The Long-Term Effects of Cash Assistance*

David J. Price† and Jae Song‡

December 24, 2016

JOB MARKET PAPER

Latest version: http://web.stanford.edu/~djprice/papers/djprice_jmp.pdf

Online appendix: http://web.stanford.edu/~djprice/papers/djprice_jmp_appendix.pdf

Abstract

We investigate the long-term effect of cash assistance for beneficiaries and their children by following up, after four decades, with participants in the Seattle-Denver Income Maintenance Experiment. Treated families in this randomized experiment received thousands of dollars per year in extra government benefits for three or five years in the 1970s. Using administrative data from the Social Security Administration and the Washington State Department of Health, we find that treatment caused adults to earn an average of $1,800 less per year after the experiment ended. Most of this effect on earned income is concentrated between ages 50 and 60, suggesting that it is related to retirement. Treated adults were also 6.3 percentage points more likely to apply for disability benefits, but were not significantly more likely to receive them, or to have died. These effects on parents, however, do not appear to be passed down to their children: children in treated families experienced no significant effects in any of the main variables studied. Taken as a whole, these results suggest that policymakers should consider the long-term effects of cash assistance as they formulate policies to combat poverty and reduce inequality.

JEL Codes: I14, I32, I38, J22

*We are grateful to Gerald Ray at the Social Security Administration for his help and support. A special thanks to Nicholas Bloom, Raj Chetty, John Pencavel, and Luigi Pistaferri for their help and guidance with this project. We have also benefited greatly from comments from Emanuele Colonnelli, Mark Duggan, Daniel Garcia-Macia, David Grusky, Atul Gupta, Fatih Guvenen, Eran Hoffman, Hilary Hoynes, Caroline Hoxby, Mordecai Kurz, Moritz Lenel, Qian (Sindy) Li, Davide Malacrino, Magne Mogstad, Melanie Morten, Elena Pastorino, Santiago Pérez, Nicola Pierri, Juan Rios, Santiago Saavedra, Isaac Sorkin, Pietro Tebaldi, Alonso Villacorta, Constantine Yannelis, and seminar participants at Stanford University, the Bay Area Graduate Student Public and Labor Economics Conference, and the Institute for Empirical Macroeconomics at the Minneapolis Federal Reserve. We are also thankful for funding from the Social Security Administration through grants #1DRC12000002-03 and #1DRC12000002-04 to the National Bureau of Economic Research; and for funding from the Stanford University Vice Provost for Graduate Education. The views expressed herein are those of the authors and do not necessarily reflect the views of the Social Security Administration.

†Stanford University; djprice@stanford.edu

‡Social Security Administration; jae.song@ssa.gov

1 Introduction

Forty-three million people live in poverty in the United States, and over 700 million more live in extreme poverty around the world. Additionally, both policymakers and researchers are increasingly interested in inequality, which has been rising in many countries over the past few decades.[1] A widely-used strategy to address both poverty and inequality, and perhaps the simplest, is to give poor families cash to buy the goods they need.

Every year, tens of billions of dollars are given to families in the United States as cash assistance. This includes about \$8 billion through Temporary Aid to Needy Families (TANF), \$30 to \$140 billion through unemployment insurance,[2] and billions more from other sources. Recently, the idea of increasing the role of cash assistance is attracting interest, with proposals for policies or experiments on a basic income in Canada, Kenya, the United States,[3] and other countries. Despite the importance policymakers attach to cash assistance, little is known about the effect of this assistance on outcomes for beneficiaries or their children in the decades after the assistance is received. Identifying these long-term causal effects is difficult largely because families who receive cash assistance generally differ from those who do not, so no control group can be easily identified.

We overcome this difficulty, investigating the long-term effect of cash assistance on future earned income, further government financial assistance (Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI)), mortality, marriage, and divorce. We are able to identify these effects by following up, after four decades, with participants in the Seattle-Denver Income Maintenance Experiment (SIME/DIME), which began in 1970. This experiment, described in more detail in Section 2, guaranteed a minimum annual income of up to \$25,900[4] to about half of the 4,800 low- to middle-income families enrolled. Treated families, randomly chosen from among all enrolled families, received the full guaranteed income if they earned no outside income; they then faced taxes of 50% to 80% on outside income, up to the point where the program no longer benefited them. Treated families received this financial guarantee for three or five years, and treatment enabled an individual to receive, on average, \$2,700 extra annually in government benefits during the experiment, compared to control individuals who did not receive any SIME/DIME guarantee.

SIME/DIME and previous smaller Income Maintenance Experiments (IMEs) were originally proposed to determine how a negative income tax (NIT)—whereby the government gives money to families, rather than taking money away—would affect labor supply and other outcomes. The IMEs were large undertakings—according to Greenberg and Shroder (2004), SIME/DIME alone

[1] See Proctor et al. (2016) for United States poverty data, World Bank Group (2016) for world poverty data, and Piketty (2013) for data on inequality.

[2] See https://www.fas.org/sgp/crs/misc/RL32760.pdf and https://fred.stlouisfed.org/series/W825RC1A027NBEA.

[3] See https://news.ontario.ca/mcss/en/2016/06/ontario-moving-forward-with-basic-income-pilot.html, https://www.givedirectly.org/basic-income, and https://blog.ycombinator.com/basic-income.

[4] Unless otherwise noted, all dollar values in this paper are adjusted for inflation to 2013 dollars using the personal consumption expenditures (PCE) deflator available at https://research.stlouisfed.org/fred2/series/PCEPI/downloaddata?cid=21.

cost about $275 million—and short-term effects have been studied extensively.[5] Treatment caused adults to work about 12% fewer hours during the experiment. They also earned about $1,600 less per year. However, treated adults observed in the two years after the experiment ended did not work significantly different hours from control adults, or earn significantly different incomes.

Although short-term effects have been previously analyzed, no outcomes were measured after the 1970s. Since the IMEs were the first large-scale randomized controlled trials in the social sciences, they offer a singular opportunity to evaluate the long-term effects on participants of a large intervention. Adults from SIME/DIME have now had four decades for effects from the experiment to accumulate or taper off, so any long-term effects are likely to have already occurred. And SIME/DIME children, now adults,[6] have had many of the adult experiences that define their lives.

We are able to determine long-term outcomes by combining SIME/DIME data with administrative records from the Social Security Administration (SSA) and the Washington State Department of Health (WA DOH). Our main outcome measures come from SSA data, which include annual earned income between 1978 and 2013; information on applications for, and awards of, disability benefits; and mortality. Described in more detail in Section 3, the administrative data allow us to determine outcomes for participants even after they are no longer being surveyed. The use of administrative data also reduces the chance that results are biased by differential misreporting or attrition.

To our knowledge, no data set exists today that includes SIME/DIME participants' names, but a publicly-available data set does include each participant's birthday, sex, and relationships to other participants. These data are used to identify participants by finding matching patterns in the birth records of multi-child families, including public data from the WA DOH and restricted-use data on parent names from the SSA.[7] Using this method, we are able to match about half of SIME/DIME participants from multi-child families with a high degree of certainty. This matching technique is discussed in more detail in Appendix A.

Using this matched data set, we find new evidence of significant effects on adults decades after the experiment ended. On average, treatment decreased the probability that participants work in a given year by 3.3 percentage points (4.6% of the mean probability of working for adults in

[5]A review of the hundreds of papers discussing the original results from the IMEs is beyond the scope of this paper. Some results, as they relate to the present paper, are discussed throughout, particularly in Section 2. The most detailed discussion of the original results from SIME/DIME can be found in the final report by SRI International and Mathematica Policy Research (1983), with similar results in *The Journal of Human Resources*, Vol. 15, No. 4 and an overview in Office of Income Security Policy et al. (1983). A bibliography of contemporary papers from the IMEs is available at `http://www.irp.wisc.edu/research/nit/NIT_index.htm`. Proceedings from a conference on the IMEs are available in Munnell (1986). Widerquist (2005) reviews the literature on the IMEs, including literature from after the 1980s.

[6]For clarity, individuals who were the children of SIME/DIME heads of household are referred to as "children" in this paper even though they are now adults. The heads of household themselves are referred to as "adults," "parents," or "beneficiaries."

[7]SSA data on parent names, as well as individual records of all other SSA data and data that are commingled with SSA data, were handled on secure SSA computers by SSA personnel authorized to use that data for other purposes, following SSA data procedures.

our sample), and decreased average annual earnings by $1,800 (7.4% of mean annual earnings). Treated adults were also 6.3 percentage points (20% of the mean) more likely to apply for disability benefits (either SSDI or SSI), but were not significantly more likely to be awarded benefits, or to have died. These effects for adults, described in more detail in Section 4, are large relative to the cash assistance received: for every $1 in additional government transfers, we find that individuals earn discounted lifetime earnings that are $3.04 lower. The effects are mainly evident later in individuals' lives: for example, effects on annual earnings are $2,000 stronger between ages 50 and 60 than before that age.

Combining these results with those originally gathered during the experiment shows a pattern that may be surprising. Treatment decreases earned income during the experiment, causes no significant effect on work outcomes immediately thereafter, but again decreases earnings much later in life. However, because individuals generally choose to consume more leisure at the end of their lives, it is not surprising that they would consume additional leisure later in life, too. To formalize this idea, Appendix B explores a simple model in which agents exogenously prefer to work less as they age. In this model, observable effects of the SIME/DIME treatment on working are strongest later in life, when agents are closer to the margin of deciding to work less. Treatment affects agents in that model through assets, as agents save some of the transfer. As discussed in Subsection 4.2, treatment could also affect agents' wage rate because they worked less, lowering their human capital (or, as in Gibbons and Katz (1991), their inferred ability); or preferences and perceptions about not working (if leisure is habit-forming, or through other mechanisms). Although we cannot say conclusively which, if any, of these mechanisms explains the effect, we discuss the evidence for and against each.

Very little other research has been conducted on the impact of cash assistance programs—or, indeed, other types of government assistance—on adult beneficiaries themselves long after the assistance has ended. One exception is Wilde et al. (2014), who study the Connecticut Jobs First program, a welfare reform experiment. They find some evidence of increased mortality hazards over the following 15 years, though it is not statistically significant. Similar to our research on SIME/DIME's long-term effects on adults, Schmieder et al. (2012) find that the effect of extensions of unemployment benefits fades by 3.5 years after the benefits end, but they do not look at longer-term results. Some other papers analyze outcomes in the first few years after programs end—for example, Card and Hyslop (2005) analyze effects of a work-subsidy program after the program ended and note that there are no significant effects 1.5 years after subsidies end. However, very few papers consider outcomes even 5 years after a program ends. This is an important omission because significant post-experimental effects from SIME/DIME take more than 5 years to appear.

Our results on adults also relate to the extensive literature, beginning with Jacobson et al. (1993), on long-term effects of layoffs. Where that literature shows that involuntary job displacement can cause lower earnings far into the future, we find that the *voluntarily* decreased hours experienced by SIME/DIME participants are also associated with lower earnings later in life (although those lower earnings may be mediated by factors other than the decreased work, such as changed preferences). Finally, similarly to studies on lottery winners such as Cesarini et al. (2015) and Imbens et al. (2001), we find that unearned income decreases labor earnings, but the effect we find is substantially larger relative to the cash received. This may be due to the longer time period we study, or because of the stronger work disincentive effects of the SIME/DIME treatment relative to a lump-sum transfer.

The long-term effect of parental cash assistance on children remains an empirical question that has not been adequately explored. Such assistance could increase children's long-term take-up of government benefits and decrease earnings; it could have the opposite effect; or it could have no significant effect. Based on adult outcomes, one might expect similar effects on children. Indeed, Murray (1984) cites the original IME findings in asking, "Does welfare undermine the family? As far as we know from the NIT experiment, it does, and the effect is large." Some research indicates that parental receipt of government benefits increases the probability that their children will receive benefits themselves,[8] a concern that is enshrined into law.[9] On the other hand, a growing literature documents that parental income, or factors related to income, can have long-lasting positive effects on children. More income can allow parents to buy more of the goods and services that their children need to succeed, including education, health care, or access to a better neighborhood; or the income guarantee could give parents the ability to be more involved in their children's lives with increased time or decreased stress. Literature reviews on this topic include Black and Devereux (2011), Cooper and Stewart (2013), Currie (2009), and Solon (2015). Finally, it is possible that cash assistance—at least, past a certain point—does not affect the factors that matter for children in the long run.[10] Some prior research, including research discussed in the review articles mentioned above, finds little scope for an effect of parental income, or factors related to it, on outcomes for children.

In fact, in contrast to the significant effects on parents, we find little evidence of an effect on children for any variable studied. For example, as discussed in Section 5, we can rule out at the 5% level treatment changing the probability of a child applying for either SSDI or SSI by more than 3 percentage points (12% of the mean). This null result is consistent with intergenerational welfare transmission being driven by information, as hypothesized by Dahl et al. (2014), because information about SIME/DIME would not be helpful for children in applying for SSDI or SSI benefits. (Dahl et al. (2014) found that parents receiving disability benefits caused their children to receive those benefits, but not other types of benefits.) We can also rule out effects on child propensity to work of more than 1.9 percentage points (2.5% of the mean), and a change in annual earned income of more than $1,500 (6.9% of mean annual earnings for children). These results

[8] See, for example, Dahl et al. (2014), Gottschalk (1996), and Pepper (2000).

[9] The authorizing legislation for TANF notes that "Children born into families receiving welfare assistance are 3 times more likely to be on welfare when they reach adulthood than children not born into families receiving welfare." See `http://www.gpo.gov/fdsys/pkg/PLAW-104publ193/pdf/PLAW-104publ193.pdf`.

[10] In particular, the cognitive and non-cognitive skills that may be vital for children's later success, as documented in a literature reviewed by Kautz et al. (2014), may not be strongly influenced by cash assistance.

allow us to rule out effects on earned income of the size found by Aizer et al. (2016), who study a similar program and find that treated children earned 14% more earnings. Because Aizer et al. (2016) studied a population targeted as particularly needy, it may be that cash assistance helps children who are in the most need while it does not improve the lives of others who are in less need. This research supports other work that has found little evidence that cash for parents benefits their children in the long term, such as that done by Bleakley and Ferrie (2013), Mayer (1997), and Shea (2000). It should also be read in the context of papers that do document positive long-term effects of parental income, including Hoynes et al. (2016) and Oreopoulos et al. (2008). (In Section 5, we do document some significant effects on some child outcomes for certain subgroups of the population—those whose parents received treatment longer, and the youngest children. As discussed below, however, some caution is necessary in interpreting these results, particularly because of the number of tests that were run on child outcomes.)

