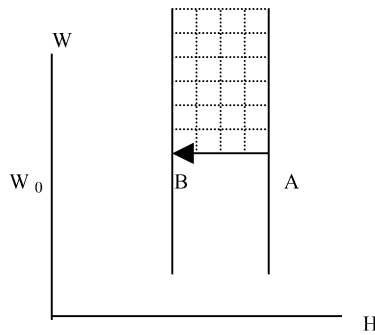


Fig. 4. The range of possible market responses to a given horizontal shift in the supply of labor.

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

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

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

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

369 Given this inherent limitation of the experiments, there are two reasonable ways to  
 370 present results: One is to obtain the best available estimates for the elasticities and simulate  
 371 the outcome (Betson et al., 1980, 1981; Betson and Greenberg, 1983; Greenberg, 1983).  
 372 The other is to present them as what they were: estimates of the boundaries of a range of  
 373 possibilities. Instead, as shown in Section 3, demand effects were sometimes ignored and  
 374 often treated with a small caveat. When treated with a caveat it was often included on a  
 375 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.

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

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

379 Nearly half of the scholarly articles on the negative income tax experiments deal in some  
380 way with empirical results for work incentive effects, and many of those present original  
381 estimates. Table 2 summarizes the findings of several of the studies on the work–effort  
382 response to the NIT experiments, giving the difference in hours worked by the experimental  
383 group relative to the control group in hours per year and in percentage terms. Results are  
384 reported for three categories of workers, husbands, wives, and “single female heads” (SFH).<sup>9</sup>  
385 Data was also collected for the work effort of youths, but is omitted from this table in the  
386 interest of brevity.<sup>10</sup> The five experiments found a range of work–effort reduction from  
387 –0.5% to –9% for husbands, which corresponds to a reduction of about 0.5–4 h per week,  
388 20–130 h per year, or 1–4 fulltime weeks per year. The three studies averaging the results  
389 from the four U.S. experiments (Robins, 1985; Burtless, 1986; Keeley, 1981a,b) found work  
390 reduction effects of 5%, 7% and 7.9%, respectively.

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

398 Robins (1985), Robins and West (1980a,b), and Moffitt (1979a) all clearly present their  
399 findings as the difference between the labor supply of the treatment group and the control  
400 group, which should avoid any confusion with broader labor market findings to anyone who  
401 understands the difference, and one would expect everyone who reads technical articles is  
402 likely to understand. Others added a simple caveat (Keeley et al., 1978a; Moffitt, 1979b),  
403 but some were not as careful to avoid confusion. Orcutt and Orcutt (1968) claimed that  
404 the experiments could produce unbiased estimates of the disincentive effects and earnings  
405 effects of an NIT, when the lack of a demand response clearly makes this impossible  
406 (Browning, 1971). Ferber and Hirsch (1978, p. 1385) referring to the “labor supply response”  
407 as the “labor market response” despite explaining the difference later in the article. Kelly  
408 and Singer (1971) write, “No experiment paper should be complete without mention of  
409 possible response bias,” but do not mention the experiment’s inability to measure demand  
410 response as a source of bias. West (1980b, p. 642) mentions three ways NIT can affect wages  
411 without mentioning the demand response. Most of these slips are small, but the omission  
412 of demand is more significant when researchers attempt to carry the results over to the cost  
413 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%	-20%	-	Depending on size of <i>G</i> and <i>t</i> .
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".

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

423 Several studies estimating the additional tax cost caused by the work–effort response  
424 found widely divergent results. Rees and Watts (1975) estimated it would add 5% to 10%  
425 to the tax cost of the program. Ashenfelter (1978) estimated that the cost of the program without  
426 labor market effects would be 78% of cost with labor market effects, which is equivalent  
427 to saying that the reduction in work effort would increase the tax cost of the program by  
428 28%. Keeley et al. (1978a) estimated that the labor supply response would account for  
429 23–55% of total program costs (equivalent to an increase of 30–122%). Burtless (1986)  
430 estimated that work disincentive would nearly triple the tax cost of the program. All of  
431 these studies neglect the demand response, implicitly assuming that demand is completely  
432 elastic. Rees and Watts's conclusion is that the costs are small and so apparently don't think  
433 it necessary to say that a demand response might make the costs even smaller. Only Keeley  
434 et al. (1978a,b) explicitly make the assumption of perfectly elastic demand. They admit  
435 that this reduces the accuracy of the results, and justify the assumption by speculating that  
436 employers could easily replace NIT recipients with workers who are not covered by the  
437 program.

438 Most of the studies that did include a demand response used data from the NIT experi-  
439 ments to examine particular changes in policy such as Carter's Program for Better Jobs and  
440 Income (Betson et al., 1980a,b; Betson and Greenberg, 1983), and so are not very useful for  
441 correcting cost estimates of an NIT for demand responses. Only Greenberg (1983) applied  
442 a microsimulation model with a demand effect to the cost of an NIT as examined in the  
443 experiments. He found that a wage response could slightly mitigate the effect on hours and  
444 costs but the general pattern remained in which a dollar spent on poverty reduction raises  
445 the incomes of the poor by less than a dollar,<sup>11</sup> but his results are tentative because they  
446 depended on assumptions about the elasticity of demand, the level of unemployment and  
447 the substitutability between NIT recipients and other workers (Greenberg, 1983). Bishop  
448 (1979) used a general equilibrium framework to examine the impact of several antipoverty  
449 programs including NIT on efficiency. The focus on efficiency rather than tax cost means  
450 that his results are not directly comparable to the others, but he finds that the NIT would  
451 produce a demand response that would increase wages and therefore it would reduce  
452 both the efficiency loss and the tax cost of the program. Unfortunately there do not seem  
453 to be any articles employing a demand response in otherwise comparable models that  
454 generate comparable estimates of tax cost, hours worked, efficiency lost, and impact on  
455 inequality.

<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).”
		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.	
Keeley et al. (1978b)	SIME/DIME	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.

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

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

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

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

486 The experiments gained significant attention in the press only twice. In 1970–1972, when  
487 Nixon’s Family Assistance Plan (FAP) was under debate in Congress, and in 1977–1978  
488 when Carter’s Program for Better Jobs and Income (PBJI) was under consideration. Both  
489 plans had elements of a negative income tax; neither was a pure guaranteed income, although  
490 FAP was considerably closer to it than PBJI. In 1970, the first experiment had only been  
491 under way for 2 years and researchers believed that they were at least 3 years away from  
492 being able to produce meaningful results, but at the insistence of the administration and some  
493 members of Congress, the researchers released preliminary reports showing no evidence  
494 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).

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

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

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

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

530 Headlines such as “Income Plan Linked to Less Work,” and “Guaranteed Income Against  
531 Work Ethic” appeared in newspapers following the hearings. The [Knight News Service](#)  
532 (1978) quoted Jodie Allen of the Labor Department commenting on Spiegelman’s cost  
533 estimates saying, “It could easily turn out that the government might spend billions of  
534 dollars on benefit payments and have little effect on the families’ incomes. Instead, most of  
535 the (government) expenditures would offset reductions in earnings.” Only a few exceptions  
536 such as Carl Rowan for the *Washington Star* (1978) considered that it might be acceptable  
537 for people working in bad jobs to work less, but he could not figure out why the government  
538 would spend so much money to find out whether people work less when you pay them to  
539 stay home.

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

549 The scientists who presented the data were not entirely to blame for this misunderstanding,  
550 as Burtless (1986) remarked, “Policymakers and policy analysts . . . seem far more  
551 impressed by our certainty that the efficiency price of redistribution is positive than they  
552 are by the equally persuasive evidence that the price is small.” It may be an impossible  
553 task to communicate such complexities to an audience interested only in sound bytes or  
554 in the bottom line, but social scientists have a responsibility to do a better job than we  
555 did in this instance. The understanding of the NIT experiments displayed in the popular  
556 press was superficial and obviously the result of spin. Few commentators kept figures like  
557 5–7% in perspective. None of the articles in the popular media that I was able to find be-  
558 trayed any understanding that the experiments measured only the horizontal shift in the  
559 labor supply function. None seemed to understand the elementary economic principle that  
560 a change in supply necessitates a demand response that can greatly affect the equilibrium  
561 outcome.

## 562 5. Conclusion

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

577 A negative spin would require a focus on three facts: First, there was a statistically signif-  
578 icant work disincentive effect, allowing willing laypersons to draw the fallacious conclusion  
579 that there was therefore a substantively significant work disincentive effect. Second, work  
580 reductions of 5–7% among primary earners in two-parent families and reductions of up to  
581 27% for other earners could be called “large.” Third, the work disincentive increased the

582 cost of the program over what it would have been if work hours were unaffected by the NIT.  
583 Estimates of the added cost vary from 10% to 200%, and it is not difficult to focus on the  
584 larger estimates.

