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The Long-Term Effects of Cash Assistance*

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Abstract

We investigate the long-term effects of cash assistance for beneficiaries and their children by following up with participants in the Seattle-Denver Income Maintenance Experiment. Treated families in this randomized experiment received thousands of dollars annually in extra government benefits for three or five years in the 1970s. We match experimental records to Social Security Administration data using a novel algorithm and find that treatment decreased adults' post-experimental annual earnings by $1,800 and increased disability benefit applications by 6.3 percentage points, possibly driven by occupational changes. In contrast, children in treated families experienced no significant effects on any main variable studied.

JEL Codes: I14, I32, I38, J22

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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 has attracted interest, with proposals for policies or experiments on a basic income in Canada, Kenya, the United States,[3] and other countries. Given the importance policymakers attach to cash assistance, it is important to know whether it can sustainably reduce poverty and inequality. Alternatively, if recipients and their children are no better off—or face unintended consequences—after benefits end, policymakers should take that into account when formulating policy. However, few studies are able to identify the long-term effects of cash assistance on children, and almost no studies try to identify long-term effects on adult who receive assistance. Identifying these long-term causal effects is difficult largely because families who receive cash assistance generally differ from those who do not; further, it is often difficult to find outcomes for both treated and control groups after a long period of time.

We overcome these difficulties, 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.

[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`.

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; indeed, they were the first large-scale randomized controlled trials in the social sciences, and SIME/DIME alone cost about $275 million (Greenberg and Shroder (2004)). Short-term effects have been studied extensively, though no outcomes were measured after the 1970s.[5] Treatment caused adults[6] 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.

We are able to determine long-term outcomes by combining SIME/DIME data with administrative records from the Social Security Administration (SSA)[7] 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.

SIME/DIME data do not include traditional identifying information from participants, so standard matching techniques are not possible. To overcome this difficulty we created a matching technique (described in detail in Appendix A) that includes two novel steps that can both be applied to other settings. First, we identify potential participants by finding birth patterns from SIME/DIME data that match parent name patterns in SSA and WA DOH data. Second, we isolate likely-true matches by using incorrect data to find how many spurious matches can be expected; and using that data to estimate a model of the matching process using maximum likelihood.

Using this matched data set, in Section 4 we provide new evidence of significant effects on adults decades after the experiment ended. Treatment caused adults to retire almost a year early, decreasing their average annual earnings by $1,800 (7.4% of mean annual earnings) throughout the post-experimental period. 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

[5] A review of the hundreds of papers discussing the original results from the IMEs is beyond the scope of this paper. 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, we refer to individuals who were adults during the experiment as "adults," "parents," or "beneficiaries." Individuals who were the children of SIME/DIME adults are referred to as "children" in this paper even though they are now adults.

[7] SSA data, as well as 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.

be awarded benefits, or to have died.

We explore the mechanisms driving these effects, finding evidence against adult outcomes being driven by increased savings; changing preferences and beliefs about welfare; or time out of work during the treatment harming human capital and leading to decreased wages immediately after the treatment. However, we do find suggestive evidence for a more nuanced human capital story: treatment caused workers to switch to jobs that require less education, and use less abstract reasoning. This suggests that workers lost human capital while they took time off during treatment, but on returning to work found jobs that paid similar wages but were worse in non-monetary ways. Those worse jobs, though, could have been harder on their bodies (evidenced by the increased disability applications), or generally made them more dissatisfied in the long run, causing them to retire early.

In contrast to the significant effects on parents, in Section 5 we find little evidence of an effect on children for any variable studied. We can rule out (at the 5% level) 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). We can also rule out treatment changing the probability of a child applying for either SSDI or SSI by more than 3 percentage points (12% of the mean). These results are estimated with enough precision to rule out effects found in other similar contexts, and to inform the literature on intergenerational mobility.

Very little other research has been conducted on the impact of cash assistance programs—or, indeed, other types of government assistance—on adult beneficiaries long after the assistance has ended. Some papers (for example, Card and Hyslop (2005)) analyze outcomes in the first few years after a program ends, while others (for example, Jones and Marinescu (2018)) look at long-run effects of permanent programs. However, very few papers consider outcomes even 5 years after a program ends. (Two exceptions are Vartanian and McNamara (2004) and Wilde et al. (2014).) 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. Our evidence on the mediating role of occupations is consistent with findings by Huckfeldt (2016), Poletaev and Robinson (2008), and others finding that changes in occupation and occupational skills explains much of the earnings decline associated with job loss. It also supports findings by Kambourov and Manovskii (2009) and others that human capital is specific to occupations. 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.