Taken as a whole, our results suggest that cash assistance could have unintended and unexpected long-term consequences for recipients without significantly improving their children's earning potential or decreasing their propensity to use government benefits. On the other hand, in our context, we can rule out the idea that cash assistance creates a welfare culture that decreases children's earned incomes or their dependency on disability benefits by a large amount. Of course, cash assistance could have very different effects in other contexts, such as for individuals who have no access to another safety net. Further, we cannot make welfare comparisons without knowing why treated adults worked less and applied more for disability benefits. More research is also needed to compare long-term effects of assistance on adults to such effects from other policies, about which little is known. We return to a discussion of policy implications in the conclusion.

The remainder of this paper proceeds as follows. Section 2 describes the institutional background of the IMEs. Section 3 describes the SIME/DIME and administrative data used in this paper. Section 4 presents the analyses and results on adults, while Section 5 does the same for results on children. Section 6 concludes with a policy discussion.

2 The Income Maintenance Experiments

2.1 General background on the experiments

The Income Maintenance Experiments were conceived in the 1960s to test possible changes to the welfare system.[11] Many at the time believed that a more generous welfare program could help families out of poverty. Additionally, because two-parent households generally received much less in public benefits than households with a single female head, it was thought that the welfare system encouraged marital dissolution. The idea of a simple but generous NIT to replace all other benefits appealed to both conservatives and liberals, but policymakers were concerned that such generosity

[11] Unless otherwise noted, the background described in this subsection is drawn from Office of Income Security Policy et al. (1983), Spiegelman (1983), or new analysis of SIME/DIME data.

Table 1: Pre-experimental data on SIME/DIME families and adults

Level	Variable	Fraction
Family	Seattle	.434
Family	Two household heads	.38
Family	Black	.389
Family	White	.415
Family	Chicano	.196
Family	Positive pre-exp benefits	.462
Adult	Positive pre-exp earned inc	.656
Adult	Male	.36
Adult	Education: HS+	.537
Adult	Education: college+	.025

Notes: Based on public SIME/DIME data for original families with at least two children. "Family" data are based on one observation per original family; "Adult" data are based on one observation per original family household head. "Fraction" indicates the fraction of the families or individuals who have the listed characteristic. "Positive pre-exp benefits" indicates that the family received some government benefits in at least one of the nine months at the start of the experiment, before treatment began. "Positive pre-exp earned inc" indicates that earned income is positive for at least one of the same first nine months. "Education: HS+" indicates at least 12 years of schooling; "Education: college+" indicates at least 16 years of schooling.

would discourage work effort and lead to welfare dependency. (Policymakers also had such concerns about the welfare system already in place at the time, for which official effective tax rates could approach 100%.) To determine if these concerns were valid, a series of IMEs were funded by the federal government: the New Jersey IME (in New Jersey and Pennsylvania, from 1968-'72); the Rural IME (in Iowa and North Carolina, from 1969-'73), the Gary IME (in Indiana, from 1971-'75) and SIME/DIME. SIME/DIME, funded by the US Department of Health, Education, and Welfare, included more families than all the other IMEs combined, and was also more generous per family. The IMEs have been extensively studied; for more details, see citations in Footnote 5.

Some characteristics of the SIME/DIME families in our sample are displayed in Table 1. As discussed in Subsection 3.2, we are only able to analyze long-term outcomes for families with at least two children; for comparability, we therefore limit analysis in this section, and elsewhere in the paper, to those families. Our sample, which includes 3,400 of the 4,800 families who enrolled in SIME/DIME, were generally of low socio-economic status. About half of the adults had fewer than 12 years of schooling; only about two-thirds of adults had any pre-experimental earned income;[12] and those who did have such income had average annual earned income of $23,000.

Each family enrolled in SIME/DIME was assigned to a treatment category using stratified

[12]Here, and throughout this paper unless otherwise noted, pre-experimental earned income refers to income earned in the nine months before the experiment: January to September of 1970 in Seattle and the same months of 1971 in Denver (when discussing annual earned income, this is multiplied by $\frac{4}{3}$). These are the only months of consistently-recorded pre-experimental data before treatment began. Earned income includes income from wages, bonuses, tips, commissions, payments-in-kind, self-employment receipts, and odd job income; the vast majority comes from wages.

random assignment in 1970-'71 (in Seattle) or 1971-'72 (in Denver), as discussed in Subsection 2.2. A family of four that was assigned to the financial treatment[13] would be given an annual transfer of $17,600, $22,200, or $25,900 (depending on the treatment group they were assigned to) if they earned no other income, and payments were adjusted for family size. Lower-income families were more likely to be assigned to lower guarantee levels. Every extra dollar the family earned would be taxed back at a rate between 50% and 80% (with the precise rate selected randomly),[14] and treatment lasted 3 or 5 years (again, depending on the randomly-assigned treatment group).[15] (To fix ideas, suppose a family received the $22,200 guarantee with a tax rate of 50%. If they earned $10,000 of outside income in the year, their total take-home income would be $27,200: the $22,200 guarantee, plus $10,000 of earned income, minus $5,000 of taxes.) Treatments were, on average, weighted to be more generous for those at higher income levels so that everyone would have a similar chance to receive the benefits. To control treated families' incentives better, almost all other government benefits were taxed at a rate of 100%; and any income taxes paid were refunded, up to the point where the treated family would have the same income whether on or off the treatment. The transfer was paid monthly and, if a family split up, both new families would be eligible.

Families not receiving SIME/DIME benefits could receive government transfers from a variety of programs, including Aid to Families with Dependent Children (AFDC), unemployment insurance, and food stamps. However, SIME/DIME benefits were generally much more generous. (Some single-headed families with the least generous treatment might have been able to get more money from other programs; however, each family could choose in each month whether to take SIME/DIME payments or other payments, so treatment could not reduce a family's choice set.) Treatment caused individuals to receive, on average, $2,700 more in annual government benefits; Figure 1a shows the effect of treatment on total government transfers received each year, for the first 5 years after assignment to treatment.[16]

Control families not receiving government benefits generally faced combined federal and state marginal income tax rates of around 20% to 35%.[17] However, government benefits received by con-

[13]SIME/DIME also included a "manpower" treatment. Families in that treatment were all given job counseling. Additionally, some families in the manpower treatment were given 50% or 100% subsidies for education. In our main specification, we control for manpower treatment status; however we do not analyze its effect in detail as it is likely to be less generalizable to economic questions and policies today. Unless otherwise specified, "treatment" refers to financial treatment.

[14]For some families, the average tax rate declined by 2.5 percentage points for every nominal $1,000 earned. So, for example, if a family with this decline facing an initial tax rate of 80% earned a nominal $2,000, their actual average tax rate would be $80 - 5 = 75\%$, so they would pay a nominal $1,500 in taxes (after receiving the full guarantee).

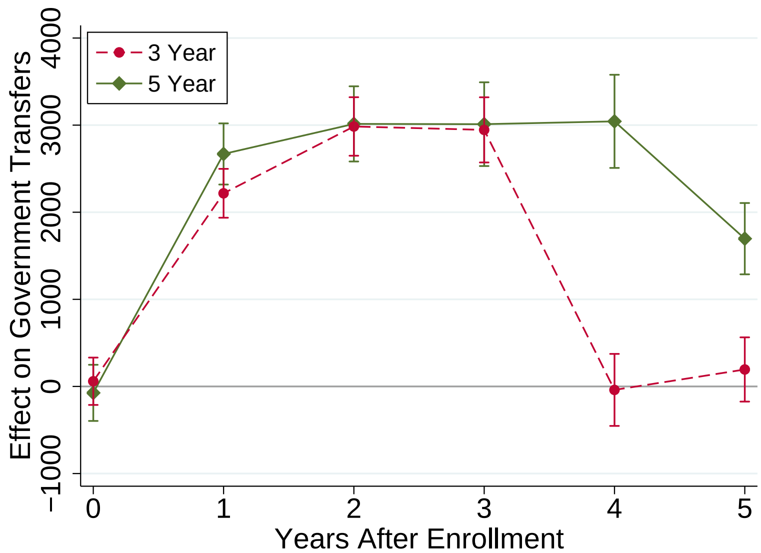
[15]In the middle of the experiment, about 150 Denver families were told that they would be guaranteed the treatment for 20 years. However, they were not actually given the treatment for this full time. These families are not included in our baseline analysis, as their treatment is different from that studied here. As noted by Robins (1984), there are few significant differences between this group and the remainder of the SIME/DIME families. Indeed, as shown in robustness checks in Tables C.2 and C.5, our results are very similar if they are included.

[16]Benefit data are recorded at the family level. For comparability with earnings data, which are recorded at the individual level, we report benefits divided equally among household heads.

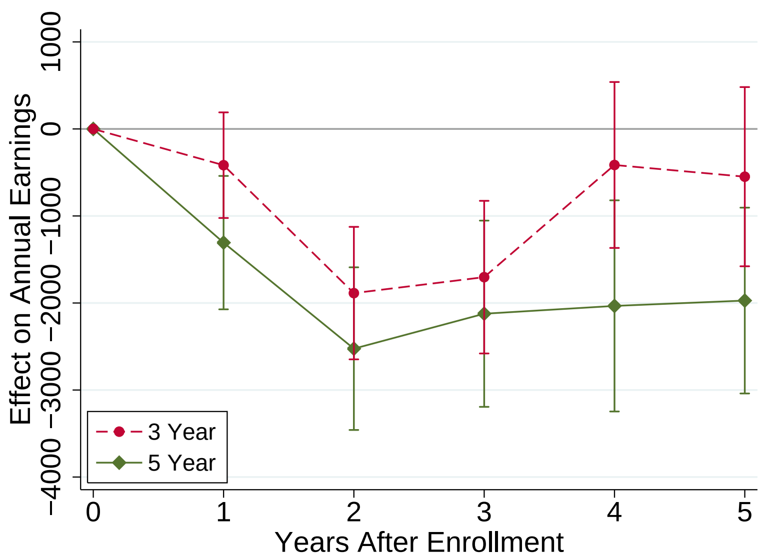
[17]See Tax Foundation (2013), Social Security Administration (2016), and Schrock (2010).

Figure 1: Effects on government transfers and earnings during and immediately after treatment

(a) Benefits



(b) Earned income



Notes: Based on public SIME/DIME data for original families with at least two children. Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from a certain number of years into the experiment. Confidence intervals are based on standard errors that are clustered at the level of the original family. Each regression includes those treated for the given number of years, plus all non-treated individuals. The dependent variable in Figure 1a is total government benefits, including SIME/DIME payment; the dependent variable in Figure 1b is earned income. Benefit levels are apportioned equally to each household head for comparability with earned income data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth.

trol families created effective tax rates that were much more complex. The precise rate depended on which benefits the family was eligible for, which in turn depended on age, family composition, work expenses, other government benefits received, and other factors. Additionally, many families eligible for benefits do not take them up, and (as with SIME/DIME treated families) some families misreport earnings to increase their benefit level. Further, several benefit programs featured notches—points in the income distribution beyond which benefits drop discretely to zero—at which the marginal tax rate is extremely high. (Indeed, eliminating these complications was part of the appeal of the SIME/DIME treatment for some policymakers.)

Thus the statutory tax rate can differ substantially from actual effective tax rates for those on government programs. Hutchens (1978) estimates the effective tax rate from AFDC as 41% in Washington State in 1971, much less than the statutory 67%. Moffitt (1979) estimates effective tax rates of around 50% for some low-income families in Indiana in 1973, combining income taxes, AFDC, food stamps, and Medicaid. Halsey (1978) uses SIME/DIME data to estimate effective tax rates for program participants that vary widely, but are generally between around 30% and 50%.

Treatment therefore generally increased families' effective tax rates in addition to serving as a cash transfer. Increases in tax rates and in wealth are both generally thought to cause decreased labor supply, and, as shown in Figure 1b, treatment reduced earned income by an average of $1,600 per year.[18] This was driven by the fact that treated individuals reduced their hours of work by an average of 12% during treatment, mostly taken as longer non-employment spells. Labor supply responses were generally larger for families in the 5-year treatment group and for those with more generous guarantees, though different tax rates did not appear to cause significantly different effects. As shown in Figure 1, no significant effects are observed on either unearned or earned income after the experiment ended for those treated for 3 years (post-experiment data are not available for the 5-year treatment group).

Combining all forms of income in a household—including earned and unearned income for all family members—and estimating taxes using TAXSIM (as provided by Feenberg (2016)) treatment increased families' after-tax income by an average of $640 per year. (This number is particularly sensitive to potential misreporting biases; adjusting the data based on the biases found by Greenberg and Halsey (1983), treatment increased annual family after-tax income by $2,000 per year.) The additional monetary resources came at the same time as additional non-working time, which could be spent with children, on home production, on leisure, or in any other way.

In addition to effects on income and work, a second important set of results were that the treatment decreased marital stability. Groeneveld et al. (1983) found that treatment caused black and white families to be approximately 40% more likely to split up; no significant effect was observed

[18] Some care must be taken in interpreting the original results on earnings, which could be affected by systematic misreporting or attrition. Neither factor seems to overturn the main experimental results of significant labor supply effects, as noted by Greenberg and Halsey (1983) and Pencavel (1979) for misreporting, and by Hausman and Wise (1979) and Robins and West (1986) for attrition. However, underreporting in particular did seem to bias the results toward a stronger labor supply effect. In particular, adjusting the data based on the biases found by Greenberg and Halsey (1983), treatment only caused earned income to decline by an average of $1,000 per year.

for Chicano families. (There is some disagreement about the robustness of the results on marital stability; see, for example, Cain and Wissoker (1990a,b) and Hannan and Tuma (1990).) Because of the importance of these findings, we explore effects on marriage and divorce for both parents and children. (We discuss these results mainly in Online Appendix C because marriage and divorce results are based on WA DOH data, and are therefore less comprehensive than other results.)

Evidence of effects on families beyond labor supply or marital stability is somewhat limited by what Hanushek (1986) calls the "tag-on nature" of research about non-labor supply effects, which "were not given the same degree of attention" in the design of the IMEs. Broadly, however, few systematic significant effects were observed on types of goods consumed, fertility, child academic outcomes, or other indicators.

2.2 Assignment to treatment groups in SIME/DIME

Assignment to treatment in SIME/DIME was based on the "Conlisk-Watts Assignment Model," a stratified random design described by Keeley and Robins (1980) and others. Families were stratified into groups on the basis of their site (Seattle or Denver), race (black, white, or Chicano),[19] family type (headed by one or two adults), and "normal income" level (one of six categories based on a subjective evaluation of the family's typical income). Statistics about the site, race, family type, and normal income level of participating families are shown in Table 1 and Figure 2, along with other details about the sample. According to published accounts, within these groups, treatment was assigned randomly.