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

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

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

625 **Uncited references**

626 Aaron and Todd (1979), Andersen (1978), Aaron (1975), Adams (1980), Anderson  
627 (1978), Ashenfelter (1983), Ashenfelter and Plant (1990), Atkinson et al. (1973), AuClaire  
628 (1977), Avery (1977), Avery and Watts (1977), Barth et al. (1975), Bawden (1970, 1976,  
629 1977a,b,c), Bawden and Harrar (1977), Bawden and Harrar (1978, 1983), Block (1991),  
630 Blum (1986), Boekmann (1976), Brown (1972), Browning (1975), Bryant (1986), Burke  
631 and Burke (1979), Burtless (1989, 1990, 1995), Burtless and Greenberg (1982), Burtless and  
632 Hausman (1978), Cain et al. (1977), Christopherson (1983a,b), Collard (1980), Conlisk and  
633 Watts (1969), Coyle and Wildavsky (1986), Danzinger et al. (1981), Davis and Waksberg  
634 (1980), Dickenson and West (1983), Elesh et al. (1971), Galloday and Havemen (1977),  
635 Garfinkel (1974, 1982), Greenberg and Halsey (1983), Halsey (1980), Hausman and Wise  
636 (1976, 1979, 1985), Havenman and Watts (1976), Heckman and Smith (1995), Hollister  
637 (1974), Hollister and Metcalf (1977), Hum (1988), Hum and Simpson (1993b, 1995),  
638 Johnson and Pencavel (1980, 1982, 1984), Johnson (1980), Juster (1974), Keeley (1978,  
639 1980a, 1980b), Keeley and Robins (1980), Keeley et al. (1980), Kehrer (1979), Kershaw  
640 and Fair (1976), Kershaw and Small (1972), Kessleman (1976), Killingsworth (1984),  
641 Killingsworth and Heckman (1986), Kurz and Spiegelman (1971), Lampman (1974), Lane  
642 (1975), Lerner and Townsend (1974), Levine (1975), Mahoney and Mahoney (1975),  
643 Masters and Garfinkle (1977), Maxfield (1980), Maynard and Murnane (1979), McDonald  
644 and Stephenson (1979), Metcalf (1973, 1977a,b,c), Moffitt (1985), Moffitt and Kehrer  
645 (1981), Morrill (1974), Moynihan (1973), Mroz (1987), Munnell (1986), Munson et al.  
646 (1980), Murray (1986), Nathan (1986), NCW (1976), Neuberger (1989), Nicholson  
647 (1977a,c), Office of Income Security Policy (1983), Palmer and Pechman (1978), Pechman  
648 and Timpane (1975a,b), Pencavel (1986), Prescott et al. (1986), Pine (1978), Rainwater  
649 (1986), Rees (1974, 1977), Rivlin (1974a,b), Rivlin and Timpane (1975), Robins and  
650 West (1980c, 1983, 1985, 1986), Ross (1966,1974), Rossi and Lyle (1976), Skidmore  
651 (1974, 1975), Solow (1985), Spiegelman (1983), Spiegelman and West (1976), Spiegelman  
652 and Yaeger (1980), Spilerman and Miller (1977a,b), Stafford (1985), Tuma and Hannan  
653 (1979), Van Loon (1979), Watts (1971), Watts et al. (1977a), Watts and Dale (1977),  
654 Watts and Horner (1977), Watts and Mamer (1977), Watts et al. (1977b), Watts and Rees  
655 (1977a,b), West (1980a,c), Williams (1972), Wilson (1974), Wright (1977), and Zellner and  
656 Rossi (1986), Associated Press (1978), Bartlett (1978), Business Week (1976), Demkovich  
657 (1978, 1980), Greene (1979), Hum and Simpson (2001), Jones (1970, 1978), Kamien  
658 (1977), Kershaw (1972), Lambro (1979), Lenkowsky (1979), Moffitt (1981), Morris (1970),  
659 Moynihan (1978a,b), Nelson (1970), New York Times Editorial Board (1979), New York  
660 Times News Service (1977), New York Times (1978), Newsweek (1978), Ostrum (1978),  
661 Reinhold (1979), Rich (1978a,b,c), Rocky Mountain News Editorial Board (1978), Rowan  
662 (1978), Sacramento Bee Editorial Board (1978), Samuelson (1977), Schiller (1978), Seattle  
663 Times (1971), SocioEconomic Newsletter (1977, 1978), Spiegelman (1978, 1979), Steiger  
664 (1977), World Report (1977), United Press International (1978), Hum and Simpson (1993b),  
665 Hum and Simpson (1995).

666 **Acknowledgements**

667 Thanks to Philippe Van Parijs, Jim Bryan, and Marc-André Pigeon for help with this draft  
 668 and to Michael Grossman, Robert Haveman, Robert Moffitt, David Greenberg, Robinson  
 669 Hollister, Allan Ostergren, and the Institute for Socio-Economic Studies for help gathering  
 670 the sources. Thanks to Harold Watts, David Levine, Walter Williams, and to everyone  
 671 else who participated in the discussion of this paper at the first USBIG Congress.

672 **Bibliography A: A few non-academic articles on the NIT experiments**

673 **Bibliography B: Published academic articles and books on the NIT experiments**<sup>13</sup>

674 **References**

- 675 Andersen, M., 1978. Welfare reform on the same old rocks. *New York Times*, November 27.  
 676 Associated Press, 1978. Social experiment finds. *New Orleans Time-Picayune*, May 19.  
 677 Bartlett, C., 1978. A new hitch for welfare reform. *Washington Star*, November 20.  
 678 Brimberg, J., 1980. Income security project flounders; halt sought: guaranteed income program fails. *Denver Post*,  
 679 February 14.  
 680 *Business Week*, 1976. Positive values of the negative income tax. *Business Week*, November.  
 681 Demkovich, L.E., 1978. Good news and bad news for welfare reform. *National Journal*, December 30.  
 682 Demkovich, L.E., 1980. It may be a race against the clock for welfare reform package in 1980. *National Journal*,  
 683 January 26.  
 684 Greene, L.M., 1979. Letter on income maintenance experiments: too soon to jump to conclusions. *New York*  
 685 *Times*, February 20.  
 686 Hum, D., Simpson, W., 2001. A guaranteed annual income? From Mincome to the millennium. *Policy Options/Options Politiques*, January–February.  
 687 Jones, M., 1970. 35 families join income plan; more to sign up next month. *Seattle Times*, November 28.  
 688 Jones, M., 1978. \$60 million, 8-year social experiment: test casts doubt on income plan. *Seattle Times*, May 18.  
 689 Kamien, A., 1977. HEW study links guaranteed income to family breakup. *Rocky Mountain News*, November 14.  
 690 Kershaw, D., 1972. A Negative-Income-Tax Experiment. *Scientific American*, October.  
 691 Knight News Service, 1978. Next welfare plan: lower cost, benefits. *San Francisco Examiner*, November 16.  
 692 Lambro, D., 1979. Easy money at HEW. *Conservative Digest*, April. Reprinted from *Policy Review*.  
 693 Lenkowsky, L., 1979. Welfare reform and the liberals. *Commentary*, March.  
 694 Moffitt, R.A., 1981. The negative income tax: would it discourage work? *Monthly Labor Review*, April.  
 695 Morris, M., 1970. 2,200 city families will get \$5.1 million income aid. *Seattle Post Intelligencer*, June 16.  
 696 Moynihan, D.P., 1978a. Interview. Some negative evidence about the negative income tax. *Fortune Magazine*,  
 697 December 4.  
 698 Moynihan, D.P., 1978b. Letter to William F. Buckley. *National Review*, September 29.  
 699 Nelson, D., 1970. Annual Income Experiment Set. *Skagit Valley Herald*, Mt. Vernon, WA, March 9.  
 700 New York Times Editorial Board, 1979. Scare talk about welfare reform. *New York Times*, February 13.  
 701 New York Times News Service, 1977. Welfare ‘sweetener’ blunts criticism. *Washington Star*, August 7.

<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 Communication: The Labor Market Findings of the Negative Income Tax Experiments and their Effects on Policy and Public Opinion” at <http://www.usbig.net>.