The null effects we find for children supports other work—for example, Bleakley and Ferrie

(2013), Mayer (1997), and Shea (2000)—that has found little evidence that additional parental income benefits children in the long term. However, our results should be read in the context of papers that do document positive long-term effects of parental income, including Bastian and Michelmore (2018), Hoynes et al. (2016), and Oreopoulos et al. (2008). These papers are part of a growing literature documents that parental income, or factors related to income, can have long-lasting positive effects on children. (Literature reviews on this topic include Black and Devereux (2011), Cooper and Stewart (2013), Currie (2009), Fryer (2016), and Solon (2015).) Other research, though, indicates that parental receipt of government benefits increases the probability that children will receive benefits themselves (for example, Dahl et al. (2014), Gottschalk (1996), and Pepper (2000)), a concern that helped motivate welfare reform in the 1990s. This may be especially relevant in our setting, given the effects we find for adults. 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.”

Taken as a whole, our results suggest that cash assistance could have unintended 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. Our results also suggest that policymakers should do more to help non-employed workers return to their previous occupation, or switch to an occupation with good long-term potential.

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 discussion of policy implications and directions for future research.

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.[8] 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 would discourage work effort and lead to welfare dependency. (Policymakers also had

[8]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.

such concerns about the welfare system already in place at the time, for which official effective tax rates could approach 100%—though as we discuss later, actual effective rates were generally substantially lower.) 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; 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 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[9] would be given an annual transfer

[9]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

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),[10] and treatment lasted 3 or 5 years (again, depending on the randomly-assigned treatment group). (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.

Control families not receiving government benefits generally faced combined federal and state marginal income tax rates of around 20% to 35%.[11] However, government benefits received by control 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.)

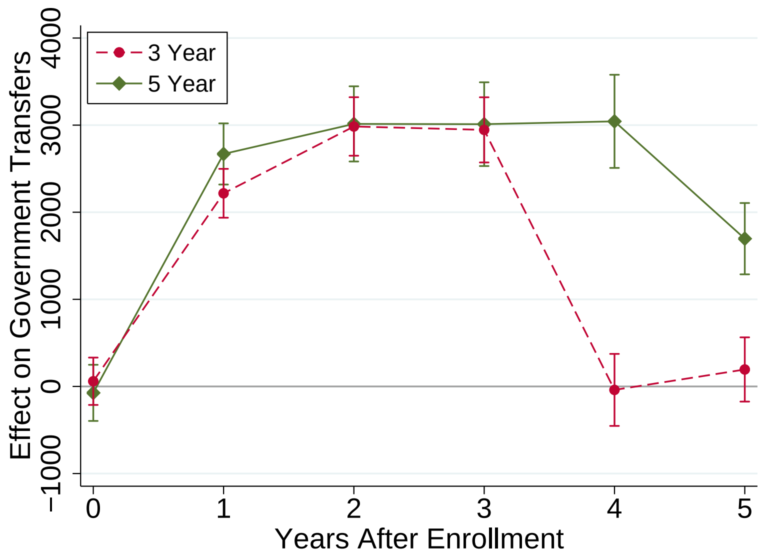
specification, we control for manpower treatment status; however we do not analyze its effect in detail as it is likely to be less generalizable to economic questions and policies today. Unless otherwise specified, "treatment" refers to financial treatment.

[10] 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).