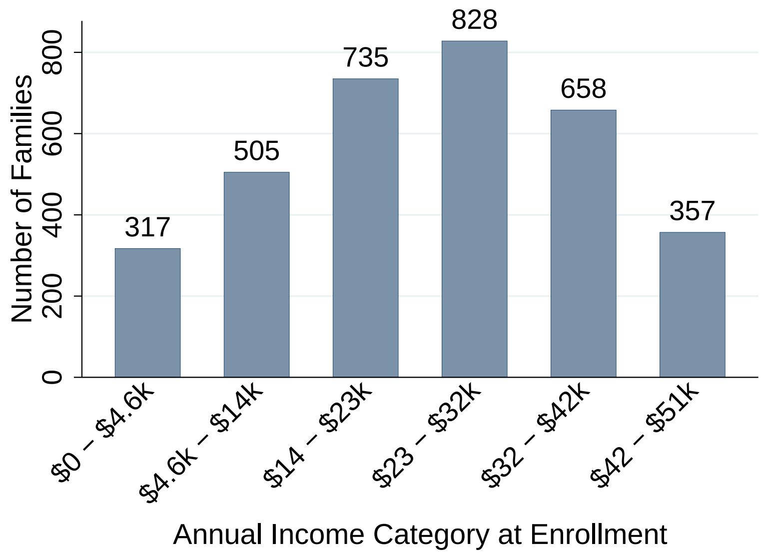
A balance test on pre-experimental variables is shown in Table 2; based on this test, treatment and control may not have been balanced in Seattle. Controlling for assignment groups, treated Seattle individuals earned \$1,400 less than controls in annual pre-experimental earned income (compared to an average of \$15,000). Significant differences also exist in pre-experimental hours worked. Such a pre-experimental difference could occur by chance, though that is unlikely.

One potential cause of the imbalance is differential attrition: as noted by Christophersen (1983), some families were assigned to enrollment but not enrolled. Some of these families were not enrolled because they could not be found; others because they were no longer eligible for treatment, due to having moved out of the city, having left the labor market, or having experienced a change in family structure that made them ineligible. Because not being locatable and eligibility changes were based on decisions made before families knew their assignment status, attrition due to these factors are unlikely to lead to biased estimates. However, 7% of eligible families who were located refused enrollment. We have little information on who these families were—even the fraction who were assigned to treatment. Thus although the attrition rate is fairly low, it is difficult to quantify any bias it might have caused.

Because we do not have more information about the cause of the imbalance, we attempt to correct for it by controlling for adult pre-experimental earned income in all regressions. However, a

[19] We follow official SIME/DIME terminology in the names for these groups, and in calling them all races.

Figure 2: Normal income levels



Notes: Based on public SIME/DIME data for original families with at least two children. Data is based on one observation per original family. Normal income level is based on a subjective evaluation of the family's typical income, scaled to be comparable to a family of 4; this evaluation was made before assignment to treatment status. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE.

simple control may not eliminate all bias. It is therefore important to note an important difference between Seattle and Denver: assignment to treatment status took place in Denver separately and at a later date. Christophersen (1983) comments that "Denver benefited greatly from the Seattle experience," and that "to a degree, the Seattle operation served as a pilot for the entire Denver operation." Indeed, there is no evidence of a statistically significant imbalance in Denver. It is possible, then, that a problem with enrollment in Seattle caused the imbalance, but that this was corrected in Denver. We therefore also present all results restricting the sample to Denver only, in Online Appendix D. Estimated effects are similar in Denver only (compared to the combined Seattle-Denver sample), though they are less precisely estimated.

3 Data and methods

3.1 Data

Data from SIME/DIME itself are almost exclusively from Mathematica Policy Research, Inc. (2000a,b), though some data, particularly for robustness checks, were derived from Department of Health, Education, and Welfare (1978). We report outcomes for adults who were household heads at the start of SIME/DIME, and any biological children who were under 18 years old when

Table 2: Pre-experimental balance test

Variable	Whole Sample	Seattle Only	Denver Only
Earned income	-625**	-1449***	63.2
	(300)	(492)	(366)
Hours worked	-22.4	-74.6**	21.2
	(20.6)	(31.4)	(27.2)
Gov't benefits	206	425	22.1
	(205)	(350)	(236)
Years of ed	-.0453	.0588	-.125
	(.075)	(.117)	(.0977)
Kids age 0-5	.0354	.0536	.0202
	(.0359)	(.0528)	(.0491)
Kids age 6-15	-.001	.0275	-.0249
	(.0518)	(.075)	(.0716)
People age 16+	-.0764	-.113	-.046
	(.047)	(.0709)	(.0627)

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Based on public SIME/DIME data for original families with at least two children. Each cell reports the results of one regression with the dependent variable given by the row, for the subgroup given by the column. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. "Earned income," "hours worked," and "gov't benefits" are based on totals in the nine months at the start of the experiment, before treatment began. "Years of ed" measures adult education, while "Kids age _-_" measures the number of children in the given age range, before the experiment began. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE.

the experiment began.[20]

We measure labor market outcomes using income data from the SSA's Master Earnings File (MEF). The MEF contains a comprehensive record of income reported in Box 1 of Form W-2, as well as all self-employment income, between 1978 to 2013. Earned income data (along with all other monetary data in this paper, as noted in Footnote 4) are adjusted to 2013 dollars with the PCE deflator. Because the MEF is such a comprehensive record, if an individual does not have income data in the MEF for a given year, we assign them to $0 of earnings in that year. Unless otherwise noted, we restrict income data to that earned in prime working age, between 20 and 60. Earned income is top-coded at $100,000 so that results are robust to outliers; only 1.9% of annual observations for children and 1.7% for adults are above this level.

To explore the effect of SIME/DIME on interactions with the disability system, we use the SSA's administrative data on SSDI and SSI applications and awards. These data come from the SSA's Master Beneficiary Record, the SSA 831 file, and the Supplemental Security Record file. Together, these files represent a comprehensive record of SSDI and SSI beneficiaries. They are also a comprehensive record of applications for SSDI and SSI benefits beginning in 1990, and include about 81% of applications between 1978 and 1989, with no known systematic difference between denied applications included or not included in the data sets.[21]

We are able to study mortality using data from the SSA Numident file, which is the source of data for the SSA Death Master File. According to Hill and Rosenwaike (2001), these data report around 95% of deaths for individuals over 65 during most the time period we study. However, death records are less comprehensive for younger individuals. (For this reason, and because there are so few deaths for children in our sample, we do not include mortality as a main variable for children.) In theory, missing death records could be problematic; if an effect on SSA program participation had occurred, that could lead to a biased measure of effects on mortality because SSA death records are likely more complete for SSA beneficiaries. As shown below, though, we find no evidence of significant effects on SSA disability awards. Further, as a robustness check, we use WA DOH death data from 1979 to 2013 (matched to SSA records with Social Security numbers (SSNs)) and confirm that results are similar.

[20]Biological children who were 18 or over at the time of assignment are not included in outcome data because there are fewer plausible mechanisms for them to be affected by their parents' financial treatment. These older children are included, though, in the matching procedure described in Appendix A. Note that any income earned by children in a household is counted toward the tax rate; thus there is a possibility for older children to be directly affected by SIME/DIME treatment. However, as shown in Table C.6, results for children aged 6 to 10 at the start of the experiment (who could not work during it) and for those 11 or older (who could) are similar.

[21]Because all SIME/DIME participants were required to be able-bodied at the start of the experiment, few are likely to have applied for disability benefits before 1978; indeed, less than 4% of the disability awards for SIME/DIME participants came from individuals who applied before 1978, and treatment did not significantly affect those awards. Missing application data between 1978 and 1989 would be a particular concern for results on applications by parents, where treatment was found to increase the probability of applications. Such a result could be generated if treatment caused individuals instead to shift applications from the 1980s to the 1990s, with many applications in the 1980s not observed. However, this is unlikely to be the case: indeed, based on data we do have, the point estimates of the treatment effect on applications before 1990 is positive. Thus our results might understate the true effect on disability applications.

We are also able to explore marriage and divorce using public data from the WA DOH from 1977 to 2013. These records are matched to Social Security records based on name and date of birth for all Seattle participants and children. Because they are based on state records, these measures are less comprehensive than the SSA data on earned income and disability benefits. In particular, individuals who left Washington would not be in these records; this could be problematic if treatment caused individuals to differentially leave the state, which we have no way of testing. However, we include these vital outcomes in our analysis because they provide the only measure of important potential effects on SIME/DIME participants and their children.

3.2 Matching experimental families to outcome data

Individuals from SIME/DIME are matched to outcomes using the procedure described in detail in Appendix A. To summarize, we look at patterns of family birthdays in SIME/DIME records and match them to similar patterns in the SSA's Numident and WA DOH birth records. For example, suppose a mother has three male children born on February 1 of 1960, 1961, and 1962. It is unlikely that another family has exactly the same birth pattern. Thus if we find three male births, on those days, with the same mother name, we can be reasonably confident that they are the same family. This procedure is only possible for families with at least two children; we therefore restrict all analysis in this paper to those families. After the initial match, we perform a placebo test by adding a certain number of days to each birthday and rerunning the match; we then use the number of matches found using the real and placebo birthdays in a maximum likelihood procedure to estimate the probability that a match is correct. In our baseline specification, we include all SIME/DIME individuals who are matched to exactly one SSN with at least 95% confidence. With this algorithm we match 45% of parents and 59% of children. There is no significant effect of treatment on the probability that we find either parents or children overall, or within various subgroups, as shown in Tables C.3 and C.6. As discussed in Appendix A, we estimate that 5.2% of matched adults and 1.3% of matched children are matched to an incorrect SSN. The rate of false matches for adults is comparable to that if SSA data are matched on name and date of birth, while the false match rate for children is better.

Summary statistics for main outcome variables for these matched individuals (in both treated and control families) are shown in Table 3. That table also includes data on a comparison group, which is based on a random sample of individuals born in Washington (for Seattle families) and Colorado (for Denver families), with state of birth, sex, and year of birth weighted to be equal to the SIME/DIME matches. Because SIME/DIME families were selected to have low or middle incomes, both parents and children have significantly lower average annual earnings in SSA data than the comparison group. They also are more likely to apply for, and receive, disability benefits, and are more likely to have died.

These differences are all reminders that results may be difficult to generalize, for several reasons. First, the participating families were only those who volunteered, among low- to middle-income fam-

Table 3: **Summary statistics based on outcome variables**

Variable	Parents			Children		
	Sample Mean	Comp Mean	p-value	Sample Mean	Comp Mean	p-value
Positive Annual Earnings	.709	.701	0.332	.769	.798	0.000
Annual Earnings	23748	27143	0.000	22281	27704	0.000
Applied SSDI/SSI	.318	.17	0.000	.245	.145	0.000
Awarded SSDI/SSI	.247	.131	0.000	.13	.0873	0.000
Died	.385	.298	0.000	.0728	.0556	0.000

Notes: "Sample" refers to the same SIME/DIME matched sample described in Section 3. Comparison group data ("comp mean") is based on a random sample of individuals born in Washington (for Seattle families) and Colorado (for Denver families), with state of birth, sex, and year of birth weighted to be equal to the SIME/DIME matches. "p-value" refers to the difference in means between SIME/DIME families and the comparison group. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data.

ilies in specific neighborhoods in two cities in the 1970s. Further, we are only able to study families with at least two children, and we are only able to use data on about half of these families, who are more likely to be in larger families and families with rare last names. These families studied here may react differently to cash assistance than families that would be affected by current policies. Furthermore, we are only able to measure effects of the SIME/DIME treatment itself rather than any policy currently under consideration. For example, the policy and experiment proposals in Canada, Kenya, and the United States mentioned in the Introduction are for "basic income" guarantees, which are similar to the NITs studied here but with tax rates of 0%. Proposed policies would also likely last longer, and include a far greater portion of the population, than SIME/DIME. We are able to analyze the effect of small variations in treatment because SIME/DIME included different treatments (among other variations, treated individuals were randomly allocated to different treatment lengths, guarantee levels, and tax rates). However, these variations do not cover all treatments we would be interested in. In particular, we can say little about general equilibrium effects of this treatment because there were very few treated families relative to the Seattle and Denver metropolitan areas. Finally, SIME/DIME control families were also able to use AFDC, food stamps, unemployment insurance, and other welfare programs. For this reason, our paper compares generous cash assistance to a standard welfare program, rather than comparing a welfare program to a lack of such a program. Despite these limitations, long-term outcomes for SIME/DIME families are important to study because there are so few other settings where long-term effects of similar interventions can be analyzed.

3.3 Empirical methods

All causal effects reported in this paper, unless otherwise specified, are based on a least squares regression of the outcome of interest against a dummy variable for initial assignment to treatment, along with other covariates. For outcomes where we have one observation per person, such as applications for disability benefits, we estimate

$$y_i = \gamma D_i + \mathbf{S}_i \delta + \mathbf{X}_i \beta + \epsilon_i, \tag{1}$$

where y_i is the outcome of interest for individual i. When the outcome of interest is an event, such as applying for disability benefits, the dependent variable is a dummy for whether the event has occurred in our data. D_i is an indicator variable that takes a value of 1 if person i is from a treated family. $\mathbf{S}_i$ is a vector of indicator variables for membership in each stratification group: unique combinations of site (Seattle or Denver), race (black, white, or Chicano), family type (headed by one or two adults), and the adult's pre-experimental "normal income" category. Finally, $\mathbf{X}_i$ is a vector of demographic variables: sex, manpower treatment status, a cubic polynomial in date of birth, and adult pre-experimental earned income (for children, data on adult pre-experimental income, as well as any other data on adults, are based on the primary breadwinner: the parent who earned the most pre-experimental income). When the outcome of interest is available at an annual frequency, as with most variables based on income, we estimate

$$y_{it} = \gamma D_i + \mathbf{S}_i \delta + \mathbf{X}_{it} \beta + \lambda_t + \epsilon_{it}, \tag{2}$$

with one observation per person i, per year t. This specification allows for unrestricted year effects λ_t; all other variables are the same as in Equation 1, except that $\mathbf{X}_{it}$ includes age in year t rather than year of birth. In either case, standard errors are clustered at the level of the original family, as constituted at the start of SIME/DIME, which is the level at which randomization occurred.

Many graphs, such as Figure 3a, show data at an annual frequency. Each point on this graph represents the results of a single regression using the methodology described above. For example, each point in Figure 3a represents the estimate and 95% confidence interval for the regression coefficient on treatment status, where the dependent variable is earned income; data is restricted to that from the year that an individual turned a given age. Figure C.1c shows, for each point, the results of a regression for whether adults had applied for disability insurance by a given number of years into the experiment, beginning with 1 in 1971 (for Seattle) or 1972 (for Denver).