- 703 New York Times, 1978. Moynihan says recent studies raise doubts about 'negative income tax' proposals. New  
704 YorkTimes, November 16.
- 705 Newsweek, 1978. Welfare: A Surprising Test. Newsweek, November 27.
- 706 Ostrum, C., 1978. To each according to his need? Seattle Sun, March 22.
- 707 Pine, Art, 1978. The negative side of negative tax. Washington Post, May 12.
- 708 Raspberry, W., 1978. A failed experiment in guaranteed income. Washington Post, November 20.
- 709 Reinhold, R., 1979. Test in Seattle challenges minimum-income plan. New York Times, February 5.
- 710 Rich, S., 1978a. Income plan linked to less work: marriages break up, study also finds. Washington Post, November  
711 16.
- 712 Rich, S., 1978b. Moynihan sees \$6 billion increase in welfare cost under revision plans. Washington Post, November  
713 18.
- 714 Rich, S., 1978c. Welfare plan linked to family splits. The Washington Post, May 2.
- 715 Rocky Mountain News Editorial Board, 1978. A valuable test. Rocky Mountain News, November 29.
- 716 Rowan, C.T., 1978. A little common sense in place of money. Washington Star, December 6.
- 717 Sacramento Bee Editorial Board, 1978. Welfare and families. Sacramento Bee, March 18.
- 718 Samuelson, P.A., 1977. Welfare reform. Newsweek, August 29.
- 719 Schiller, B.R., 1978. When welfare families know their rights. The Wall Street Journal, July 11.
- 720 Seattle Times, 1971. 1,000 families to receive income aid. Seattle Times, February 3.
- 721 Socioeconomic Newsletter, 1977. Califano relies on HEW tests to bolster welfare plan. SocioEconomic Newsletter,  
722 July.
- 723 SocioEconomic Newsletter, 1978. Flare-up on negative income tax. SocioEconomic Newsletter, January.
- 724 Spiegelman, R.G., 1978. Letter to the Editor. Washington Star, December 15.
- 725 Spiegelman, R.G., 1979. Letter to the Editor. SocioEconomic Newsletter, March.
- 726 Steiger, P.E., 1977. Divorce linked to income gains in welfare study. Los Angeles Times, November 4.
- 727 U.S. News and World Report, 1977. ABC's of carter welfare plan—and the changes it would bring. U.S. News  
728 and World Report, August 22.
- 729 U.S. Senate, 1978. Welfare research and experimentation: hearings before the subcommittee on public assistance  
730 of the committee on finance, United States Senate. U.S. Government Printing Office, Washington.
- 731 United Press International, 1977. Guaranteed income against work ethic. Seattle Daily Journal Commerce, Novem-  
732 ber 16.
- 733 United Press International, 1978. Study Raises Questions on Welfare Reform. Washington Star, November 16.
- 734 Aaron, H.J., Todd, J., 1979. The use of income maintenance experiment findings in public policy, 1977–1978.  
735 Industrial Relations Research Association, 31st Annual Proceedings, Madison, Wisconsin, pp. 46–56.
- 736 Aaron Jr., H., 1975. Cautionary notes on the experiment. In: Pechman, J.A., Timpane, P.M. (Eds.), *Work Incentives  
737 and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings Institution, Washington,  
738 DC, pp. 88–110.
- 739 Adams, C., 1980. A reappraisal of the work incentive aspects of welfare reform. *Social Service Review* 54 (4),  
740 521–536.
- 741 Anderson, G.M., Block, W., 1993. Economic response to a guaranteed annual income: experience from Canada  
742 and the United States: comment. *Journal of Labor Economics* 11 (1), S348–S363.
- 743 Anderson, M., 1978. *Welfare: The Political Economy of Welfare Reform in the United States*. Hoover Institution  
744 Press, Sanford, CA.
- 745 Ashenfelter, O., 1978. The labor supply response of wage earners. In: Palmer, J.L., Pechman, J.A. (Eds.), *Welfare  
746 in Rural Areas*. Brookings Institution, Washington, DC.
- 747 Ashenfelter, O., 1983. Determining participation in income-tested social programs. *Journal of the American  
748 Statistical Association* 78, 517–525, applications section.
- 749 Ashenfelter, O., 1986. Discussion (of 'The work response to a guaranteed income. A survey of experimental  
750 evidence'). In: Munnell, A.H. (Ed.), *Lessons from the Income Maintenance Experiments*. Federal Reserve  
751 Bank of Boston, Boston.
- 752 Ashenfelter, O., Plant, W.M., 1990. Nonparametric estimates of the labor-supply effects of negative income tax  
753 programs. *Journal of Labor Economics* 8 (1(part 2)), S396–S415.
- 754 Atkinson, T., Cutt, J., Stevenson, H.M., 1973. *Public Policy Research and the Guaranteed Annual Income: A  
755 Design for the Experimental Evaluation of Income Maintenance Policies in Canada*. York University, Toronto.

- 756 AuClaire, P.A., 1977. Informing social policy: the limits of experimentation. *Sociological Practice* 2 (1), 24–37.
- 757 Avery, R., 1977. Effects of Welfare ‘bias’ on family earnings response. In: Watts, H.W., Rees, A. (Eds.), *The New*
- 758 *Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior,*
- 759 *and the Quality of the Evidence.* Academic Press, New York, pp. 303–322.
- 760 Avery, R., Watts, H.W., 1977. The application of an error component model to experimental panel data. In: Watts,
- 761 H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-supply Responses.*
- 762 *Academic Press, New York, pp. 383–392.*
- 763 Avrin, M.E., 1980. Utilization of subsidized housing. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G.
- 764 (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment.* Academic Press, New York.
- 765 Barth, M.C., Orr, L.L., Palmer, J.L., 1975. Policy implications: a positive view. In: Pechman, J.A., Timpane, P.P.M.
- 766 (Eds.), *Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment.* Brookings
- 767 *Institution, Washington, DC.*
- 768 Bawden, D.L., 1977b. Income and work response of wives and dependents. In: Bawden, D.L., Harrar, W.S. (Eds.),
- 769 *Final Report of The Rural Income Maintenance Experiment.* Institute for Research on Poverty, University of
- 770 *Wisconsin, Madison, WI.*
- 771 Bawden, D.L., 1970. Income maintenance and the rural poor: an experimental approach. *American Journal of*
- 772 *Agricultural Economics* 52, 438–441.
- 773 Bawden, D.L., 1976. Implications of a negative income tax for rural people. *American Journal of Agricultural*
- 774 *Economics*, 754–760.
- 775 Bawden, D.L., 1977a. Income and work response of husbands. In: Bawden, D.L., Harrar, W.S. (Eds.), *Final Report*
- 776 *of The Rural Income Maintenance Experiment.* Institute for Research on Poverty, University of Wisconsin,
- 777 *Madison, WI.*
- 778 Bawden, D.L., 1977c. Purpose and design of the rural income maintenance experiment. *American Journal of*
- 779 *Agricultural Economics* 59 (5), 855–858.
- 780 Bawden, D.L., Harrar, W.S. (Eds.), 1977. *Final Report of The Rural Income Maintenance Experiment.* Institute
- 781 *for Research on Poverty, University of Wisconsin, Madison, WI.*
- 782 Bawden, D.L., Harrar, W.S. (Eds.), 1983. *Final Report of the Seattle–Denver Income Experiment, vol. I: Design*
- 783 *and Results.* U.S. Government Printing Office, Washington, DC.
- 784 Bawden, D.L., Harrar, W.S., 1978. Design and operation. In: Palmer, J.L., Pechman, J.A. (Eds.), *Welfare in Rural*
- 785 *Areas: The North Carolina–Iowa Income Maintenance Experiment.* Brookings Institution, Washington, DC,
- 786 *pp. 23–54.*
- 787 Betson, D., Greenberg, D., 1983. Uses of microsimulation in applied poverty research. In: Goldstein, R., Sacks,
- 788 S.M. (Eds.), *Applied Policy Research.* Rowman and Allanheld, Totowa, NJ.
- 789 Betson, D., Greenburg, D., Kasten, R., 1980a. A microsimulation model for analyzing alternative welfare reform
- 790 *proposals: an application to the program for better jobs and income.* In: Haveman, R., Hollenbeck, K. (Eds.),
- 791 *Microeconomic Simulation Models for Public Policy Analysis, vol. 1.* Academic Press, New York.
- 792 Betson, D., Greenburg, D., Kasten, R., 1980b. Using labor supply results to simulate welfare reform alternatives.
- 793 *In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), A Guaranteed Annual Income: Evidence*
- 794 *from a Social Experiment.* Academic Press, New York.
- 795 Betson, D., Greenburg, D., Kasten, R., 1981. A simulation analysis of the economic efficiency and distributional
- 796 *effects of alternative program structures: the negative income tax versus the credit income tax.* In: Garfinkel,
- 797 I. (Ed.), *Income-Tested Transfer Programs: A Case For and Against.* Academic Press, New York.
- 798 Bishop, J.H., 1979. The general equilibrium impact of alternative antipoverty strategies. *Industrial and Labor*
- 799 *Relations Review* 32 (2), 205–223.
- 800 Bishop, J.H., 1980. Jobs, cash transfers, and marital instability: a review and synthesis of the evidence. *Journal of*
- 801 *Human Resources* 15 (3).
- 802 Block, W., 1991. *Economic Freedom: Toward a Theory of Measurement.* Fraser Institute, Vancouver.
- 803 Blum, B.B., 1986. Views of a policymaker and public administrator. In: Munnell, A.H. (Ed.), *Lessons from the*
- 804 *Income Maintenance Experiments.* Federal Reserve Bank of Boston, Boston.
- 805 Boekmann, M., 1976. Policy implications of the New Jersey income maintenance experiment. *Policy Sciences* 7,
- 806 *53–76.*
- 807 Boumol, W., 1974. An overview of the results on consumption, health, and social behavior. *Journal of Human*
- 808 *Resources* 9 (2), 253–264.