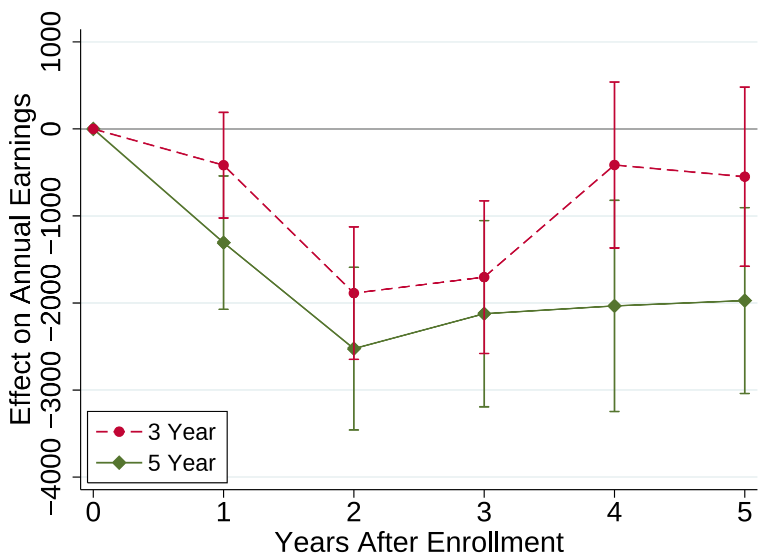
[11] See Tax Foundation (2013), Social Security Administration (2016), and Schrock (2010).

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.

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.[12] 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).

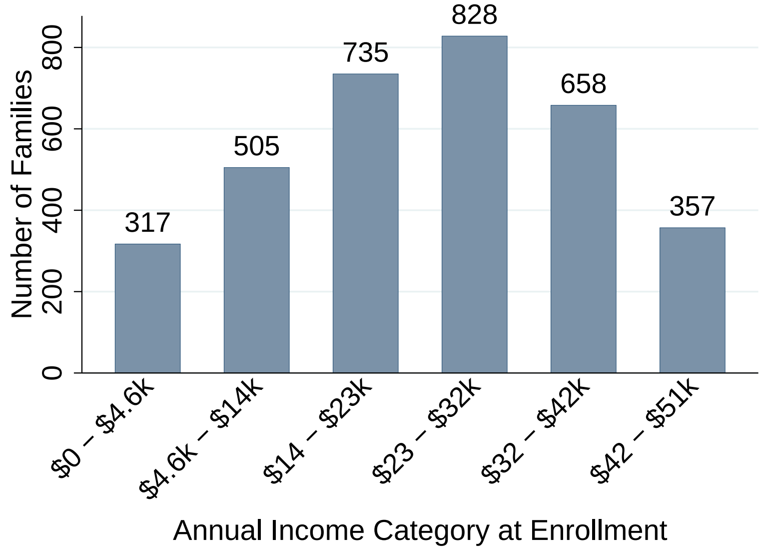
Combining all forms of income in a household—including earned and unearned income for all family members—and estimating taxes using TAXSIM (as provided by Feenberg (2016)) treatment increased families' after-tax income by an average of $640 per year. However, this number is particularly sensitive to 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 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

[12] 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.

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.

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),[13] 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),

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

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 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 about a year after Seattle. 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 the experiment began.

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.

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.

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.[14]

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.

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

To our knowledge, no data set exists today that includes SIME/DIME participants' traditional identifying information, such as names or social security numbers, so standard matching techniques are not possible. Instead, individuals from SIME/DIME are matched to outcomes using the procedure described in detail in Appendix A. This procedure includes two steps, both of which are

[14]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.

innovations on the prior matching literature and can be used in other settings to improve match rates or match previously-unmatchable data.

First, 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.) Other matching techniques match single records on one data set to single records on another; our technique gains power by matching multiple records at once.

Second, 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 to estimate a model of the matching process in a maximum likelihood procedure, thereby estimating the probability that each 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 families 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"

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.

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. (We pool all treatments to improve power. Treatments are analyzed separately in Tables 6 and 10; in those regressions, D_i is a matrix describing the family's treatment.) $\mathbf{S}_i$ 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 3, 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 3 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 other 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. On the other hand, treated individuals are no more likely to have been *awarded* disability benefits, or to have died by 2013. The point estimates on both disability awards and mortality are both positive, but—as shown in column 4 of Table 5—treatment increased the chance of being rejected for disability benefits among those who did apply. (This might be expected even if treatment increased actual disability, since applicants close to the margin of applying are likely less clearly disabled than the average applicant.) Columns 5 to 9 of Table 5 show that the increase in applications for disability are driven more by increases in claimed musculoskeletal disorders than any other cause. This is consistent with treatment causing workers to be in worse jobs that could leave them wanting to retire early, a possibility discussed in Subsection 4.2.