4 Outcomes for adults

4.1 Results

For adults, we focus on five main outcomes of primary economic interest. We estimate the effect of treatment on annual work (a dummy for whether the individual earned any income in each year); the amount of money earned in each year; whether the individual applied for either SSDI or SSI benefits; whether they were awarded them; and whether they had died by the end of the period analyzed.

Effects on these five outcomes for adults are shown in Table 4; in Table 5, we explore the intensive margins associated with several of these outcomes. Treatment caused adults to be 3.3 percentage points less likely to work in a given year. In column 1 of Table 5, we see that this effect is not explained by any differential mortality: the effect is nearly identical if we include only years in which the individual is not known to have been dead. Partially because they work fewer years, treatment caused individuals to earn \$1,800 less per year; this decrease represents 7.4% of the participants' \$24,000 mean annual earnings (for both treated and untreated participants). As shown in column 2 of Table 5, there is no significant effect of treatment on annual earnings conditional on working in a given year; however, the large negative point estimate indicates that there could be an important effect on this intensive margin. The effect on lifetime earnings is quite large relative to the initial cash assistance shock. Discounting future earnings at 3% (after adjusting for inflation) and summing measured annual effects, treatment caused individuals to earn, on average, \$3.04 less in lifetime earned income during their prime working years for every dollar of extra government transfers during the experiment. This includes \$0.64 less earnings during the experiment and \$2.40 after. (These numbers are somewhat sensitive to the discount rate, particularly because post-treatment effects are strongest later in life. However, even a 10% rate implies \$0.62 lower earnings during the experiment and \$0.87 after.) This effect of government benefits on earned income is substantially higher than that estimated by Cesarini et al. (2015) or Imbens et al. (2001), who study pure wealth shocks. We return to this point below.

Treatment also caused adults to be 6.3 percentage points more likely to *apply* for disability benefits, an increase of 20% on the 32% chance that the average (treated or untreated) participant would apply for such benefits. This effect does not appear to be related to underlying medical conditions: treated individuals are no more likely to have been *awarded* disability benefits, or to have died by 2013. In fact, as shown in column 3 of Table 5, treatment increased the chance of being rejected for disability benefits, among those who did apply. Thus the individuals who are induced to apply for disability benefits are judged by the SSA upon application to be less deserving of those benefits than the average applicant from our sample. Columns 4 to 8 of Table 5 show that treatment also somewhat changed the mix of impairments that individuals applied under, making them less likely to apply based on a mental disorder or a less-common impairment. As noted by Autor and Duggan (2003), the increase in SSDI beneficiaries with mental disorders (along

Table 4: Parents, effects on main outcomes

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Treated	$-.0329^{**}$	-1761^{**}	$.0628^{***}$	.0216	.0138
	(.0136)	(816)	(.0199)	(.019)	(.0196)
Dep var summary stats					
Mean	.709	23748	.318	.247	.385
Std. Dev.	.454	25161	.466	.432	.487
N	52867	52867	2280	2280	2280
People	2252	2252	2280	2280	2280
Clusters	1699	1699	1720	1720	1720

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family.

Table 5: Parents, intensive margins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Awarded SSDI/SSI**	**Cancer**	**Circul-atory Disorder**	**Musculo-skeletal Disorder**	**Mental Disorder**	**Other Impair-ment**
Condition	**Alive**	**Earn>0**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**
Treated	$-.0327^{**}$	-1030	$-.0716^{**}$	.0161	$-.0255$	.00725	$-.078^{**}$	$-.0709^{*}$
	(.0128)	(806)	(.0362)	(.0246)	(.0338)	(.041)	(.0336)	(.0418)
Dep var summary stats								
Mean	.74	33514	.727	.0898	.199	.387	.211	.514
Std. Dev.	.438	23794	.446	.286	.399	.487	.409	.5
N	50458	37461	724	724	724	724	724	724
People	2236	2105	724	724	724	724	724	724
Clusters	1692	1609	651	651	651	651	651	651

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Dependent variables in columns 4 to 8 are indicators for whether the individual ever applied for disability benefits on the basis of the listed impairment. Independent variable "treated" indicates whether the individual was in a treated family. Observations are only included if they fit the condition listed. "Alive" indicates that the individual is not listed as having died in SSA records by the given year; "Earn> 0" indicates that the individual earned positive income in the given year; and "Applied SSDI/SSI" indicates that the individual ever applied for disability benefits.

with those with musculo-skeletal disorders) is an important component of the rapidly increasing disability rolls. That treatment actually decreased the probability of applying based on a mental disorder suggests that the mechanism at work here may be unlikely to be related to the increase in SSDI receipt over the past few decades. Even so, these results can inform our understanding of the economic factors that cause individuals to apply for assistance from SSA disability programs.

These results are generally quite robust to alternative specifications. Table C.2 presents a variety of robustness checks on these results, while Online Appendix D presents these results, along with all others, for Denver only. Point estimates for each variable under different specifications remain similar, and remain statistically significant under almost any alternative specification. Estimates are also generally similar among different subgroups of the population, as shown in Table C.3. Although some differences are statistically significant, there are few *systematic* differences among the groups. For example, the effect on annual earned income is significantly higher (or less negative) at the 10% level for those in the lowest pre-experimental income category (under \$14,000) than the middle category (between \$14,000 and \$32,000); and the effect on whether they earned income is higher (at the 10% level) for the lowest pre-experimental income category than for the highest category (above \$32,000). However, other differences among these groups for these variables are not significant, and the point estimate of the effect on disability applications is actually higher among those in the lowest category than those in the highest category. Thus, although there is some evidence that the effects we observe are weaker for those who start out earning less, it is far from conclusive.[22]

Regardless of the mechanism, the significant results on long-term outcomes may be surprising in the context of the original finding that there was no effect on earned income in the two years after the experiment ended, as shown in Figure 1b. Indeed, in many contexts—such as the Self Sufficiency Project studied by Card and Hyslop (2005)—the fact of no significant effect immediately after treatment ends is taken as evidence that there are no significant long-term effects. To help understand these results, Figure 3 plots the effect on earned income, and on (cumulative) disability applications, at each age from 35 to age 65 or 75.[23][24] As shown in Figure 3a, effects are strongest between approximately ages 50 and 60, which corresponds to the time when most people leave the labor force and retire. The difference between effects in the 50s and at younger ages occurs within

[22] Note that the financial treatment was, on average, more generous for those in higher pre-experimental income categories so that all families would receive an approximately similar benefit. Thus, when comparing effects across different income categories, we are holding approximately constant the expected benefit, not the absolute guarantee. However, as noted below, there is little significant difference in effect by guarantee level, so this distinction may not be especially important.

[23] We present results up to 65 for disability because almost no one applies for disability benefits after 65, when SSA retirement benefits generally replace disability benefits. We present results for earnings up to age 75, because few adults in our sample earn after that age. (Note that, in our main specification, we ignore observations of individuals who are over 60 years old in regressions on income, so some observations in the graph do not contribute to the main statistic.)

[24] Similar graphs for other main variables, as well as graphs looking at effects for given numbers of years after the experiment, are available in Figures C.1 and C.2. Annual averages for these variables are shown in Figures C.3 and C.4.

individual treated workers, and is significant: controlling for individual fixed effects, the effect on earned income is \$2,000 stronger between ages 50 and 60 than at ages younger than 50 (a difference that is significant at the 5% level). Figure 3b shows that many of the applications for disability benefits that are induced by the experiment occur between ages 50 and 60 as well. As noted above, treatment induced individuals to apply for—but not receive—disability benefits; thus we might think of disability applications mostly as a signal of wanting to leave the labor market. Taken together with the fact that only 12% of adults were 50 or over by the fifth year of the experiment, these results suggest that the reason there were not effects on earned income immediately after the experiment is that the effect is driven by individuals retiring earlier. We explore this idea further—that effects may be delayed due to a retirement motive—in Subsection 4.2 and Appendix B.

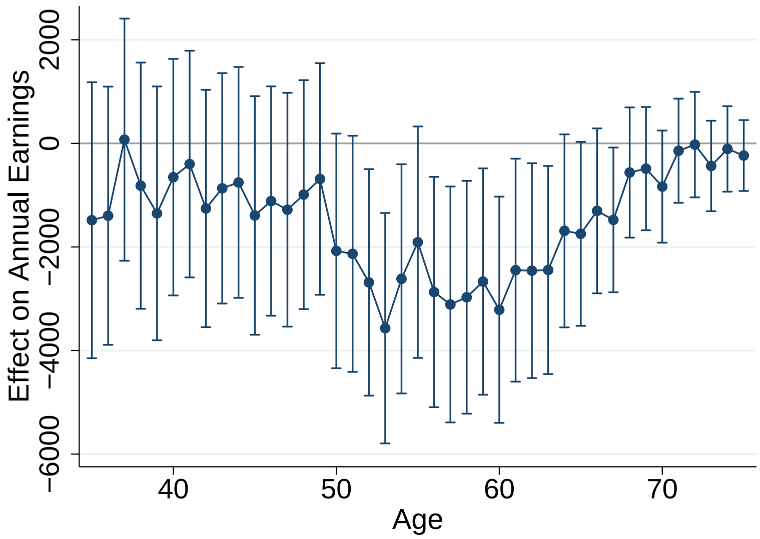
An important issue with these results concerns external validity: how would the results be different if the treatment had been different? For some variations on the treatment, we cannot know the answer. For example, SIME/DIME treated only a few thousand of the millions of people living in their metropolitan areas. If more had been treated, we might expect general equilibrium effects to occur, such as wages rising as individuals work less; these effects could have long-run consequences. Given the scale of this experiment, though, such an analysis is beyond the scope of this paper.

However, different SIME/DIME treated families did receive somewhat different treatments. Families' treatments could vary in the length of treatment (3 or 5 years); their guarantee level (the money received if no income is earned); the tax rate when \$0 is earned (either 50%, 70%, or 80%); whether that tax rate declined as more money is earned; and whether they were in the manpower treatment (discussed in Footnote 13). Table 6 shows how results vary when we include these variables in a regression. In general, few of these effects are statistically significant, suggesting that these results are somewhat generalizable. Most significantly, individuals on the longer 5-year treatment experienced a stronger effect on disability applications, indicating that a policy—which would likely be longer-lasting—might have stronger effects. A priori, we might also expect that the high tax rate, rather than the cash assistance itself, might have caused people to leave the workforce, which could lead to long-run effects. However, there was little evidence during the experiment that variations in the tax rate caused adults to work less. Consistent with that, we do not find that a higher tax rate is associated with stronger effects; if anything, point estimates lead to the opposite conclusion. Of course, much caution is needed in predicting the effect of, for example, a 0% tax rate or a permanent program because such predictions require extrapolation beyond the domain of treatments tested. This is particularly so here, given the large standard errors on the estimates in Table 6. Regardless, these results provide some suggestive evidence.

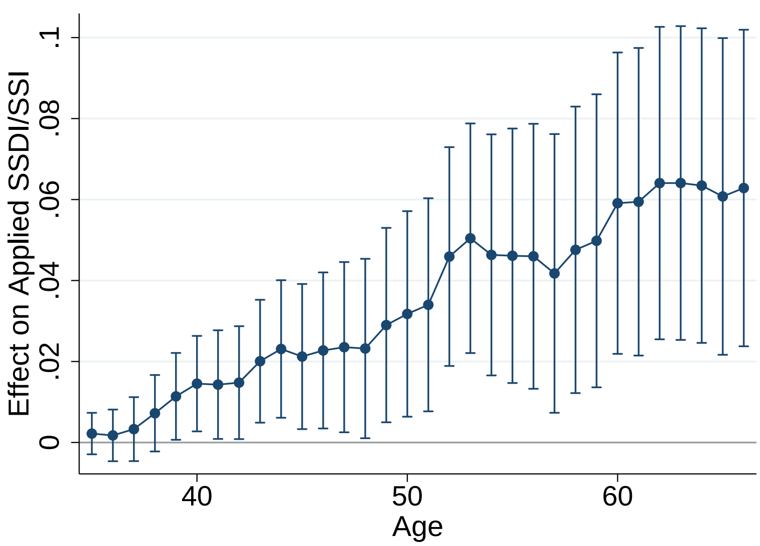
In addition to the five main variables discussed above, the richness of the SSA and WA DOH data allow us to study several other outcomes; see Table C.1. These results show that the effect on disability applications acts through both the SSDI and SSI programs. It also shows that we find no

Figure 3: Parents, effect on earned income and disability applications at different ages

(a) Earned income



(b) Disability applications



Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from individuals when they are a certain age. Confidence intervals are based on standard errors that are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event occurred by the time indicated.

Table 6: Parents, different treatments

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Treated	$-.0331^{**}$	-1825^{**}	$.0615^{***}$	.0182	.0125
	(.0137)	(818)	(.02)	(.019)	(.0197)
5-Year Trtmnt	$-.00882$	-52.5	$.0786^{***}$	$.0455^{*}$	.00453
	(.0187)	(1039)	(.0285)	(.0263)	(.0263)
Guar Level	$-4.45e-08$	.216	$2.70e-08$	$7.63e-06^{*}$	$4.09e-06$
	(2.97e-06)	(.157)	(4.51e-06)	(4.31e-06)	(4.01e-06)
Tax Rate, \$0	$-.0026$	1622	$-.276$	$-.364^{**}$	$-.106$
	(.109)	(6071)	(.168)	(.156)	(.157)
Tax Decline?	$-.0227$	-1594	$.0898^{**}$	.0508	.0206
	(.0251)	(1350)	(.0382)	(.0358)	(.0358)
Manpower	.01	-11.7	$-.00549$	$-.00064$	.0123
	(.0134)	(795)	(.02)	(.019)	(.0193)
Dep var summary stats					
Mean	.709	23748	.318	.247	.385
Std. Dev.	.454	25161	.466	.432	.487
N	52867	52867	2280	2280	2280
People	2252	2252	2280	2280	2280
Clusters	1699	1699	1720	1720	1720

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variables are variations on possible treatments. "5-Year Trtmnt" is an indicator for being in the treatment for 5 years, as opposed to 3 years. "Guar Level" is the guaranteed income the family received if there was no outside income. "Tax Rate, \$0" is the marginal tax rate on the first dollar of outside income during treatment. "Tax Decline?" is an indicator for whether the tax rate declines as the family gets more outside income. "5-Year Trtmnt," "Guar Level," "Tax Rate, \$0," and "Tax Decline?" variables are all demeaned, so the coefficient on treatment status is evaluated for the average type of financial treatment. "Manpower" is an indicator for being in the manpower treatment, which can include job counseling and educational subsidies. Each regression also includes a dummy variable for treatment status.

significant evidence of an effect on marriage or divorce. Finally, we see no effect on self-employment income, likely because the overall level of such income was so low; and that annual earned income generally declined by several different measures.

This table, along with several other additional tables and figures, is in Online Appendix C.