- 809 Boumol, W., 1977. An overview of the results. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-*  
 810 *Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the Quality*  
 811 *of the Evidence*. Academic Press, New York, pp. 1–14.
- 812 Bradbury, K.L., 1986. Discussion (of ‘Non-labor supply responses to the income maintenance experiments’). In:  
 813 Munnell, A.H. (Ed.), *Lessons from the Income Maintenance Experiments*. Federal Reserve Bank of Boston,  
 814 Boston, pp. 122–125.
- 815 Bradbury, K.L., 1978. Income maintenance alternatives and family composition: an analysis of price effects.  
 816 *Journal of Human Resources* 13 (3), 305–331.
- 817 Brown, C.V., 1972. Negative income tax and the incentive to work. *New Society*, 461–462.
- 818 Browning, E.K., 1971. Incentive and disincentive experimentation for income maintenance policy purposes: note.  
 819 *American Economic Review* 61, 709–712.
- 820 Browning, E.K., 1975. *Redistribution and the Welfare System*. American Enterprise Institute for Public Policy  
 821 Research, Washington, DC.
- 822 Bryant, W.K., 1986. A portfolio analysis of poor rural wage-working families’ assets and debts. *American Journal*  
 823 *of Agricultural Economics* 68 (2), 237–245.
- 824 Burke, V.J., Burke, V., 1979. *Nixon’s Good Deed: Welfare Reform*. Columbia University Press, New York.
- 825 Burtless, G., 1995. The case for randomized field trial in economic and policy research. *Journal of Economic*  
 826 *Perspectives* 9, 63–84.
- 827 Burtless, G., 1986. The work response to a guaranteed income. A survey of experimental evidence. In: Munnell,  
 828 A.H. (Ed.), *Lessons from the Income Maintenance Experiments*. Federal Reserve Bank of Boston, Boston.
- 829 Burtless, G., 1989. The effect of welfare reform on employment, earnings, and income. In: Cottingham, P.H.,  
 830 Ellwood, D.T. (Eds.), *Policy for the 1990s*. Harvard University Press, Cambridge, MA, pp. 103–140.
- 831 Burtless, G., 1990. The economist’s lament: public assistance in America. *Journal of Economic Perspectives* 4,  
 832 57–78.
- 833 Burtless, G., Greenberg, D., 1982. Inferences concerning labor supply behavior based on limited duration experi-  
 834 ments. *American Economic Review* 72, 488–497.
- 835 Burtless, G., Hausman, J.A., 1978. The effect of taxation on labor supply: evaluating the Gary negative income  
 836 tax experiments. *The Journal of Political Economy* 86 (6), 1103–1130.
- 837 Cain, G.C., 1977. Fertility behavior. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance*  
 838 *Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the Quality of the Evidence*.  
 839 Academic Press, New York, pp. 225–250.
- 840 Cain, G.C., 1986. The income maintenance experiments and the issues of marital stability and family composition  
 841 and the income maintenance experiments. In: Munnell, A.H. (Ed.), *Lessons from the Income Maintenance*  
 842 *Experiments*. Federal Reserve Bank of Boston, Boston, pp. 60–93.
- 843 Cain, G.C., Nicholson, W., Mallar, C., Wooldridge, J., 1974. The labor-supply response of married women,  
 844 husbands present. *Journal of Human Resources* 9 (2), 201–223.
- 845 Cain, G.C., Nicholson, W., Mallar, C., Wooldridge, J., 1977. Labor-supply response of wives. In: Watts, H.W.,  
 846 Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-supply Responses*. Academic  
 847 Press, New York, pp. 115–162.
- 848 Cain, G.C., Wissoker, D., 1990a. A reanalysis of marital stability in SIME/DIME. *American Journal of Sociology*  
 849 95 (5), 1235–1269.
- 850 Cain, G.C., Wissoker, D., 1990b. Response to Hannan and Tuma. *American Journal of Sociology* 95 (5),  
 851 1299–1314.
- 852 Christopherson, G., 1983a. Implementation. In: Bawden, D.L., Harrar, W.S. (Eds.), *The Final Report of the*  
 853 *Seattle–Denver Income Maintenance Experiment, Design and Results, vol. 1*. SRI International, Menlo Park,  
 854 CA, pp. 55–87.
- 855 Christopherson, G., 1983b. *The Final Report of the Seattle–Denver Income Maintenance Experiment, vol. 2*.  
 856 *Mathematica Policy Research*, Princeton, NJ.
- 857 Cogan, J.F., 1983. Labor supply and negative income taxation: new evidence from the New Jersey–Pennsylvania  
 858 experiment. *Economic Inquiry* 21 (4), 465–484.
- 859 Collard, D., 1980. Social dividend and negative income tax. In: Sandford, C., Pond, C., Walker, R. (Eds.), *Taxation*  
 860 *and Social Policy*. Heinemann, London, pp. 190–202.

- 861 Conlisk, J., Watts, H.W., 1969. A model for optimizing experimental designs for estimating response surfaces.  
862 American Statistical Association Proceedings, Social Statistics Section 64.
- 863 Coyle, D., Wildavsky, A., 1986. Social experimentation in the face of formidable fables. Lessons from the income  
864 maintenance experiments. In: Munnell, A.H. (Ed.), *Lessons from the Income Maintenance Experiments*.  
865 Federal Reserve Bank of Boston, Boston, pp. 167–184.
- 866 Danzinger, S., Haveman, R., Plotnick, R., 1981. How income transfer programs affect work, savings and the  
867 income distribution: a critical review. *Journal of Economic Literature* 19, 975–1028.
- 868 Davis, V., Waksberg, A., 1980. Data collection and processing. In: Robins, P.K., Spiegelman, R.G., Weiner, S.,  
869 Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New  
870 York.
- 871 Dickenson, K.P., West, R.W., 1983. Impacts of counseling and education subsidy programs. In: Bawden, D.L.,  
872 Harrar, W.S. (Eds.), *The Final Report of the Seattle–Denver Income Maintenance Experiment*, vol. 1: Design  
873 and Results. SRI International, Menlo Park, CA, pp. 201–256.
- 874 Elesh, D., Landinsky, J., Lefcowitz, M.J., Spilerman, S., 1971. The New Jersey–Pennsylvania experiment: a field  
875 study in negative taxation. In: Orr, L.L., Hollister, R.G., Lefcowitz, M.J. (Eds.), *Income Maintenance: Inter-*  
876 *disciplinary Approaches to Research*. Marham, Chicago, pp. 14–35.
- 877 Elesh, D., Lefcowitz, M.J., 1977. The effects of health on the supply of and returns to labor. In: Watts, H.W., Rees,  
878 A. (Eds.), *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-supply Responses. Academic  
879 Press, New York, pp. 289–320.
- 880 Ellwood, D.T., 1986. Discussion (of ‘The issues of marital stability’). In: Munnell, A. (Ed.), *Lessons from the*  
881 *Income Maintenance Experiments*. The Federal Reserve Bank of Boston, Boston, pp. 94–98.
- 882 Ferber, R., Hirsch, W., 1978. Social experimentation and economic policy: a survey. *Journal of Economic Literature*  
883 16, 1379–1414.
- 884 Galligan, R.J., Bahr, S.J., 1978. Economic well-being and marital stability: implications for income maintenance  
885 programs. *Journal of Marriage and the Family*, 283–290.
- 886 Galloday, F.L., Havemen, R.H., 1977. *The Economic Impacts of Tax-Transfer Policy: Regional and Distributional*  
887 *Effects*. Academic Press, New York.
- 888 Garfinkel, I. (Ed.), 1982. *Income-Tested Transfer Programs: The Case For and Against*. Academic Press, New  
889 York.
- 890 Garfinkel, I., 1974. The effects of welfare programs on experimental responses. *Journal of Human Resources* 9 (4),  
891 530–555. Also published in: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment*,  
892 vol. III: *The Impact on Expenditures, Health, and Social Behavior, and the Quality of the Evidence*. Academic  
893 Press, New York, pp. 279–302.
- 894 Greenberg, D., Moffitt, R., Friedmann, J., 1981. Underreporting and experimental effects of work effort: evidence  
895 from the Gary Income Maintenance Experiment. *Review of Economics and Statistics* 63, 581–589.
- 896 Greenberg, D.H., 1983. Some labor market effects of labor supply responses to transfer programs. *Social-Economic*  
897 *Planning Sciences* 17 (4), 141–151.
- 898 Greenberg, D.H., Halsey, H., 1983. Systematic misreporting and effects of income maintenance experiments on  
899 work effort: evidence from the Seattle–Denver experiment. *Journal of Labor Economics* 1 (4), 380–407.
- 900 Greenberg, D.H., Links, D., Mandell, M., 2003. *Social Experimentation and Public Policy Making*. Urban Institute  
901 Press.
- 902 Groeneveld, L., Tuma, N., Hannan, M., 1980a. The effects of negative income tax programs on marital dissolution.  
903 *Journal of Human Resources* 15, 654–674.
- 904 Groeneveld, L., Tuma, N., Hannan, M., 1980b. Marital dissolution and remarriage. In: Robins, P.K., Spiegel-  
905 man, R.G., Weiner, S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment*.  
906 Academic Press, New York.
- 907 Groeneveld, L., Tuma, N., Hannan, M., 1983. Marital Stability. In: Bawden, D.L., Harrar, W.S. (Eds.), *Final Report*  
908 *of the Seattle–Denver Income Maintenance Experiment*, vol. 1: Design and Results. SRI International, Menlo  
909 Park, CA, pp. 257–387.
- 910 Hall, A.R., 1980a. Education and training. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A*  
911 *Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 912 Hall, A.R., 1980b. The counseling and training subsidy treatments. *Journal of Human Resources* 15, 591–610.