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)

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, other margins

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Retire Age (Max 65)**	**Awarded SSDI/SSI**	**Cancer**	**Circul-atory Disorder**	**Musculo-skeletal Disorder**	**Mental Disorder**	**Other Impair-ment**
Condition	**Alive**	**Earn>0**		**Applied SSDI/SSI**					
Treated	$-.0327^{**}$	-1030	$-.91^{***}$	$-.0716^{**}$	.00696	.0118	$.032^{**}$	$-.00397$	.00354
	(.0128)	(806)	(.343)	(.0362)	(.00742)	(.0113)	(.0145)	(.0109)	(.0167)
Dep var summary stats									
Mean	.74	33514	59.4	.727	.0307	.068	.13	.0724	.176
Std. Dev.	.438	23794	7.08	.446	.173	.252	.337	.259	.381
N	50458	37461	1780	724	2280	2280	2280	2280	2280
People	2236	2105	1780	724	2280	2280	2280	2280	2280
Clusters	1692	1609	1412	651	1720	1720	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. The dependent variable in column 3 is the individual's last year with any earnings, capped at 65; only individuals who are 65 or over in 2013, and who worked at least one year in the SSA data, are included. Dependent variables in columns 5 to 9 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.

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.

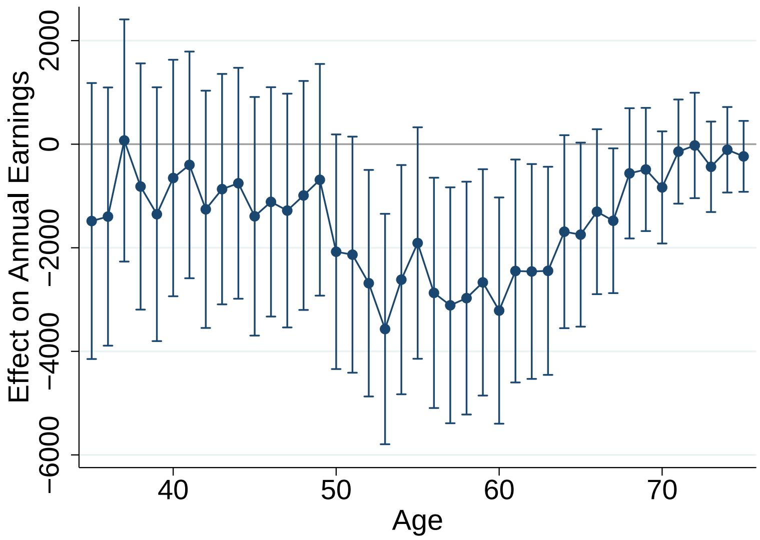
Regardless of the mechanism, the significant results on long-term outcomes may be surprising in the context of the original finding that there was no effect on earned income in the two years after the experiment ended, as shown in Figure 1b. Indeed, in many contexts—such as the Self Sufficiency Project 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 at each age from 35 to 75.[15] Effects are strongest between approximately ages 50 and 60, which corresponds to the time when most people leave the labor force and retire (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). Indeed, as shown in column 3 of Table 5, treatment causes workers to retire almost a year earlier. (No retirement effect was found in the original experiment because only 12% of adults were 50 or over by the fifth year of the experiment.) Although these delayed effects may be surprising, we might expect that effects on leisure would be strongest on the retirement margin. Most people take most of their leisure in retirement, so (absent any external forces like the SIME/DIME wage shock) it is natural that additional leisure is taken as earlier retirement. Any persistent shock to propensity to work would have the strongest effect when people are close to the margin between working and not, which occurs around retirement age for many people for a variety of reasons—precautionary savings, human capital accumulation, declining health, and so on. Indeed, in a related result, Imbens et al. (2001) find that winning the lottery leads to largest declines in earnings for those close to retirement age.

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.

[15] 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.

Figure 3: Effect on parents' earned income at different ages



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.