4.2 Mechanisms driving the effects on adults

As discussed above, treatment caused no significant effect on earnings immediately after cash assistance ended, but did cause significant effects much later in life, as adults approached retirement. In this subsection, we explore why these effects may have been delayed, and why they occurred at all.

First, the time pattern of the effects may be surprising, as economists sometimes take absence of effects immediately after a treatment ends as evidence that the treatment has no long-term effect. However, individuals generally consume much more leisure toward the end of their lives, with drops in earned income occurring soon before retirement; see, for example, Figures C.4a and C.4b, which show the fraction of people earning income, and the average income earned, for SIME/DIME adults at different ages. On average, then, older individuals may be closer to being at the margin of consuming more leisure than are younger individuals, because they can simply retire earlier. If this is true, any long-lasting shock should be expected to have strongest effects on the labor/leisure decision only when treated individuals are older. To formalize this idea, in Appendix B, we present a simple life cycle model where agents exogenously experience an increased disutility from labor as they age. In this model, agents who face a treatment similar to SIME/DIME save some of the assets they receive, and spend them on leisure later in life. Because agents exogenously prefer to work less as they grow older, that leisure is consumed more as they approach retirement age, as opposed to immediately after the treatment.

In our model in Appendix B, the long term effects are driven by accumulated wealth. Long-term effects could also arise through the wage rate: treatment caused individuals to leave work, which could depress long-run wages. A third set of potential mechanisms operate through the way that individuals perceive leisure and government benefits. With the data we have, we cannot conclusively say which, if any, of these mechanisms drive the results that we see. However, we discuss evidence for and against these potential mechanisms below.

First, treatment could increase total assets because treated families received large cash transfers. Those assets could then be spent on leisure after the experiment ended. However, the scale of the effect in the data is substantially larger than we might expect based on the asset shock alone. As discussed above, the long-term earnings response is 3 times larger than the initial shock to unearned income, and it is implausible that a shock to wealth would cause a large decrease in consumption of market goods. This is particularly true here because SIME/DIME includes individuals earning a wide range of incomes—between \$0 and \$51,000 per year—so the effect is unlikely to be explained by a constraint that a small amount of money can overcome. Indeed, Cesarini et al. (2015) analyze a pure wealth shock and find a lifetime marginal propensity to consume leisure out of unearned income of approximately 0.11 (though their estimate is based on an extrapolation of only 10 years of post-treatment data).

A few considerations may make it more plausible that the effect seen in SIME/DIME families is purely driven by wealth.[25] First, our estimate of the effect on initial wealth could be low if misreporting or attrition biased the original results to indicate a stronger effect on earned income during the experiment. As noted in Subsection 2.1, estimates of misreporting from Greenberg and Halsey (1983) indicate that the change in total household income may be substantially higher than that estimated from survey data. Additionally, we do not measure unearned income after the experiment ended. Although we see no effect on disability benefit awards, the fact that treatment increased applications suggests that there may have been effects on other benefits. Unfortunately, we have no data on post-experimental welfare (AFDC or TANF), food stamps, or other programs, but this other unearned income could help us understand how a positive income shock could lead to lower total observed income. However, other mechanisms may be necessary for explaining why this change in income source would have occurred.

A second potential channel for the effects on adults would be through wages. The time out of work could have decreased the human capital of the treated relative to control individuals because they would not be able to use the time for learning-by-doing, as described in Arrow (1962), Imai and Keane (2004), and others. Alternatively, time out of work could decrease inferred ability, as described by Gibbons and Katz (1991). In either case, the additional leisure time during the experiment would result in lower wages once the treatment was no longer in effect. Such an effect would be related to findings by Jacobson et al. (1993) and others that involuntary separations can lower earnings in the long term. In this case, however, the non-working time was *voluntarily* chosen, as treated individuals could have behaved similarly to control individuals.

Based on original data from the experiments, though, we see no significant effect on post-treatment wages. In fact, in the first two years after the experiment ended—the only years for which we have post-experimental hours data—the point estimate for the effect on the log wage rate is positive. (We cannot rule out, however, that treatment reduced hourly log wages by as much as .028 in the year after the treatment ended.) Furthermore, if there was an effect on wages, we might expect that earnings would be lower immediately in the SSA data (in addition to a possibly increasing effect as individuals age), as it would be harder for treated individuals to earn the same total earnings as control individuals. However, we see no significant evidence of this. Thus, the evidence we have does not support the idea that the long-term effects are driven by changes to the wage rate.

Finally, it is possible that the experiment changed the way treated individuals perceived leisure

[25] If the effect were driven by wealth, we might expect results to be similar if the tax rate were lower. As shown in Table 6, the effect does not vary significantly with tax rate, indicating that the wealth itself is important. On the other hand, it also does not vary with the guarantee level. In all, it is possible that those results are too imprecise to differentiate between hypotheses.

or government benefits. This could occur in a variety of ways. One example would be habit formation: individuals induced to work less find that they enjoy leisure more. The treatment could also reduce the stigma attached to receiving government benefits, or cause individuals to expect government benefits, because it induced many individuals to receive such benefits who would not have otherwise. We see that treated individuals are more likely to apply for disability benefits; this could be due, in part, to such a change in feelings about government benefits. Treatment could also cause individuals to become more efficient at home production; thus what we view as leisure time would actually be time engaged in a productive activity. For each of these mechanisms, as with the wealth channel, we may not observe effects immediately after the experiment because individuals at that time are not close to the margin of retirement. Unfortunately, although these mechanisms are plausible drivers of the effects we see, we do not have a robust measure of such preferences, perceptions, or abilities.

5 Outcomes for children

5.1 Results

Effects on four main outcomes of interest for children—two labor market outcomes, and two outcomes related to the disability system—are shown in Table 7. There are no significant effects on any of these outcomes. Based on the 95% confidence intervals, we can rule out treatment decreasing the average child's propensity to work in any given year by more than 1.5 percentage points, or increasing this propensity by more than 1.9 percentage points. We can rule out that treatment decreased annual earned income by more than $1,500, or increased it by more than $820. We can also rule out large effects on interactions with the disability system. The 95% confidence interval for the effect on disability applications runs from -1.9 to 3 percentage points, while the confidence interval for the effect on disability awards runs from -1.7 to 2.1 percentage points. These null results are robust to a variety of alternative specifications. As shown in Table C.5, point estimates are similar under alternative specifications, and under no alternative tested is any estimate significantly different from zero. Effects are also generally insignificantly different from zero on an annual basis, as shown in Figures C.5 and C.6. Additionally, as shown in Table 8, there are no effects on any of the intensive margins analyzed.

Estimated effects on a variety of other variables are shown in Table C.4. No effect on any of these variables is statistically significantly different from zero. We see no significant effect on applications for, or awards of, either SSDI or SSI. There was no significant effect on either marriage or divorce, and no effect on mortality (measured with either WA DOH data or SSA data). We also see no significant effect on self-employment income (which, as for adults, was quite low on average); and no significant effect on other moments of the earned income distribution.

The baseline null results are estimated with enough precision to meaningfully inform the literature on the intergenerational effects of cash assistance, as discussed below. Because we do find

Table 7: Children, effects on main outcomes

	(1)	(2)	(3)	(4)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
Treated	.00177	−356	.00537	.0018
	(.00872)	(601)	(.0125)	(.00962)
Dep var summary stats				
Mean	.769	22281	.245	.13
Std. Dev.	.422	24384	.43	.336
N	163340	163340	5658	5658
People	5658	5658	5658	5658
Clusters	2101	2101	2101	2101

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family.

Table 8: Children, intensive margins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Awarded SSDI/SSI**	**Cancer**	**Circul-atory Disorder**	**Musculo-skeletal Disorder**	**Mental Disorder**	**Other Impair-ment**
Condition	**Alive**	**Earn>0**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**
Treated	.00541	−492	−.000502	−.000955	.0133	.015	−.0079	.0114
	(.00815)	(570)	(.0295)	(.0116)	(.0167)	(.0281)	(.0291)	(.0287)
Dep var summary stats								
Mean	.79	28991	.511	.044	.0874	.352	.483	.557
Std. Dev.	.408	24064	.5	.205	.282	.478	.5	.497
N	158763	125530	1385	1385	1385	1385	1385	1385
People	5636	5556	1385	1385	1385	1385	1385	1385
Clusters	2101	2097	976	976	976	976	976	976

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Dependent variables in columns 4 to 8 are indicators for whether the individual ever applied for disability benefits on the basis of the listed impairment. Independent variable "treated" indicates whether the individual was in a treated family. Observations are only included if they fit the condition listed. "Alive" indicates that the individual is not listed as having died in SSA records by the given year; "Earn> 0" indicates that the individual earned positive income in the given year; and "Applied SSDI/SSI" indicates that the individual ever applied for disability benefits.

so many null effects, some caution is needed before digging deeper for effects on various subgroups of the population. When we analyze so many subgroups, we expect about 5% of effects to be significant at the 5% level. It is possible, however, that the overall null result masks important heterogeneity. Therefore, we consider possible heterogeneity below; significant results, though, should be taken as suggestive evidence of differences.

We first look at whether results vary by the family's type of treatment. Table 9 shows that children in the 5-year treatment have results more similar to their parents: they are significantly less likely to earn money in a given year and are more likely to apply for disability insurance. They are also more likely to be awarded disability insurance. This suggests that a longer-lasting program could have caused significant effects for children. Results for various subgroups of the population are shown in Table C.6. In general, few significant effects are found for any subgroup. One subgroup of particular interest, though, is the youngest children; many believe that early-life interventions may be most influential.[26] Results for children born during the experiment are shown in the row marked "Age ≤ 0." There is evidence that the treatment significantly reduced earnings for this group. However, we find no significant effect on other outcomes for this group.

5.2 Relationship to other literature

Overall, as noted above, we can rule out large effects of SIME/DIME cash assistance on children's later labor market outcomes or interaction with the disability program. This subsection explores how these results can inform other literature on the intergenerational effects of cash assistance.

Some research has found that parental receipt of benefits increases children's probability to receive benefits themselves. Gottschalk (1996) finds that if they had children themselves, daughters of mothers who received AFDC benefits were approximately 300 to 500% more likely to receive AFDC benefits themselves, and concludes that much of this correlation is causal. Similarly, Dahl et al. (2014) find that parental receipt of Norwegian disability insurance (DI) causes an approximately 200% increase in a child's later receipt of DI (compared to the mean rate of receipt); Dahl et al. (2014) are also able to rule out effects on another type of government assistance of more than 100%.

SIME/DIME treatment increased the probability that a family receives any government benefits by 22 percentage points.[27] Thus, inflating estimates and standard errors in Table 7 by 1/.22 as a rough estimate, we can rule out (at the 5% level) that receipt of such cash assistance increased SSDI or SSI applications by more than 56% (of the mean application rate), and rule out increases

[26] Research supporting the idea that early interventions are most useful include Cascio and Schanzenbach (2013), Cunha et al. (2006), Cunha et al. (2010), Duncan et al. (2010), and Kautz et al. (2014). However, as noted by Kautz et al. (2014), evidence is simply more scarce about later-life interventions. Indeed, others find the evidence for longer-lasting impacts of early-life interventions less compelling, including Anderson (2008), Hoxby (2013), and Puma et al. (2012).

[27] This includes any government benefits in all SIME/DIME records. Restricting the analysis to income-dependent cash assistance (that is, AFDC, unemployment benefits, and SIME/DIME payments) increases the effect to 34 percentage points; restricting to only AFDC or SIME/DIME payments increases the effect to 48 percentage points. These larger effects on receipt of benefits would imply even more tightly-estimated null effects on children.

Table 9: Children, different treatments

	(1)	(2)	(3)	(4)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
Treated	.00186	−339	.00522	.00221
	(.00874)	(601)	(.0125)	(.00965)
5-Year Trtmnt	$-.0339^{***}$	−1276	$.0392^{**}$	$.0349^{***}$
	(.0122)	(816)	(.0178)	(.013)
Guar Level	$-8.02e-08$	−.0767	$-4.03e-07$	$-1.70e-06$
	(1.87e-06)	(.124)	(2.72e-06)	(1.94e-06)
Tax Rate, $0	−.0167	−1244	−.0581	.0236
	(.0712)	(4801)	(.103)	(.0753)
Tax Decline?	.00456	368	$.0426^{*}$	.0277
	(.0162)	(1074)	(.023)	(.0172)
Manpower	$-.0147^{*}$	−710	.00537	−.00604
	(.00854)	(587)	(.0124)	(.00965)
Dep var summary stats				
Mean	.769	22281	.245	.13
Std. Dev.	.422	24384	.43	.336
N	163340	163340	5658	5658
People	5658	5658	5658	5658
Clusters	2101	2101	2101	2101

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variables are variations on possible treatments. "5-Year Trtmnt" is an indicator for being in the treatment for 5 years, as opposed to 3 years. "Guar Level" is the guaranteed income the family received if there was no outside income. "Tax Rate, $0" is the marginal tax rate on the first dollar of outside income during treatment. "Tax Decline?" is an indicator for whether the tax rate declines as the family gets more outside income. "5-Year Trtmnt," "Guar Level," "Tax Rate, $0," and "Tax Decline?" variables are all demeaned, so the coefficient on treatment status is evaluated for the average type of financial treatment. "Manpower" is an indicator for being in the manpower treatment, which can include job counseling and educational subsidies. Each regression also includes a dummy variable for treatment status.

in SSDI/SSI receipt of more than 73%. One possible reason that we do not find significant effects is mentioned by Dahl et al. (2014) in explaining the difference between their results on DI receipt and their results on other assistance: it may be that much of the intergenerational effect is due to transfers of information about particular government programs. Information about SIME/DIME would not be useful to a child in applying for SSDI or SSI, so the null effect we find may support the hypothesis that any intergenerational effects act more through information transfers than through effects on beliefs about working or the stigma associated with benefits. This is an important distinction: increasing information about benefits may be considered by some policymakers as a good outcome if it helps those who need them receive benefits, while reducing desire to work or the stigma associated with government benefits may be viewed more negatively by some.

On the other hand, Aizer et al. (2016) analyze the Mothers' Pension (MP) program, an early welfare program that had similar generosity and duration to the average payment from SIME/DIME treatment. They estimate that receipt of MP benefits increases a child's later income by at least 14%, whereas we can rule out treatment increasing earned income by more than 3.7%. This difference may occur because the SIME/DIME treatment discouraged parental work, whereas Aizer et al. (2016) view the MP program as an unconditional cash transfer; the potential for such a disincentive to affect children is discussed above. SIME/DIME also differed importantly from the MP program in that SIME/DIME families were not selected to be in the most need, and SIME/DIME control families could receive other benefits, such as AFDC. It is therefore possible that cash assistance can help children in families with the greatest need without affecting those in better-off families.