- 913 Hall, R., 1975. Effects of the experimental negative income tax on labor supply. In: Pechman, J.A., Timpane, P.M.  
 914 (Eds.), *Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings  
 915 institution, Washington, DC, pp. 115–147.
- 916 Halsey, H.I., 1980. Data validation. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A Guaranteed  
 917 Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 918 Hannan, M., Tuma, N., 1990. A reassessment of the effects of income maintenance on marital dissolution in the  
 919 Seattle–Denver experiment. *American Journal of Sociology* 95, 1270–1298.
- 920 Hannan, M., Tuma, N., Groenveld, L., 1977. Income and marital events: evidence from an income-maintenance  
 921 experiment. *American Journal of Sociology* 82 (6), 1186–1211.
- 922 Hannan, M., Tuma, N., Groenveld, L., 1978. Income and independence effects on marital dissolution: results from  
 923 the Seattle and Denver income-maintenance experiments. *American Journal of Sociology* 84 (3), 611–633.
- 924 Hanusheck, E., 1986. Non-labor-supply response to the income maintenance experiments. In: Munnell, A. (Ed.),  
 925 *Lessons from the Income Maintenance Experiments*. The Federal Reserve Bank of Boston, Boston, pp.  
 926 106–121.
- 927 Hausman, J.A., Wise, D.A. (Eds.), 1985. *Social Experimentation*. University of Chicago Press, Chicago.
- 928 Hausman, J.A., Wise, D.A., 1976. The evaluation of results from truncated samples: the New Jersey income  
 929 maintenance experiment. *Annals of Economic and Social Measurement* 5, 421–475.
- 930 Hausman, J.A., Wise, D.A., 1979. Attrition bias in experimental and panel data: the Gary Income Maintenance  
 931 Experiment. *Econometrica* 47 (2), 455–473.
- 932 Havenman, R.H., Watts, H.W., 1976. Social experimentation as policy research: a review of negative income tax  
 933 experiments. *Evaluation Studies* 1, 406–431.
- 934 Heckman, J.J., Smith, J.A., 1995. Assessing the case for social experiments. *Journal of Economic Perspectives* 9  
 935 (2), 85–110.
- 936 Hollister, R., 1974. The labor-supply response of the family. *Journal of Human Resources* 9 (2), 223–252.
- 937 Hollister, R.G., Metcalf, C.E., 1977. Family labor-supply response in the New Jersey Experiment. In: Watts, H.W.,  
 938 Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-supply Responses*. Academic  
 939 Press, New York, pp. 185–220.
- 940 Hum, D., 1988. Integrating taxes and transfers. *Canadian Tax Journal* 3, 671–690.
- 941 Hum, D., Choudry, S., 1992. Income, work and marital dissolution: Canadian experimental evidence. *Journal of  
 942 Comparative Family Studies* 23 (2), 249–265.
- 943 Hum, D., Simpson, W., 1991. Income Maintenance, Work Effort, and the Canadian Experiment. Economic Council  
 944 of Canada, Ottawa.
- 945 Hum, D., Simpson, W., 1993a. Economic response to a guaranteed annual income: experience from Canada and  
 946 the United States. *Journal of Labor Economics* 11 (1(part 2)), S263–S296.
- 947 Hum, D., Simpson, W., 1993b. Whatever happened to the guaranteed income idea? *Canadian Public Administration*  
 948 36 (3), 442–450.
- 949 Hum, D., Simpson, W., 1995. Reducing spending and increasing equity: how far can refundable tax credits take  
 950 us? *Canadian Public Administration* 38 (4), 598–612.
- 951 Johnson, T.R., Pencavel, J.H., 1980. Welfare payments and family composition. In: Robins, P.K., Spiegelman, R.G.,  
 952 Weiner, S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment*. Academic  
 953 Press, New York.
- 954 Johnson, T.R., Pencavel, J.H., 1982. Forecasting the effects of a negative income tax program. *Industrial and Labor  
 955 Relations Review* 35, 221–234.
- 956 Johnson, T.R., Pencavel, J.H., 1984. Dynamic hours of work functions for husbands, wives, and single females.  
 957 *Econometrica* 52, 363–389.
- 958 Johnson, W.R., 1980. The effect of a negative income tax on risk-taking in the labor market. *Economic Inquiry* 18  
 959 (3), 395–407.
- 960 Juster, T.F., 1974. Rethinking the allocation of resources in social research. *Monthly Labor Review*, 36–39.
- 961 Kaluzny, R.L., 1979. Changes in the consumption of housing services: the Gary Experiment. *Journal of Human  
 962 Resources* 14 (4), 496–506.
- 963 Keeley, M.C., 1978. The estimation of labor supply models using experimental data. *American Economic Review*  
 964 68, 873–887.