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 9). 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 permanent policy 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 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 participants to retire early and apply more for disability benefits. In this subsection, we explore why these effects may have occurred. In Subsubsection 4.2.1, we find evidence against effects being driven by wealth accumulation, changes in preferences or beliefs, or changes to wages. Instead, in Subsubsection 4.2.2, we show evidence suggesting that effects operated through changes in human capital and job quality. (An alternative method of analyzing mechanisms would be to look for subgroups of the population that experience particularly strong effects, and seeing whether there are other suggestive characteristics of those subgroups. However, that is not possible in this setting because there are few systematic differences in effects among subgroups.)

4.2.1 Evidence against effects being driven by wealth, preferences, or wages

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

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.

Table 7: Tests for wealth, preferences, and wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Change Assets Yr 1 - 3	**General Female Attitude To Work**	**General Male Attitude To Work**	**Believe Only Gov't Ben if Can't Support Self**	**Any Earnings Year 4**	**Log Wage Rate Year 4**
Treated	−1691	−.0198	.0824*	.0378	−.00896	.0199
	(2148)	(.0359)	(.0475)	(.0268)	(.015)	(.0242)
Dep var summary stats						
Mean	5723	.0324	.1	2.22	.728	2.57
Std. Dev.	41115	.999	.928	.656	.445	.656
N	2691	3047	1696	2632	3247	2279
People	2691	3047	1696	2632	3247	2279
Clusters	1753	3041	1696	2511	2213	1761

Notes: "Change Assets Yr 1 - 3" is the total dollar value of the change in an individual's family's total financial assets between years 1 and 3, for those individuals for whom we have data on assets in both years. "General Female/Male Attitude To Work" variables are summary measures of surveys of female and male attitudes toward work, respectively; higher values are associated with more positive attitudes toward work. "Believe Only Gov't Ben if Can't Support Self" is based on a survey question; a higher value is associated with a greater belief that people should receive government benefits regardless of their need. "Any Earnings Year 4" indicates whether the individual earned any income in Year 4 of the experiment. "Log Wage Rate Year 4" is the log of the wage in Year 4; it is only available for those who earned any money in that year. (Year 4 data on earnings and wages are restricted to individuals who were not in the 5-year treatment group.) Data on assets and survey variables are described in more detail in Appendix B.2.

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 positive shock to wealth would cause most people to choose to consume substantially fewer 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). Further, we have evidence from contemporaneous surveys of assets that there was no change in assets in Table 7, which investigates several potential mechanisms. As shown in column 1, treatment did not cause an increase in household assets between the first and third years of the experiment.

Second, it is possible that the experiment changed the way treated individuals perceived leisure 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. (The fact that treatment increased disability 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.) 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. Although survey elicitations of preferences and beliefs are imperfect, we do have some measures of preferences and beliefs about working and government assistance; see columns 2, 3, and 4 of Table 7. (These measures are described in more detail in Online Appendix B.2.) There is no evidence that treatment increased the value of leisure or the stigma associated with government benefits; if anything, treatment caused men to have a more positive attitude about work (though that question was asked less than a year after treatment began for most respondents, and thus might be a less reliable measure of long-term beliefs).

A third 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) 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 could 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. Column 5 of Table 7 shows that there is no significant effect of treatment on whether a person worked after treatment ends, and column 6 shows that there is no significant effect on their log wage rate. 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.

4.2.2 Evidence for changes in human capital and job quality

There is a more nuanced version of the human capital effect. It could be that, after spending additional time out of work, treated adults are hired in occupations that pay a similar initial wage, but that are also associated with lower long-term earnings—for example, because the occupation is more physically demanding. Adults might choose to switch to a less desirable occupation rather than accept a lower wage because they and their families had grown accustomed to a certain level of income. Alternatively, if workers are already close to the minimum wage, a job with lower long-run earnings may be the only less-desirable job available. Although we do not have occupation in our administrative data, we do have it in data from the experiment itself; we use that data to test this mechanism in Table 8.[16]

If treatment caused workers to switch to occupations with lower long-term earnings, then we might expect that treatment would increase the probability that a worker would switch occupations. In column 1 of Table 8, we see that there is no such effect, though this could be because fully 62% of individuals changed occupation during the experiment.