6 Discussion and conclusion

The Income Maintenance Experiments of the 1960s and 1970s were the first large-scale social science randomized controlled trials, offering a unique opportunity to identify the long-term causal effects of cash assistance. We use data from the IMEs in Seattle and Denver, along with data from the SSA and the WA DOH, to follow up, for the first time, on long-term outcomes for participants and their children from these experiments. Even after participants were no longer being directly treated, this cash assistance caused earned income for treated adults to be $1,800 lower per year, and increased the probability these adults would apply for disability benefits by 6.3 percentage points. On the other hand, there was no effect on adults' mortality rate or their propensity to receive these benefits.

We also find little evidence that any effect was passed on to their children. We can rule out effects in either direction on child applications for SSDI or SSI of more than 3 percentage points, and effects on child annual earned income of more than $1,500. While narrower confidence intervals would be desirable, these findings—in the context of other literature—can improve our understanding of how cash assistance affects children. Although the standard errors may mask smaller causal effects, these results provide evidence that in some cases, cash assistance may not have the large intergenerational effects found in other contexts.

Although we discuss potential reasons why our results may differ from results in other studies, more research is needed to understand the causal mechanisms involved in order to better apply these lessons to policy. For example, if the high tax rate were crucial for the effects we observe, then policymakers may favor an unconditional guarantee, or an EITC-style negative tax rate. Additionally, if the control group's access to AFDC and other benefits was the reason we didn't observe significant effects for children, then we should not generalize these results as indicating the effect of *any* cash assistance, but *additional* cash assistance. Understanding the causal mechanisms will improve external validity, but it could also allow for more welfare analysis of the effects. The long-term effects on adults were likely unintended consequences from the perspective of policymakers. But given the data we have, we cannot say if, or by how much, they represent a welfare gain or loss for the adults themselves, or their families.

Lack of evidence also makes it difficult to compare our significant results for adults to the long-term effects of other government programs and policies, such as food stamps or public housing. More research on the long-term effects of other programs can help put the present results in their proper context, and better inform policymakers of the long-term trade-offs they face. This research should take into account that long-term effects may exist even in the absence of measurable medium-term effects, and that long-term effects on adults can be important separately from effects on children. More broadly, researchers studying long-term effects should be aware that effects on a given outcome may be most visible when individuals are close to a margin for changing that outcome, which may not be immediately after a treatment.

Poverty and other aspects of low socioeconomic status often last a lifetime, and are passed down through the generations. Guaranteeing a minimum income above the poverty line ensures that a family is not in poverty while the guarantee is in place, but it alone may not be a panacea to break the cycle of poverty or reduce inequality in a sustainable way. Taken together, our results suggest that policymakers should consider the long-term impacts of cash assistance. In SIME/DIME, assistance does not cause large observable benefits for children, and may lead to unintended consequences for adults. On the other hand, we also find no evidence that this government assistance creates a welfare culture that is passed down to future generations. In a time of rising inequality, more research is needed to understand how cash assistance affects families that are struggling to get by.

References

Aizer, Anna, Shari Eli, Joseph Ferrie, and Adriana Lleras-Muney. The Long-Run Impact of Cash Transfers to Poor Families. *American Economic Review*, 106(4):935–971, 2016.

Anderson, Michael L. Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, 103(484):1481–1495, 2008.

Arrow, Kenneth J. The Economic Implications of Learning by Doing. *The Review of Economic Studies*, 29(3):155–173, 1962.

Autor, David H. and Mark G. Duggan. The Rise in the Disability Rolls and the Decline in Unemployment. *Quarterly Journal of Economics*, 118(1):157–206, 2003.

Black, Sandra E. and Paul J. Devereux. *Recent Developments in Intergenerational Mobility*, volume 4 of *Handbook of Labor Economics*, chapter 16, pages 1487–1541. 2011.

Bleakley, Hoyt and Joseph P. Ferrie. Shocking Behavior: Random Wealth in Antebellum Georgia and Human Capital Across Generations. Working Paper 19348, National Bureau of Economic Research, 2013.

Cain, Glen G. and Douglas A. Wissoker. A Reanalysis of Marital Stability in the Seattle-Denver Income-Maintenance Experiment. *American Journal of Sociology*, 95(5):1235–1269, March 1990a.

Cain, Glen G. and Douglas A. Wissoker. Response to Hannan and Tuma. *American Journal of Sociology*, 95(5):1299–1314, March 1990b.

Card, David and Dean R. Hyslop. Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers. *Econometrica*, 73(6):1723–1770, 2005.

Cascio, Elizabeth and Diane Whitmore Schanzenbach. The Impacts of Expanding Access to High-Quality Preschool Education. *Brookings Papers on Economic Activity*, 47(2):127–178, 2013.

Cesarini, David, Erik Lindqvist, Matthew J. Notowidigdo, and Robert Östling. The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries. Working Paper 21762, National Bureau of Economic Research, 2015.

Christophersen, Gary. *Administration*. Volume 2 of SRI International and Mathematica Policy Research (1983), 1983.

Cooper, Kerris and Kitty Stewart. Does Money Affect Children's Outcomes? A systematic review. Technical report, Joseph Rowntree Foundation, October 2013.

Cunha, Flavio, James J. Heckman, Lance Lochner, and Dimitriy V. Masterov. *Interpreting the Evidence on Life Cycle Skill Formation*, volume 1 of *Handbook of the Economics of Education*, chapter 12, pages 697–812. 2006.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach. Estimating the Technology of Cognitive and Noncognitive Skill Formation. *Econometrica*, 78(3):883–931, 2010.

Currie, Janet. Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development. *Journal of Economic Literature*, 47(1):87–122, 2009.

Dahl, Gordon B., Andreas Ravndal Kostøl, and Magne Mogstad. Family Welfare Cultures. *Quarterly Journal of Economics*, 129(4):1711–1752, 2014.

Davis, Margaret R. and Kenneth Kehrer. *Overview of Research on Health, Consumption, and Social Behavior*, chapter 6, pages 385–446. Volume 1 of SRI International and Mathematica Policy Research (1983), 1983.

Department of Health, Education, and Welfare. Income Maintenance Experiment Files, 1968-1978. Machine-readable data file, 1978. Distributed by the National Archives, Identifier 629379.

Duncan, Greg J., Jens Ludwig, and Katherine A. Magnuson. *Child Development*, chapter 2, pages 27–58. 2010.

Feenberg, Daniel. TAXSIM Related Files at the NBER. http://www.nber.org/taxsim/, 2016. Accessed: 2016-11-18.

French, Eric and John Bailey Jones. The Effects of Health Insurance and Self-Insurance on Retirement Behavior. *Econometrica*, 79(3):693–732, 2011.

Gibbons, Robert and Lawrence F. Katz. Layoffs and Lemons. *Journal of Labor Economics*, 9(4): 351–380, 1991.

Gottschalk, Peter. Is The Correlation In Welfare Participation Across Generations Spurious? *Journal of Public Economics*, 63(1):1–25, 1996.

Greenberg, David and Harlan Halsey. Systematic Misreporting and Effects of Income Maintenance Experiments on Work Effort: Evidence from the Seattle-Denver Experiment. *Journal of Labor Economics, University of Chicago Press*, 1(4):380–407, October 1983.

Greenberg, David and Mark Shroder. *The Digest of Social Experiments*. The Urban Institute Press, Washington, DC, third edition, 2004.

Groeneveld, Lyle P., Michael T. Hannan, and Nancy Brandon Tuma. *Marital Stability*, chapter 5, pages 257–383. Volume 1 of SRI International and Mathematica Policy Research (1983), 1983.

Gustman, Alan L. and Thomas L. Steinmeier. A Structural Retirement Model. *Econometrica*, 54 (3):555–584, 1986.

Halsey, Harlan I. AFDC, Food Stamp, and Public Housing Taxes in Seattle and Denver in 1970-71. Research Memorandum 52, Stanford Research Institute, March 1978.

Hannan, Michael T. and Nancy Brandon Tuma. A Reassessment of the Effect of Income Maintenance on Marital Dissolution in the Seattle-Denver Experiment. *American Journal of Sociology*, 95(5):1270–1298, March 1990.

Hanushek, Eric A. *Non-Labor-Supply Responses to the Income Maintenance Experiments*, pages 106–121. In Munnell (1986), 1986.

Hausman, Jerry A. and David A. Wise. Attrition Bias in Experimental and Panel Data: The Gary Income Maintenance Experiment. *Econometrica*, 47(2):455–473, 1979.

Hill, Mark E. and Ira Rosenwaike. The Social Security Administrations Death Master File: The Completeness of Death Reporting at Older Ages. *Social Security Bulletin*, 64(1):45–51, 2001.

Hoxby, Caroline M. Comments and Discussion: The Impacts of Expanding Access to High-Quality Preschool Education. *Brookings Papers on Economic Activity*, 47(2):179–184, 2013.

Hoynes, Hilary W., Diane Whitmore Schanzenbach, and Douglas Almond. Long Run Impacts of Childhood Access to the Safety Net. *American Economic Review*, 106(4):903–934, 2016.

Hutchens, Robert M. Changes in AFDC Tax Rates, 1967–1971. *Journal of Human Resources*, 13 (1):60–74, 1978.

Imai, Susumu and Michael P. Keane. Intertemporal Labor Supply and Human Capital Accumulation. *International Economic Review*, 45(2):601–641, 2004.

Imbens, Guido W., Donald B. Rubin, and Bruce I. Sacerdote. Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players. *American Economic Review*, 91(4):778–794, 2001.

Jacobson, Louis, Robert LaLonde, and Daniel Sullivan. Earnings Losses of Displaced Workers. *American Economic Review*, 83(4):685–709, 1993.

Kautz, Tim, James J. Heckman, Ron Diris, Bas ter Weel, and Lex Borghans. Fostering and Measuring Skills: Improving Cognitive and Non-Cognitive Skills to Promote Lifetime Success. Working Paper 20749, National Bureau of Economic Research, 2014.

Keeley, Michael C. and Philip K. Robins. Experimental Design, the Conlisk-Watts Assignment Model, and the Proper Estimation of Behavioral Response. *The Journal of Human Resources*, 15(4):480–498, 1980.

Mathematica Policy Research, Inc. Denver income maintenance experiment, 1970-1975: 16th monthly composite principal person file. Machine-readable data file, 2000a. Distributed by Madison, WI: University of Wisconsin, Data and Program Library Service.

Mathematica Policy Research, Inc. Seattle income maintenance experiment, 1970-1975: 16th monthly composite principal person file. Machine-readable data file, 2000b. Distributed by Madison, WI: University of Wisconsin, Data and Program Library Service.

Mayer, Susan E. *What Money Can't Buy: Family Income and Children's Life Chances*. Harvard University Press, Cambridge, MA, 1997.

Moffitt, Robert A. Cumulative Effective Tax Rates and Guarantees in Low-Income Transfer Programs. *Journal of Human Resources*, 14(1):122–129, 1979.

Munnell, Alicia H., editor. *Lessons from the Income Maintenance Experiments*. Federal Reserve Bank of Boston and The Brookings Institution, 1986.

Murray, Charles. *Losing Ground: American Social Policy, 1950-1980*. Basic Books, Inc., Publishers, New York, NY, 1984.

Office of Income Security Policy, Office of the Assistant Secretary for Planning and Evaluation, and U.S. Department of Health and Human Services. Overview of the Final Report of the Seattle-Denver Income Maintenance Experiment, 1983.

Oreopoulos, Philip, Marianne Page, and Ann Huff Stevens. The Intergenerational Effects of Worker Displacement. *Journal of Labor Economics*, 26(3):455–483, 2008.

Pencavel, John H. Income Underreporting and Labor Supply in the Seattle-Denver Income Maintenance Experiments. Draft, May 1979.

Pepper, John V. The Intergenerational Transmission of Welfare Receipt: A Nonparametric Bounds Analysis. *Review of Economics and Statistics*, 82(3):472–488, 2000.

Piketty, Thomas. *Capital in the Twenty-First Century*. Harvard University Press, 2013.

Proctor, Bernadette D., Jessica L. Semega, and Melissa A. Kollar. Income and Poverty in the United States: 2015. Current Population Reports P60-256(RV), United States Census Bureau, 2016.

Puckett, Carolyn. The Story of the Social Security Number. *Social Security Bulletin*, 69(2):55–74, 2009.

Puma, Mike, Stephen Bell, Ronna Cook, Camilla Heid, Pam Broene, Frank Jenkins, Andrew Mashburn, and Jason Downer. Third Grade Follow-up to the Head Start Impact Study Final Report. OPRE Report 2012-45, Office of Planning, Research and Evaluation, Administration for Children and Families, U.S. Department of Health and Human Services, 2012.

Robins, Philip K. The Labor Supply Response of Twenty-Year Families in the Denver Income Maintenance Experiment. *Review of Economics and Statistics*, 66(3):491–195, 1984.

Robins, Philip K. and Richard W. West. Sample Attrition and Labor Supply Response in Experimental Panel Data: A Study of Alternative Correction Procedures. *Journal of Business & Economic Statistics*, 4(3):329–338, 1986.

Schmieder, Johannes F., Till Von Wachter, and Stefan Bender. The Long-Term Effects of UI Extensions on Employment. *American Economic Review: Papers and Proceedings*, 102(3):514–519, 2012.

Schrock, Jason. History of Colorado Income Tax Rates. Memorandum, Colorado Legislative Council Staff, 2010.

Shea, John. Does Parents' Money Matter. *Journal of Public Economics*, 77:155–184, 2000.

Social Security Administration. Social Security & Medicare Tax Rates. https://www.ssa.gov/oact/progdata/taxRates.html, 2016. Accessed: 2016-11-18.

Solon, Gary. What Do We Know So Far about Multigenerational Mobility? Working Paper 21053, National Bureau of Economic Research, 2015.

Spiegelman, Robert G. *History and Design*, chapter 1, pages 1–51. Volume 1 of SRI International and Mathematica Policy Research (1983), 1983.

SRI International and Mathematica Policy Research, editors. *Final Report of the Seattle-Denver Income Maintenance Experiment*. U.S. Government Printing Office, 1983.

Tax Foundation. U.S. Federal Individual Income Tax Rates History. http://taxfoundation.org/sites/default/files/docs/fed_individual_rate_history_nominal.pdf, 2013. Accessed: 2016-11-18.

Widerquist, Karl. A failure to communicate: what (if anything) can we learn from the negative income tax experiments? *Journal of Behavioral and Experimental Economics (formerly The Journal of Socio-Economics)*, 34(1):49–81, 2005.

Wilde, Elizabeth T., Zohn Rosen, Kenneth Couch, and Peter A. Muennig. Impact of Welfare Reform on Mortality: An Evaluation of the Connecticut Jobs First Program, A Randomized Controlled Trial. *American Journal of Public Health*, 104(3):534–538, 2014.