- 965 Keeley, M.C., 1980a. Demand for children. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A*  
966 *Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 967 Keeley, M.C., 1980b. Migration. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A*  
968 *Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 969 Keeley, M.C., 1980c. The effects of negative income tax on migration. *Journal of Human Resources* 15, 695–706.
- 970 Keeley, M.C., 1980d. The effects of negative income tax programs on fertility. *Journal of Human Resources* 15,  
971 675–694.
- 972 Keeley, M.C., 1981a. *Labor Supply and Public Policy: A Critical Review*. Academic Press, New York.
- 973 Keeley, M.C., 1981b. *Labor Supply and Public Policy: Critical Review*. Academic Press, New York.
- 974 Keeley, M.C., Robins, P., 1980. Experimental design, the Conlisk–Watts assignment model, and the proper esti-  
975 mation of behavioral response. *The Journal of Human Resources* 15 (4), 480–498.
- 976 Keeley, M.C., Robins, P., Spiegelman, R., West, R., 1978a. The labor supply effects and costs of alternative negative  
977 income tax programs. *Journal of Human Resources* 13 (1), 3–36.
- 978 Keeley, M.C., Robins, P., Spiegelman, R., West, R., 1978b. The estimation of labor supply models using experi-  
979 mental data. *American Economic Review* 68, 873–887.
- 980 Keeley, M.C., Spiegelman, R., West, R., 1980. Design of the Seattle/Denver income-maintenance experiments and  
981 an overview of results. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A*  
982 *Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 983 Kehrer, B.H., Wolin, C.M., 1979. Impact of income maintenance on low birthweight: evidence from the Gary  
984 Experiment. *Journal of Human Resources* 14 (4), 434–462.
- 985 Kehrer, K.C., 1979. Introduction (to the JHR special issue on the Gary Income Maintenance Experiment). *Journal*  
986 *of Human Resources* 14 (4), 431–433.
- 987 Kelly, T.F., Singer, L., 1971. The Gary Income Maintenance Experiment: plans and progress. *American Economic*  
988 *Review* 61, 30–42.
- 989 Kerachsky, S.H., 1977. Health and medical care utilization: a second approach. In: Watts, H.W., Rees, A. (Eds.),  
990 *The New Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social*  
991 *Behavior, and the Quality of the Evidence*. Academic Press, New York, pp. 129–150.
- 992 Kershaw, D.N., Fair, J., 1976. *The New Jersey Income-Maintenance Experiment, vol. I: Operations, Surveys, and*  
993 *Administration*. Academic Press, New York.
- 994 Kershaw, D.N., Small, J.C., 1972. Data confidentiality and privacy: lessons from the New Jersey Negative Income  
995 Tax Experiment. *Public Policy* 20 (2), 257–280.
- 996 Kessleman, J.R., 1976. Tax effects on job search, training, and work effort. *Journal of Public Economics* 6, 255–  
997 272.
- 998 Killingsworth, M., 1984. *Labor Supply*. Cambridge University, Cambridge.
- 999 Killingsworth, M., Heckman, J., 1986. Female labor supply: a survey. In: Ashenfelter, O., Layard, R. (Eds.),  
1000 *Handbook of Labor Economics, vol. 1*. North Holland, Amsterdam, pp. 103–204.
- 1001 Knudsen, J.H., Mamer, J., Scott, R.A., Shore, A.R., 1977a. Information levels and labor response. In: Watts,  
1002 H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-supply Responses*.  
1003 Academic Press, New York, pp. 347–368.
- 1004 Knudsen, J.H., Scott, R.A., Shore, A.R., 1977b. Household consumption. In: Watts, H.W., Rees, A. (Eds.), *The New*  
1005 *Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior,*  
1006 *and the Quality of the Evidence*. Academic Press, New York, pp. 251–276.
- 1007 Kurz, M., Spiegelman, R.G., 1971. The Seattle Experiment: the combined effect of income maintenance and  
1008 manpower investments. *American Economic Review* 61 (2), 22–29.
- 1009 Ladinsky, J., Wells, A., 1977. Social integration, leisure activity, media exposure, and lifestyle enhancement.  
1010 In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. III: The Impact on*  
1011 *Expenditures, Health, and Social Behavior, and the Quality of the Evidence*. Academic Press, New York, pp.  
1012 195–224.
- 1013 Lampman, R., 1974. The decision to undertake the New Jersey Experiment. *Final Report of the New Jersey*  
1014 *Experiment, vol. 4*. Institute for Research on Poverty and Mathematica, Madison, WI.
- 1015 Lane, R., 1975. Social science research and public policy. In: Nagel, S.S. (Ed.), *Policy Studies and the Social*  
1016 *Sciences*. Lexington Book, D.C. Health and Company, Lexington, MA, pp. 287–291.

- 1017 Lefcowitz, M.J., Elesh, D., 1977. Health and medical care utilization. In: Watts, H.W., Rees, A. (Eds.), *The New*  
1018 *Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior,*  
1019 *and the Quality of the Evidence.* Academic Press, New York, pp. 113–128.
- 1020 Lerner, R.I., Townsend, A.A., 1974. Conflicting objections in income maintenance programs. *The American*  
1021 *Economic Review* 64 (2), 205–211.
- 1022 Levine, R.A., 1975. How and why the experiments came about. In: Pechman, J., Timpane, M. (Eds.), *Work*  
1023 *Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment.* Brookings Institution,  
1024 Washington, DC.
- 1025 Levine, R., Watts, H., Hollister, R., Williams, W., O'Connor, A., Widerquist, K., 2004. Looking back at the negative  
1026 income tax experiments from 30 years on. In: Lewis, M., Pressman, S., Widerquist, K. (Eds.), *The Ethics and*  
1027 *Economics of the Basic Income Guarantee.* Ashgate, New York.
- 1028 Mahoney, B.S., Mahoney, W.M., 1975. Policy implications: a skeptical view. In: Pechman, J.A., Timpane, P.M.  
1029 (Eds.), *Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment.* Brookings  
1030 institution, Washington, DC.
- 1031 Mallar, C.D., 1977. The educational and labor-supply responses of young adults in experimental families. In: Watts,  
1032 H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-supply Responses.*  
1033 Academic Press, New York, pp. 163–184.
- 1034 Masters, S., Garfinkle, I., 1977. Estimating the Labor Supply Effects of Income-Maintenance Alternatives. *Acad-*  
1035 *emic Press, New York, NY.*
- 1036 Masters, S.H., 1978. Comments on Robert Michael: the consumption studies. In: Palmer, J., Pechman, J. (Eds.),  
1037 *Welfare in Rural Areas: The North Carolina–Iowa Income Maintenance Experiment.* Brookings Institution,  
1038 Washington, DC, pp. 171–173.
- 1039 Maxfield Jr., M., 1980. Aspects of a negative income tax: program cost, adequacy of support, and induced labor  
1040 supply reduction. In: Haveman, R., Hollenbeck, K. (Eds.), *Microsimulation models for Public Policy Research.*  
1041 Academic Press, New York.
- 1042 Maynard, R., Murnane, R.J., 1979. The effects of a negative income tax on school performance: results of an  
1043 experiment. *Journal of Human Resources* 14 (4), 463–476.
- 1044 Maynard, R., 1977. The effects of the Rural Income Maintenance Experiment on the school performance of  
1045 children. *American Economic Review* 67 (1), 370–375.
- 1046 McDonald, J.F., Stephenson Jr., S.P., 1979. The effect of income maintenance on the school-enrollment and  
1047 labor-supply decisions of teenagers. *Journal of Human Resources* 14 (4), 488–495.
- 1048 Metcalf, C., 1973. Making inferences from Controlled Income Maintenance Experiments. *American Economic*  
1049 *Review* 63, 478–483.
- 1050 Metcalf, C., 1974. Predicting the effects of permanent programs from a limited duration experiment. *Journal of*  
1051 *Human Resources* 9 (4), 530–555.
- 1052 Metcalf, C., 1977a. Sample design and the use of experimental data. In: Watts, H.W., Rees, A. (Eds.), *The New*  
1053 *Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior,*  
1054 *and the Quality of the Evidence.* Academic Press, New York, pp. 413–440.
- 1055 Metcalf, C., 1977b. Consumption behavior: implications for a permanent program. In: Watts, H.W., Rees, A.  
1056 (Eds.), *The New Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and*  
1057 *Social Behavior, and the Quality of the Evidence.* Academic Press, New York, pp. 93–112.
- 1058 Metcalf, C., 1977c. Predicting the effects of permanent programs from a limited duration experiment. In: Watts,  
1059 H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures,*  
1060 *Health, and Social Behavior, and the Quality of the Evidence.* Academic Press, New York, pp. 375–399.
- 1061 Michael, R., 1978. The consumption studies. In: Palmer, J., Pechman, J. (Eds.), *Welfare in Rural Areas: The North*  
1062 *Carolina–Iowa Income Maintenance Experiment.* Brookings Institution, Washington, DC, pp. 149–171.
- 1063 Middleton, R., Allen, V.L., 1977. Social psychological effects. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey*  
1064 *Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the*  
1065 *Quality of the Evidence.* Academic Press, New York, pp. 151–194.
- 1066 Moffitt, R.A., 1979a. The labor supply response in the Gary Experiment. *Journal of Human Resources* 14 (4),  
1067 477–487.
- 1068 Moffitt, R.A., 1979b. The labor market replacement effect of a negative income tax. *Industrial and Labor Relations*  
1069 *Review* 33 (1), 85–94.