World Bank Group. Taking On Inequality. Poverty and shared prosperity, 2016.

A Creating the sample of SIME/DIME participants

A.1 Finding families with matching patterns

To the best of our knowledge, no traditional identifying information such as names or Social Security numbers (SSNs) exists from SIME/DIME or the other IMEs. However, public-use SIME/DIME data includes date of birth, sex, and relationship to household head for each family member in the experiment. From this data, we can find patterns that identify families. For example, suppose a Seattle single mother born January 1, 1930 has three male children born on February 1 of 1960, 1961, and 1962. It is unlikely that another family has exactly the same birth pattern.

To find families matching SIME/DIME patterns, we use public-record birth data from the WA DOH and restricted-use data on parent names from the SSA's Numident file. For each child-parent pair, we determine a set of "possible matches:" individuals from the birth records with individual characteristics matching those of SIME/DIME children.[28] So, to continue our example from above, we would look in WA DOH birth records for all boys born February 1, 1960 to 30-year-old mothers; boys born February 1, 1961 to 31-year-old mothers; and boys born February 1, 1961 to 32-year-old mothers. Then, for each head of household, we look for parent names that are common to possible matches for multiple children. In our example, then, one boy from each of the three lists of possible matches may have a mother named Jane Smith. In this case, we provisionally assume that Jane Smith is the SIME/DIME mother, and the three boys found in this way are her three children. Where necessary, we then match these individuals to SSNs.[29]

This method is not the only possible method of finding SIME/DIME families. It does have an important drawback: we cannot match anyone in a family with fewer than two children.[30] Crucially, though, as discussed in Subsection A.2, this method allows us to estimate the probability that each match corresponds to an actual SIME/DIME family, thus minimizing spurious matches.

A.2 Determining which matching families were in SIME/DIME

Not everyone provisionally matched using the procedure discussed in Subsection A.1 will be actual participants in SIME/DIME. In addition to actual SIME/DIME families, we may find other individuals through two channels: actual families with similar characteristics, and unrelated individuals with matching birthdays whose parents share a name. To determine which families are actually SIME/DIME families, we create a model of the matching procedure and estimate its parameters

[28] For SSA data, that includes all individuals with the same birthday and sex, who were born in Washington or Colorado (for the Seattle or Denver samples, respectively). For the WA DOH data, used only for the Seattle sample, we also restrict the sample to those whose parent is of the correct age (parent age is not available in SSA data).

[29] Parent and child matches from the WA DOH are matched to SSNs on the basis of first name, last name, and state of birth from WA DOH records; and birthday from SIME/DIME records. Child matches from the SSA are already attached to SSNs, so no additional match is necessary. Parent matches from the SSA are matched to SSNs on the basis of first name and last name from SSA records; and date of birth from SIME/DIME records.

[30] Individuals in families with zero or one children are dropped from all analyses in this paper (unless otherwise noted), including those that analyze the match rate.

via maximum likelihood estimation (MLE). Note that this model does not capture all features of the match process; however, as discussed in Subsection A.3, it succeeds well enough to be useful for the purposes of this analysis.

Suppose that, for each family in the SIME/DIME data, there is a τ chance that their records in the SSA Numident and WA DOH data matches the pattern sought, where τ is a constant: in particular, it does not depend on name frequency. (We do not assume that $\tau = 1$; any typos in SIME/DIME or SSA/WA DOH data, or children born in a different state, would cause a family not to be matched.) Next, suppose that, in expectation, there are $\alpha\tau$ other families for whom the other family's pattern of births matches the pattern of the IME family (where α may depend on data set used, but is assumed not to depend on name frequency).

Now, consider unrelated individuals with matching birthdays whose parents share a name. Suppose the match is based on N children, and each child i is found within n_i possible matches, where a "possible match" is defined as an observation where all variables match (as described in Footnote 28). For a specific parent name that occurs with frequency f,[31] we would expect that name to appear $n_i f$ times. Assuming independence, the expected number of matches with that name would be approximately $\prod_i(n_i f) = f^N \prod_i(n_i)$. Thus from the true match probability and the two spurious match channels, for a specific name with frequency f, we expect to find $\tau f + \alpha\tau f + f^N \prod_i(n_i)$ matches with N individuals in the family.

We can also run a placebo test: add t days to everyone's birthday, and rerun the algorithms; then add $t+1$ days and rerun it; and so on, T times.[32] (In our analysis, we set $T = 50$; based on results from the cross-validation procedure discussed below, this number is enough to accurately estimate parameters.) For T placebos, we expect to find $T\left(\alpha\tau f + f^N \prod_i(n_i)\right)$ matches. Thus putting placebos together with matches using the true birthdays, the fraction found using the true birthday would be

$$\text{Frac Sample} = \frac{\tau f + \alpha\tau f + f^N \prod_i(n_i)}{\tau f + \alpha\tau f + f^N \prod_i(n_i) + T\left(\alpha\tau f + f^N \prod_i(n_i)\right)} = \frac{1 + \alpha + \beta f^{N-1}}{1 + (T+1)(\alpha + \beta f^{N-1})}, \quad (3)$$

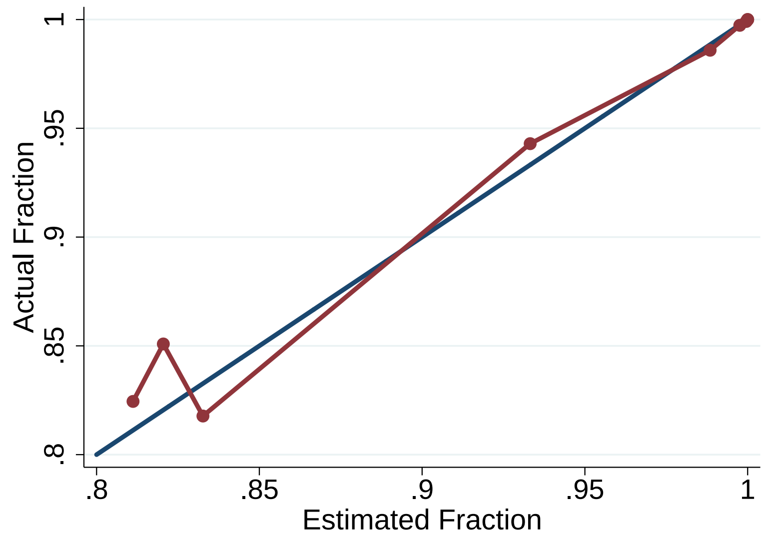
where $\beta \equiv \frac{\prod_i(n_i)}{\tau}$. We can now estimate α and β with MLE using one observation for all matched people (with placebo birthdays or with real birthdays), and set an indicator variable to 1 if the observation is from a real birthday, and 0 if it is from a placebo birthday.[33] From this we calculate

[31] Name frequency is calculated based on the frequency in SSA Numident data of the first name, times frequency of the last name, among the names being searched: parents of all people born in the same decade as the child being considered, or individuals born in the same decade as the adult being considered. It is calculated as the maximum of this frequency in the US as a whole, or in the state of interest.

[32] One potentially important complication is that all the children may be correctly matched, but are associated with a common parent name. Other individuals with the same name and birthday as the parent would then be spuriously matched, but would not be matched in the placebo test. To correct for this, we use additional placebos for parents based on name and true child birthdays, but placebo parent birthdays. (This is much less of a concern for children or parents in the WA DOH data, where we know state of birth; name, birthday, and state of birth are nearly a unique identifier even for those with common names.)

[33] Because the parameters could be different for different types of matches, the maximum likelihood procedure is run separately, and parameters are calculated independently, for each site; for each data set (SSA and WA DOH birth

Figure A.1: Cross-validation of MLE predictions



Notes: Families are randomized into two groups; MLE parameters are estimated with one group and probability of being in the non-placebo sample (i.e., matched with correct birthday rather than birthday plus an offset) is assigned to the other group based on these parameters. There may be multiple observations per person if one person is matched with multiple strategies (for example, using data from both the father and mother). Only observations with at least 75% chance of being from SIME/DIME are included. Observations are placed into deciles by probability of being in the sample; within each decile, average estimated probability of being in sample and fraction actually in sample are recorded. In this sample, the coefficient (and standard error) in a regression of actual fraction on estimated fraction is 0.920 (0.053).

the probability that each match is a SIME/DIME family as

$$\text{Prob SIME/DIME} = \frac{1}{1 + \alpha + \beta f^{N-1}} \tag{4}$$

One potential problem with this procedure is that, if a match is found with real birthdays, that match changes our estimates of the model, increasing the probability we will call the match a true SIME/DIME family. This is a particular problem if the number of placebo days is too low. To test whether parameters estimated by our model perform well out of sample, we ran a cross-validation procedure: we estimated α and β with a randomly-chosen half of the families, and then compared the estimated probability that each match from the other sample was found with the true birthdays, as opposed to placebo birthdays. As shown in Figure A.1, this procedure predicts probabilities out of sample well.

records); for parents, children for whom we matched a parent, and children for whom we did not match a parent; for each number of children matched; and by whether a found parent's birth state corresponds to the child's birth state.

Table A.1: Parents, effect on match rate

	(1)	(2)
Sample	**Parents**	**Children**
Treated	−.00125	−.00623
	(.0158)	(.0167)
Dep var summary stats		
Mean	.45	.589
Std. Dev.	.498	.492
N	5185	9676
People	5185	9676
Clusters	3400	3345

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Independent variable "treated" indicates whether the individual was in a treated family. The dependent variable is an indicator for the individual being matched to an SSN with at least 95% certainty. There is one observation per child or parent in any IME family with at least two children. Results are shown separately for children and parents.

A.3 Matching results

In our main sample, we only include matches if we calculate that they are true SIME/DIME participants with at least 95% probability. (We also show robustness checks with 75% and 99% probability thresholds.) With this threshold, 45% of parents and 59% of children in SIME/DIME are matched to a Social Security number (SSN).[34] Importantly, as shown in Table A.1, treatment is not significantly correlated with match probability, after controlling for assignment group. Tables C.3 and C.6 show that treatment is also not correlated with match probability within various subgroups of the population. Indeed, it is unlikely that treatment would have much effect on match probability, which is based on administrative records. However, we do discuss this possibility in Subsection A.4.

Many of the effects found in this paper are not statistically significantly different from zero. If our match procedure spuriously matches some SIME/DIME individuals to other, random individuals, this could attenuate results toward zero, which could lead to the null results we find. Two pieces of evidence convince us that there is little attenuation. First, only 1.1% of adults and .45% of children are matched to multiple SSNs.[35] We can use a simple model to show that this duplicate

[34] We may fail to match a SIME/DIME family for several reasons. Any typo or mistake in either SIME/DIME data or SSA data will greatly decrease the chance of a match. Any child born outside of Washington or Colorado will not be found. Finally, children born to smaller families with more common last names will not be found, because they cannot be reliably matched.

[35] Actually, the numbers reported here are the average count of the number of SSNs matched to each IME participant, minus one, where those who are not matched to any SSN have a value of zero. If the number is small, as it is here, it will be quite close to the fraction of duplicates. However, this number is preferable as it is sensitive to

rate suggests a very low rate of spurious matches. Consider a simple model where a fraction X of SIME/DIME individuals are matched to their actual SSN, while a fraction Y of SIME/DIME individuals match to a different SSN. Define the fraction of matched participants who are matched incorrectly as C; this is our quantity of interest. Further define the fraction of the population matched to multiple SSNs as D; this is the duplicate rate reported above. Assuming that $Y \ll X$ and that the sample size is large, we can approximate $C \approx Y/X$ and $D \approx XY$, so $C \approx D/X^2$. With the same assumptions, we can approximate X as our match rate, so that the fraction of spurious matches is just the duplicate rate divided by the match rate squared. From this, we estimate that only 5.2% of matched adults and 1.3% of matched children are matched to an incorrect SSN. That fraction for parents is comparable to a match to SSA data based on name and birthday alone, and for children it is better than such a match. (In our regression analyses, we exclude any duplicate matches.)

The high match rate, and the low spurious match rate, are dependent on the MLE procedure outlined above. If, for example, we include in our sample any individual who is found on the basis of at least three child matches in their family (as in the running example above), we match only 32% of parents (of whom 37% are matched spuriously) and 43% of children (of whom 13% are matched spuriously). This lower sample size, and greater noise, would make any inference more difficult, particularly for adults.

We can also test the quality of matches by comparing data from the SSA and SIME/DIME that was not used in the match. Unfortunately, there are no variables common to the two data sets. However, we do know an individual's race from SIME/DIME data; and name from SSA data, which is strongly correlated with race.

Suppose that, within each race R, participants are drawn randomly from the general population. That would mean that, for any name N, the probability $\mathbf{P}(\text{name} = N|\text{race} = R)$ that a SIME/DIME participant of race R has name N is the same as in the general population. For common names, $\mathbf{P}(N|R)$ is available from the Census,[36] so we can match that with the names of participants we find using the SSA Numident file. Using Bayes' Theorem,

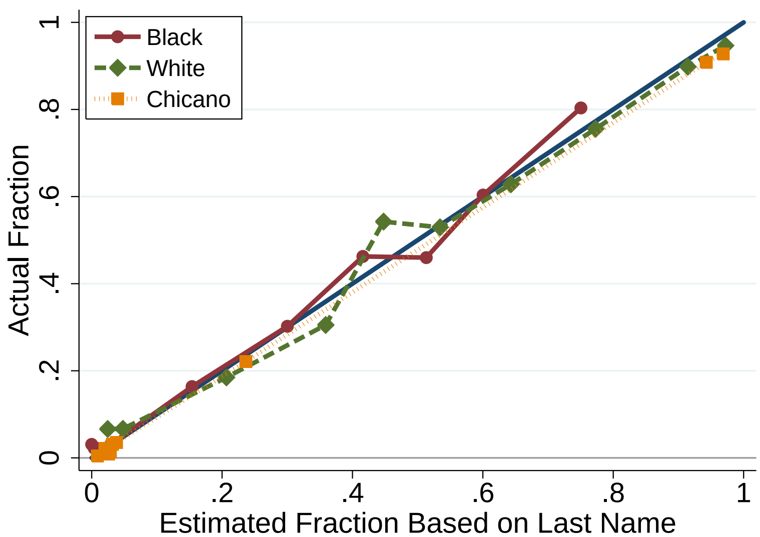
$$\mathbf{P}(R|N) = \frac{\mathbf{P}(N|R)\mathbf{P}(R)}{\sum_r \mathbf{P}(N|\text{race} = r)\mathbf{P}(\text{race} = r)}, \tag{5}$$

where $\mathbf{P}(\text{race} = r)$ is the fraction of people in the experiment who are of race r. We can then compare this estimated probability to the true fraction of people, for a given probability, who are of race R. Although the assumptions underlying Equation 5 may not hold perfectly, the estimated probability does predict actual race quite well; see Figure A.2.

situations where one IME participant is matched to more than two SSNs.