- 1070 Moffitt, R.A., 1985. A problem with the negative income tax. *Economic Letters* 17, 261–265.
- 1071 Moffitt, R.A., Kehrer, K., 1981. The effect of tax and transfer programs on labor supply: the evidence from the  
1072 income maintenance experiments. *Research in Labor Economics* 4, 103–150.
- 1073 Morrill, W.A., 1974. Introduction (to JHR symposium—the Graduated Work Incentives Experiment). *Journal of*  
1074 *Human Resources* 9 (2), 156–157.
- 1075 Moynihan, D.P., 1973. *The Politics of a Guaranteed Income: The Nixon Administration and the Family Assistance*  
1076 *Plan*. Random House, New York.
- 1077 Mroz, T., 1987. The sensitivity of an empirical model of married women’s hours of work to economic and statistical  
1078 assumptions. *Econometrica* 55 (4), 765–799.
- 1079 Munnell, A.H. (Ed.), 1986. *Lessons from the Income Maintenance Experiments*. Federal Reserve Bank of Boston,  
1080 Boston.
- 1081 Munson, C.E., Robins, P.K., Stieger, G., 1980. Labor supply and childcare arrangements of single mothers. In:  
1082 Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from*  
1083 *a Social Experiment*. Academic Press, New York.
- 1084 Murnane, R., Maynard, R., Ohls, J., 1981. Home Resources and Children’s Achievement. *The Review of Economics*  
1085 *and Statistics* 63 (3), 369–377.
- 1086 Murray, C., 1986. Discussion of the policy lessons. In: Munnell, A.H. (Ed.), *Lessons from the Income Maintenance*  
1087 *Experiments*. Federal Reserve Bank of Boston, Boston.
- 1088 Nathan, R.P., 1986. Lessons for future public policy and research. In: Munnell, A.H. (Ed.), *Lessons from the*  
1089 *Income Maintenance Experiments*. Federal Reserve Bank of Boston, Boston.
- 1090 National Council of Welfare (Canadian), 1976. *Guide to the Guaranteed Income*. National Council of Welfare,  
1091 Ottawa.
- 1092 Neuberg, L.G., 1989. *Conceptual Anomalies in Economics and Statistics: Lessons from the Social Experiment*.  
1093 Cambridge University Press, New York.
- 1094 Nicholson, W., 1977a. Differences among the three sources of income data. In: Watts, H.W., Rees, A. (Eds.),  
1095 *The New Jersey Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social*  
1096 *Behavior, and the Quality of the Evidence*. Academic Press, New York, pp. 353–374.
- 1097 Nicholson, W., 1977b. Expenditure patterns: a descriptive survey. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey*  
1098 *Income-Maintenance Experiment, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the*  
1099 *Quality of the Evidence*. Academic Press, New York, pp. 15–44.
- 1100 Nicholson, W., 1977c. Relationship of female labor-supply characteristics of the experimental sample to those of  
1101 other samples. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. III:*  
1102 *The Impact on Expenditures, Health, and Social Behavior, and the Quality of the Evidence*. Academic Press,  
1103 New York, pp. 323–340.
- 1104 O’Connor, J., Madden, F., Madden, J.P., 1979. The negative income tax and the quality of dietary intake. *Journal*  
1105 *of Human Resources* 14 (4), 507–517.
- 1106 O’Connor, A., 2001. *Poverty Knowledge: Social Science, Social Policy, and the Poor in Twentieth Century U.S*  
1107 *History*. Princeton University Press, Princeton, NJ.
- 1108 Office of Income Security Policy, U.S. Department of Health and Human Services, 1983. *Overview of the*  
1109 *Seattle–Denver Income Maintenance Experiment Final Report*. U.S. Government Printing Office, Washington,  
1110 DC.
- 1111 Ohls, J., 1980. The demand for housing under a negative income tax. In: Stromsdorfer, E., Farkas, G. (Eds.),  
1112 *Evaluation Studies Review Annual, vol. 5*, p. 502.
- 1113 Orcutt, G., Orcutt, A., 1968. Incentive and disincentive experimentation for income maintenance policy purposes.  
1114 *American Economic Review* 58, 754–772.
- 1115 Palmer, J., Pechman, J.A. (Eds.), 1978. *Welfare in Rural Areas: The North Carolina–Iowa Income Maintenance*  
1116 *Experiment*. Brookings Institution, Washington, DC.
- 1117 Pechman, J.A., Timpane, P.M. (Eds.), 1975a. *Work Incentives and Income Guarantees: The New Jersey Negative*  
1118 *Income Tax Experiment*. Brookings institution, Washington, DC.
- 1119 Pechman, J.A., Timpane, P.M., 1975b. Introduction and summary. In: Pechman, J.A., Timpane, P.M. (Eds.), *Work*  
1120 *Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings Institution,  
1121 Washington, DC.

- 1122 Pencavel, J., 1986. Labor supply of men: a survey. In: Ashenfelter, O., Layard, R. (Eds.), *Handbook of Labor*  
 1123 *Economics*, vol. 1. North Holland, Amsterdam, pp. 103–204.
- 1124 Poirier, D.J., 1977a. Characteristics of attriters who took the attrition interview. In: Watts, H.W., Rees, A. (Eds.),  
 1125 *The New Jersey Income-Maintenance Experiment*, vol. III: The Impact on Expenditures, Health, and Social  
 1126 Behavior, and the Quality of the Evidence. Academic Press, New York, pp. 399–412.
- 1127 Poirier, D.J., 1977b. Spline functions and their applications in regression analysis. In: Watts, H.W., Rees, A. (Eds.),  
 1128 *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses. Academic Press, New  
 1129 York, pp. 369–382.
- 1130 Poirier, D.J., 1977c. The determinants of home buying. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-*  
 1131 *Maintenance Experiment*, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the Quality  
 1132 of the Evidence. Academic Press, New York.
- 1133 Pozdena, R.J., Johnson, T.R., 1980. Demand for assets. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G.  
 1134 (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 1135 Prescott, D., Swidinsky, R., Wilton, D., 1986. Labour supply estimates for low-income female heads of households  
 1136 using Mincome data. *Canadian Journal of Economics* 19 (1), 134–141.
- 1137 Rainwater, L., 1986. A sociologist's view of the income maintenance experiments. In: Munnell, A.H. (Ed.), *Lessons*  
 1138 *from the Income Maintenance Experiments*. Federal Reserve Bank of Boston, Boston.
- 1139 Rea Jr., S.A., 1977. Investment in human capital under a negative income tax. *Canadian Journal of Economics* 10  
 1140 (4), 607–620.
- 1141 Rees, A., 1974. An overview of the labor-supply results. *Journal of Human Resources* 9 (2), 158–180.
- 1142 Rees, A., 1977. Labor supply results of the experiment: a summary. In: Watts, H.W., Rees, A. (Eds.), *The New*  
 1143 *Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses. Academic Press, New York.
- 1144 Rees, A., Watts, H.W., 1975. An overview of the labor supply results. In: Pechman, J.A., Timpane, P.M. (Eds.), *Work*  
 1145 *Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings institution,  
 1146 Washington, DC.
- 1147 Reichauer, R.D., 1986. Discussion (of 'A political scientists view of the income maintenance experiments'). In:  
 1148 Munnell, A.H. (Ed.), *Lessons from the Income Maintenance Experiments*. Federal Reserve Bank of Boston,  
 1149 Boston.
- 1150 Rivlin, A.M., 1974a. How can experiments be more useful? *The American Economic Review* 64 (2), 346–354.
- 1151 Rivlin, A.M., 1974b. Social experiments: their uses and limitations. *Monthly Labor Review*, 28–35.
- 1152 Rivlin, A.M., Timpane, P.M., 1975. Ethical and Legal Issues of Social Experimentation. The Brookings Institution,  
 1153 Washington, DC.
- 1154 Robins, P.K., 1980a. Labor supply response of family heads and implications for a national program. In: Robins,  
 1155 P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from a Social*  
 1156 *Experiment*. Academic Press, New York.
- 1157 Robins, P.K., 1980b. Job satisfaction. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), *A Guaranteed*  
 1158 *Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 1159 Robins, P.K., 1984. The labor supply response of twenty-year families in the Denver Income Maintenance Exper-  
 1160 iment. *Review of Economics and Statistics* 66 (3), 491–495.
- 1161 Robins, P.K., 1985. A comparison of the labor supply findings from the four negative income tax experiments.  
 1162 *Journal of Human Resources* 20 (4), 567–582.
- 1163 Robins, P.K., Brandon, N., Yeager, K.E., 1980a. Effects of SIME/DIME on changes in employment status. *The*  
 1164 *Journal of Human Resources* 15 (4), 545–573.
- 1165 Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.), 1980b. *A Guaranteed Annual Income: Evidence*  
 1166 *from a Social Experiment*. Academic Press, New York.
- 1167 Robins, P.K., West, R., 1980a. Program participation and labor-supply response. *The Journal of Human Resources*  
 1168 15 (4), 499–523.
- 1169 Robins, P.K., West, R., 1980b. Labor-supply response over time. *The Journal of Human Resources* 15 (4), 524–544.
- 1170 Robins, P.K., West, R., 1980c. Labor supply response of family heads over time. In: Robins, P.K., Spiegelman, R.G.,  
 1171 Weiner, S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from A Social Experiment*. Academic  
 1172 Press, New York.
- 1173 Robins, P.K., West, R., 1983. Labor supply response. In: Bawden, D.L., Harrar, W.S. (Eds.), *Final Report of the*  
 1174 *Seattle-Denver Income Maintenance Experiment*. Government Printing Office, Washington, DC.

- 1175 Robins, P.K., West, R., 1985. Program participation and labor-supply response. *Journal of Human Resources* 20,  
1176 567–582.
- 1177 Robins, P.K., West, R., 1986. Sample attrition and labor supply response in experimental panel data. *Journal of*  
1178 *Business and Economic Statistics* 4, 329–338.
- 1179 Ross, H., 1966. A Proposal for a Demonstration of New Techniques in Income Maintenance. United Planning  
1180 Organization, Washington, DC.
- 1181 Ross, H., 1974. Case study of testing experimentation: income maintenance and social policy research. In: Abert,  
1182 J.G., Kamass, M. (Eds.), *Social Experiments and Social Program Evaluation*, Proceedings of the Washington  
1183 Operations Research Council Symposium. Bollinger Press, Cambridge, MA.
- 1184 Rossi, P.H., 1975. A critical review of the analysis of nonlabor force responses. In: Pechman, J.A., Timpane, P.M.  
1185 (Eds.), *Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings  
1186 institution, Washington, DC.
- 1187 Rossi, P.H., Lyle, K.C., 1976. *Reforming Public Welfare: A Critique of the Negative Income Tax Experiments*.  
1188 Russell Sage Foundation, New York.
- 1189 Skidmore, F., 1974. Availability of data from the Graduated Work Incentive Experiment. *Journal of Human*  
1190 *Resources* 9 (2), 265–278.
- 1191 Skidmore, F., 1975. Operational design of the experiment. In: Pechman, J.A., Timpane, P.M. (Eds.), *Work In-*  
1192 *centives and Income Guarantees: The New Jersey Negative Income Tax Experiment*. Brookings institution,  
1193 Washington, DC.
- 1194 Solow, R.M., 1985. An economist's view of the income maintenance experiments. In: Munnell, A.H. (Ed.), *Lessons*  
1195 *from the Income Maintenance Experiments*. Federal Reserve Bank of Boston, Boston.
- 1196 Spiegelman, R.G., 1983. History and design. In: Bawden, D.L., Harrar, W.S. (Eds.), *Final Report of the*  
1197 *Seattle–Denver Income Maintenance Experiment, vol. 1: Design and Results*. SRI International, Menlo Park,  
1198 CA, pp. 1–51.
- 1199 Spiegelman, R.G., West, R.W., 1976. Feasibility of a social experiment and issues in its design: experiences  
1200 from the Seattle and Denver Income Maintenance Experiments. In: *Business and Economic Statistics Section,*  
1201 *Proceedings of the American Statistical Association*, pp. 168–176.
- 1202 Spiegelman, R.G., Yaeger, K.E., 1980. Overview (of the special issue *The Seattle and Denver Income Maintenance*  
1203 *Experiments*). *The Journal of Human Resources* 15 (4), 463–479.
- 1204 Spilerman, S., Miller, R.E., 1977a. The effect of negative income tax payments on job turnover and unemployment  
1205 duration. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment, vol. II: Labor-*  
1206 *Supply Responses*. Academic Press, New York, pp. 221–252.
- 1207 Spilerman, S., Miller, R.E., 1977b. The impact of the experiment on job selection. In: Watts, H.W., Rees, A. (Eds.),  
1208 *The New Jersey Income-Maintenance Experiment, vol. II: Labor-Supply Responses*. Academic Press, New  
1209 York, pp. 253–286.
- 1210 Stafford, F.P., 1985. Income-maintenance policy and work effort: learning from experiments and labor-market  
1211 studies. In: Hausman, J.A., Wise, D.A. (Eds.), *Social Experimentation*. University of Chicago Press, Chicago,  
1212 pp. 95–143.
- 1213 Thoits, P., Hannan, M.T., 1980. Income and psychological distress. In: Robins, P.K., Spiegelman, R.G., Weiner,  
1214 S., Bell, J.G. (Eds.), *A Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New  
1215 York.
- 1216 Tuma, N.B., 1986. Discussion (of 'The issues of marital stability'). In: Munnell, A. (Ed.), *Lessons from the Income*  
1217 *Maintenance Experiments*. The Federal Reserve Bank of Boston, Boston, pp. 99–105.
- 1218 Tuma, N.B., Hannan, M.T., 1979. Dynamic analysis of event histories. *American Journal of Sociology* 84 (4),  
1219 820–854.
- 1220 Tuma, N.B., Robins, P.K., 1980. A dynamic model of employment behavior: an application to the Seattle and  
1221 Denver Income Maintenance Experiments. *Econometrica* 48 (4), 1031–1052.
- 1222 Van Loon, R., 1979. Reforming welfare in Canada. *Public Policy* 27, 469.
- 1223 Watts, H., 1971. The graduated work incentive experiments: current progress. *American Economic Review* 61,  
1224 15–21.
- 1225 Watts, H.W., Avery, R., Elesh, D., Horner, D., Lefcowitz, M.J., Mamer, J., Poirier, D.J., Spilerman, S., Wright,  
1226 S., 1974. The labor-supply response of husbands. *Journal of Human Resources* 9 (2), 181–200.

- 1227 Watts, H.W., Dale, J., Poirier, D.J., Mallar, C., 1977a. Sample, variables, and concepts used in the analysis.  
1228 In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply  
1229 Responses. Academic Press, New York, pp. 33–56.
- 1230 Watts, H.W., Dale, P., 1977. The estimation of normal wage rates and normal income. In: Watts, H.W., Rees, A.  
1231 (Eds.), *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses. Academic Press,  
1232 New York, pp. 393–414.
- 1233 Watts, H.W., Horner, D., 1977. Labor-supply response of husbands. In: Watts, H.W., Rees, A. (Eds.), *The New*  
1234 *Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses. Academic Press, New York, pp.  
1235 57–114.
- 1236 Watts, H.W., Mamer, J., 1977. Analysis of wage-rate differentials. In: Watts, H.W., Rees, A. (Eds.), *The New*  
1237 *Jersey Income-Maintenance Experiment*, vol. III: The Impact on Expenditures, Health, and Social Behavior,  
1238 and the Quality of the Evidence. Academic Press, New York, pp. 341–352.
- 1239 Watts, H.W., Peck, J.K., Taussig, M., 1977b. Site selection, representativeness of the sample, and possible attrition  
1240 bias. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment*, vol. III: The Impact  
1241 on Expenditures, Health, and Social Behavior, and the Quality of the Evidence. Academic Press, New York,  
1242 pp. 441–466.
- 1243 Watts, H.W., Rees, A. (Eds.), 1977b. *The New Jersey Income-Maintenance Experiment*, vol. III: The Impact on  
1244 Expenditures, Health, and Social Behavior, and the Quality of the Evidence. Academic Press, New York.
- 1245 Watts, H.W., Rees, A., 1977a. *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses.  
1246 Academic Press, New York.
- 1247 Weiss, Y., Hall, A., Dong, F., 1980. The effect of price and income on investment in schooling. *Journal of Human*  
1248 *Resources* 15, 611–640.
- 1249 West, R., 1980a. The effects on the labor supply of young nonheads. *Journal of Human Resources* 15, 574–590.
- 1250 West, R., 1980b. Effects on wage rates: an interim analysis. *Journal of Human Resources* 15, 641–653.
- 1251 West, R., 1980c. Labor supply response of youth. In: Robins, P.K., Spiegelman, R.G., Weiner, S., Bell, J.G. (Eds.),  
1252 *A Guaranteed Annual Income: Evidence from a Social Experiment*. Academic Press, New York.
- 1253 Williams, W., 1972. *The Struggle for a Negative Income Tax*. University of Washington, Institute of Government  
1254 Research, Seattle, Washington.
- 1255 Wilson, J.O., 1974. Social experimentation and public-policy analysis. *Public Policy* 22, 15–37.
- 1256 Wooldridge, J., 1977. Housing consumption. In: Watts, H.W., Rees, A. (Eds.), *The New Jersey Income-*  
1257 *Maintenance Experiment*, vol. III: The Impact on Expenditures, Health, and Social Behavior, and the Quality  
1258 of the Evidence. Academic Press, New York, pp. 45–72.
- 1259 Wright, S., 1977. Social psychological characteristics and labor-force response of male heads. In: Watts, H.W.,  
1260 Rees, A. (Eds.), *The New Jersey Income-Maintenance Experiment*, vol. II: Labor-Supply Responses. Academic  
1261 Press, New York, pp. 321–346.
- 1262 Zellner, A., Rossi, P.E., 1986. Evaluating the methodology of social experiments. In: Munnell, A.H. (Ed.), *Lessons*  
1263 *from the Income Maintenance Experiments*. Federal Reserve Bank of Boston, Boston.