[36]See `www2.census.gov/topics/genealogy/2000surnames/names.zip`.

Figure A.2: Correspondence between SSA and MLE data



Notes: Estimated fractions are based on the assumption that, within each race R, participants are drawn randomly from the general population. Based on that assumption, for an individual with name N, the estimated probability of being of a given race R is $\mathbf{P}(R|N) = \frac{\mathbf{P}(N|R)\mathbf{P}(R)}{\sum_r \mathbf{P}(N|\text{race}=r)\mathbf{P}(\text{race}=r)}$, where, for any name n and race r, $\mathbf{P}(n|r)$ is based on Census 2000 data on last names and race (black, white, or Hispanic), while $\mathbf{P}(r)$ is based on racial composition of the matched SIME/DIME sample (black, white, or Chicano). Only adults are considered. Coefficients (and standard errors) in a regression of actual fraction against estimated fraction are 1.000 (0.028) for black adults, 0.960 (0.019) for white adults, and 0.960 (0.015) for Chicano adults.

A.4 Differential attrition

As discussed above, there is no evidence that treatment changed the probability that an individual would be found, on average or for various subgroups of the population. It is nevertheless possible that there is such an effect. That is possible if the effect is smaller than the confidence intervals we measure; or if there is heterogeneity in the effect of treatment on matching along dimensions that are not measured here.

There are three basic reasons that treatment could affect the probability of a match: fertility, mobility, and SSN applications. First, treatment could affect the probability that a new child appears in the SIME/DIME data, either through changes in actual fertility or because treated and control families have different incentives to report births. Based on original SIME/DIME data on all original household heads (including those with 0 children or 1 child), there is no significant evidence of an effect on number of children born during the experiment. Regardless, if treatment caused a birth to appear (or not appear) in SIME/DIME data, then that would make it more (or less) likely that our matching procedure finds the other members of that family. The second confounding factor is mobility. Davis and Kehrer (1983) note some evidence that treatment increases mobility. If treatment caused a family to move out of their original state, our matching procedure will not find any children born after the move, thus making it less likely to find the entire family.[37] A third confounding factor is SSN applications. Although each participating adult needed an SSN, it is possible that the treatment could affect changes to SSA records, such as name changes. Additionally, many participating children likely did not have SSNs when the experiment began, particularly because all children studied were under 18 at that time and SSN enumeration at birth did not begin until about twenty years later. Thus treatment could have affected the probability that a child would apply for an SSN. Although there is no evidence for such an effect, if it did occur, it could change the probability that a match would be made.

To account for most of these possibilities, we can rerun the match algorithm in two ways. First, we can leave out all records of children born after the experiment began; see Tables C.2 and C.5, rows denoted "No Post-Exp Births." There is no possibility for differential fertility or mobility to affect results based on these matches. To additionally remove any possibility of treatment affecting matches through parent SSA records, rows denoted "No Post-Exp Births Or Parent Recs" also leave out from the match procedure all parent SSA records collected after the experiment began. These restrictions reduce the sample size and thus increase standard errors of the estimates. They also change the population that it is possible to match, so the underlying parameters might be different for this group. However, the estimates are very similar under either restriction.

We do not have a robustness check excluding SSA data on children collected after the experiment began because excluding this data allows us to match only about 1% of participants. However, it is unlikely that this channel will affect our results because almost all children likely got an SSN at

[37] We also will not find a child if they are born in a different state before the experiment. However, this is not a confounding factor as treatment cannot affect it.

some point in their lives. SSNs are required for almost any legitimate job, as well as participation in any SSA benefit program. Additionally, as noted by Puckett (2009), SSNs have increasingly been required for many other government and private services, from food stamps and welfare to bank accounts and student loans. Thus it is unlikely that a substantial fraction of the SIME/DIME child population would have avoided getting an SSN.

B Model of delayed adult outcomes

B.1 Setup

As discussed in the main text, the negative income tax treatment in the Seattle-Denver Income Maintenance Experiment induced treated individuals to work less during the experiment; led to no statistically significant effect immediately after the experiment; and caused treated individuals to work less, and earn less money, later in life, as they approached retirement age. This may be a surprising result, as it is sometimes assumed that effects of an intervention tend to fade over time. However, any effect on an individual's action is likely to be strongest when that individual is closest to the margin for taking that action; and individuals may be closest to the margin of consuming more leisure as they approach retirement age. Thus if the treatment increased individuals' propensity to consume leisure in a lasting way, that increased propensity may be most measurable as the individual approaches retirement age. The simple model developed in this appendix formalizes that idea as an existence proof. It shows that, in a calibrated model with a retirement motive, measurable effects of a shock can be strongest later in life, as an agent approaches retirement.

In our model, rational, forward-looking agents (with preference parameters β, γ, η_0, and η_1) maximize their expected utility, a time-separable function of their consumption of market goods (measured by C_τ) and whether they work (indicated by L_τ) at any age τ between a starting age t and a final age of T (which we set to 70):

$$E_t \sum_{\tau=t}^{T} \beta^\tau \left[\frac{C_\tau^{1-\gamma}}{1-\gamma} - e^{\eta_0+\eta_1(\tau-35)} L_\tau \right]. \tag{6}$$

The increasing preference for leisure, parameterized by η_1, will induce agents to retire; in modeling retirement as being induced to some extent by an increasing preference for leisure, we follow French and Jones (2011), Gustman and Steinmeier (1986), and others. We model labor supply as a dichotomous variable: agents either work full time or do not work in a given year. This is motivated by the fact that most of the earnings effects we see are on the extensive margin. If they choose to participate in the labor market (indicated by $P_\tau = 1$), agents are only able to find work with probability f:

$$L_\tau = \begin{cases} P_\tau & \text{with probability } f \\ 0 & \text{with probability } (1-f) \end{cases} \tag{7}$$

Thus, at each τ, agents choose participation $P_\tau \in \{0, 1\}$, and any level of consumption $C_\tau > 0$.

Agents are subject to an intertemporal budget constraint on A_τ, the agent's asset holdings at age τ:

$$A_{t+1} = (1+r)A_t + Y \times L_t - T(Y \times L_t) - C_t. \tag{8}$$

If an agent does work, they earn income Y, which is constant over time, but varies across agents. Agents can borrow and lend costlessly at interest rate r, subject to the constraint that they are not in debt after the final period: $A_{T+1} \geq 0$. Agents also face a safety net and taxes, parameterized by $T(\cdot)$.

B.2 Simulation and Calibration

We solve the model for policy functions with backward induction, beginning with the final period at age 70. To do this, we pre-set parameters for which we do not have sufficiently detailed data to estimate. We set $r = .03$, with $\beta = \frac{1}{1+r}$. We also set $\gamma = 2$.

The transfer system is modeled after that in place for SIME/DIME participants. After the experiment, and during the experiment for control agents,

$$T(y) = \bar{T}_c - 0.3 \times y. \tag{9}$$

This tax and transfer scheme is based on benefits and incentives faced by families who do not receive the experimental treatment. $\bar{T}_c$ is set to \$14,000, which is the average of government benefits received in the pre-experimental period for those who earn between \$0 and \$1,000 in that year. The effective tax rate of 30% is chosen to approximate that faced by both those receiving benefits and those paying positive taxes. During treatment, treated agents face transfers of

$$T(y) = \max\left\{\bar{T}_c - 0.3 \times y\ ,\ \bar{T}_t - 0.6 \times y\right\}, \tag{10}$$

which is modeled on the SIME/DIME treatment. $\bar{T}_t$ set to vary based on an agent's earnings, recognizing that the SIME/DIME payment was higher, on average, for those deemed to have higher earnings potentials. In particular, those with the population's median value of unconditional earnings Y face $\bar{T}_t$ =\$22,200, the middle guarantee level; and $\bar{T}_t$ rises by \$.11 for every increase in Y of \$1, based on a regression of the guarantee level with normal income levels. The effective tax rate of 60% is chosen as representative of the taxes faced in treatment for those receiving SIME/DIME benefits.

Initial wealth is distributed normally, with mean \$18,000 and standard deviation \$36,000—both values taken from moments of the pre-experimental asset distribution. We parameterize Y—the distribution of earnings were agents to work, unconditional on whether they actually worked—as a log-normal distribution, $\ln \mathcal{N}(\mu_u, \sigma_u^2)$, truncated at \$100,000, with unobserved population parameters μ_u and σ_u estimated as described below.

We estimate five parameters of this model by calibrating moments from the model to moments observed in the data. We calibrate the fraction f of individuals who do not work; preference parameters η_0 and η_1; and parameters of the earnings distribution μ_u and σ_u. These parameters are calibrated to minimize the squared distance between the observed mean and standard deviation of log annual earnings for those who work (top-coded at \$100,000); and the fraction who work at each age between 35 and 70. For calibration, we model individuals starting at age 35, the average age of adults when treatment ends. In testing the effect of the treatment, we allow the treatment

Table B.1: Calibrated parameters

Parameter	Description	Value
f	Probability of finding a job	.9447
η_0	Initial labor disutility	-12.67
η_1	Annual change in labor disutility	.05551
μ_u	Mean of truncated uncond. earnings	13.16
σ_u	Std. dev. of truncated uncond. earnings	3.988

Notes: Parameters are calibrated to match modeled to actual values of mean and standard deviation of log annual earnings (top-coded at \$100,000); and the fraction who work at each age between 35 and 70. μ_u and σ_u are parameters of the distribution of earnings that agents would receive if they worked full time in a given year, unconditional on whether they actually do work. This unconditional earnings distribution is a log-normal distribution $\ln \mathcal{N}(\mu_u, \sigma_u^2)$ truncated at \$100,000.

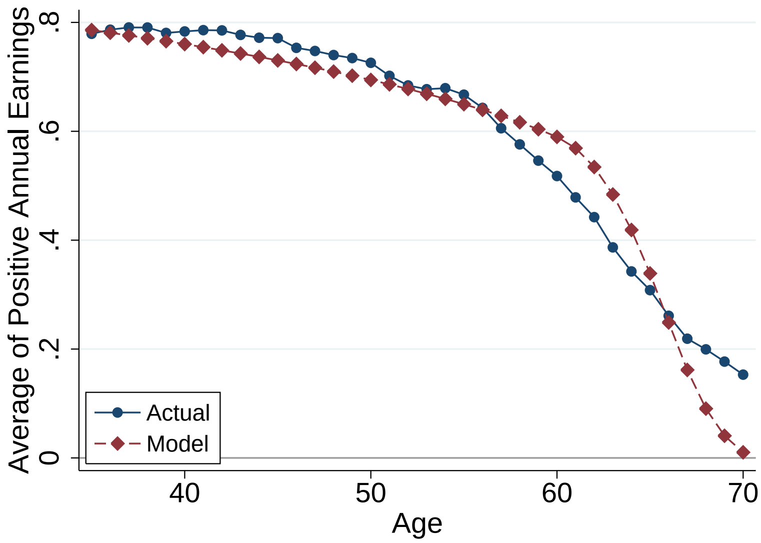
to last for 4 years, starting at age 31, the average age at the start of the experiment.

Calibrated parameters are shown in Table B.1.[38] The fraction of those who work in a given year, in the model and the data, are shown in Figure B.1. (We emphasize whether an agent works here rather than dollars earned because the extensive margin is the key factor in this model.) As in the data, individuals choose to retire as they get older.

Figure B.2 displays the effect of treatment on the fraction of agents who work in this model. For this result, we use parameters calculated based on data after treatment ended, but start the simulation at age 31 and allow treated agents to face the budget constraint defined by Equation 10 between ages 31 and 34. As hypothesized, the effect on the fraction working is strongest later in life, as agents approach retirement. Note that the effect of the treatment on working later in life is driven entirely by its effect on assets. As noted in the main text, the observed effect is much larger than would be expected due to the wealth shock alone. Indeed, the long-term effect on earnings is smaller than that seen in SIME/DIME, relative to the initial shock to assets. For every \$1 in increased transfers from the treatment, treated agents in the model earn only \$0.16 less in discounted lifetime earnings. Even so, the time pattern of effects mirrors that seen in SIME/DIME: treatment decreases propensity to work while it is in effect, then causes stronger effects as individuals approach retirement.

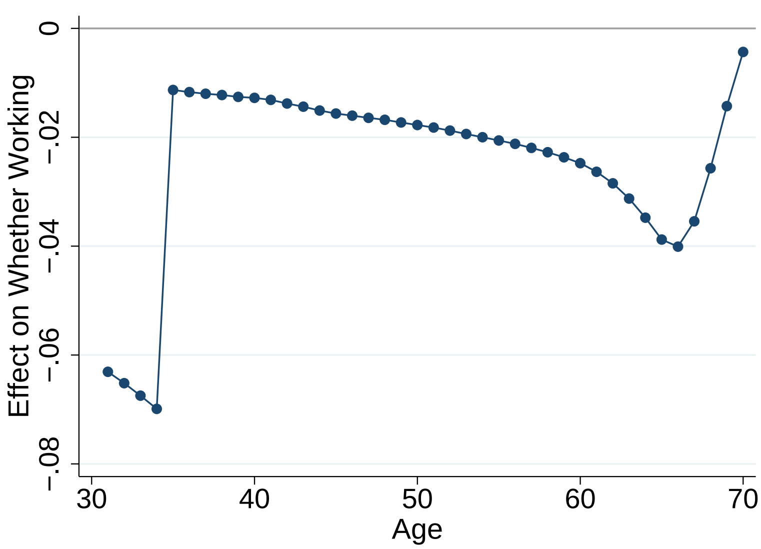
[38]Note that, because we bound the distribution of earnings at \$100,000, these parameters do not imply an implausibly high distribution of earnings, as they might seem to. However, it does imply that some agents would have very low earnings if they worked. This is because our model does not include agents working less than full time, whereas some individuals in the data likely did not work full time, and therefore have very low earnings. However, without data on hours, it is difficult to take this into account.

Figure B.1: Modeled and measured fraction working



Notes: "Actual" data shows the fraction of individuals in the SIME/DIME sample who worked at the given age. "Model" data shows the fraction of agents in the model who work at the given age. Parameters of the model are calibrated to match these two time series between ages 35 and 70, as shown in this graph.

Figure B.2: Simulated effect of treatment on working



Notes: Values shown are the difference between the fraction who work among treated agents, as opposed to untreated agents, as simulated in the model. Treated agents face a budget constraint with taxes and transfers shown in Equation 10 between ages 31 and 34; and taxes and transfers shown in Equation 9 beginning at age 35. Control agents always face the budget constraint with taxes and transfers shown in Equation 9.