However, treatment did cause individuals to change what *kind* of occupations they worked in. Column 2 of Table 8 shows that treatment caused a switch to occupations requiring less education and column 3 shows that new jobs required less abstract reasoning. (There was no effect on the routine or manual intensity of the jobs; see columns 4 and 5.) This suggests that workers lost human capital during their time out of work. Although the new occupations did not involve lower earnings in either 1970 or 1990 (columns 6 and 7), and were not declining in that decade (column 8), it is possible that the new occupations had other non-pecuniary drawbacks that caused workers to want to retire earlier. Indeed, in light of the increased applications for disability benefits due to musculoskeletal issues, it is possible that the new jobs involved more strenuous work that left it difficult for treated individuals to work in old age, even if they didn't meet the SSA's definitions of disability.

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 9. 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,

[16] It is most natural to think about human capital's effects on jobs operating through occupation rather than industry: a job as a janitor, for example, requires similar skills in a school and an office. However, we examine effects on industry, and occupation/industry combinations, in Appendix Tables C.7 and C.8.

Table 8: Tests for changes in occupation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Occ Change	**Occ 1970 Avg Educ**	**Occ Abstract**	**Occ Routine**	**Occ Manual**	**Occ 1970 Avg Annual Earnings**	**Occ 1990 Avg Annual Earnings**	**Occ Change Empl 1970-'90**
Treated	−.0136	−.12**	−.205***	−.0129	.0146	−400	−528	−.043
	(.0226)	(.061)	(.0718)	(.101)	(.0548)	(452)	(509)	(.0366)
Dep var summary stats								
Mean	.624	10.9	1.94	4.25	1.39	23196	25971	.601
Std. Dev.	.485	1.49	1.68	2.39	1.32	12374	13360	.855
N	1898	2482	2478	2478	2478	2482	2482	2482
People	1898	2482	2478	2478	2478	2482	2482	2482
Clusters	1616	1902	1899	1899	1899	1902	1902	1902

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. All variables are restricted to individuals who were not in the 5-year treatment group. "Occ Change" indicates whether the individual changed occupations, for all individuals who have occupation recorded both before the experiment began and at least 4 years after it began. "Occ 1970 Avg Educ" measures the average education of the occupation in 1970. "Occ Y Avg Annual Earnings" measures annual earnings in year Y in the given occupation. "Occ Abstract," "Occ Routine," and "Occ Manual" measure task intensity of occupations. "Occ Change Empl 1970-'90" measures the log change in total employment in that occupation between 1970 and 1990. Other than "Occ Change," variables are based on the final occupation observed in the data in at least the fourth year after the experiment began. Variables about occupation are described in more detail in Appendix B.2.

as shown in Figures C.5 and C.6. Additionally, as shown in Table 11, there are no effects on any of the other 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. It is possible, however, that the overall null result masks important heterogeneity. Therefore, we consider possible heterogeneity below.

We first look at whether results vary by the family's type of treatment. Table 10 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.[17] 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. For example, 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.[18] Thus, inflating estimates and standard errors in Table 9 by 1/.22 as

[17] 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).

[18] This includes any government benefits in all SIME/DIME records. Restricting the analysis to income-dependent

Table 9: 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 10: 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.

Table 11: Children, other 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**					
Treated	.00541	−492	−.000502	.0000215	.00548	.0059	−.00282	.00508
	(.00815)	(570)	(.0295)	(.00281)	(.00415)	(.00798)	(.00945)	(.00991)
Dep var summary stats								
Mean	.79	28991	.511	.0111	.0217	.088	.125	.142
Std. Dev.	.408	24064	.5	.105	.146	.283	.33	.35
N	158763	125530	1385	5658	5658	5658	5658	5658
People	5636	5556	1385	5658	5658	5658	5658	5658
Clusters	2101	2097	976	2101	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. 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.

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 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, some research has found that cash assistance can cause children to earn more money later in life. For example, Aizer et al. (2016) analyze the Mothers' Pension (MP) program, an early welfare program that had similar absolute generosity and duration to the average payment from SIME/DIME treatment.[19] 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. Discouraging work could potentially change the way children view the role of work. (On the other hand, the disincentive effectively made it cheaper to spend time with children, which might be expected to lead to positive income effects later on.) 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 IME 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

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.

[19] Aizer et al. (2016) find that the median duration in an MP program was three years, versus three to five years for SIME/DIME. The average annual MP transfer (adjusted for inflation based on the 1929 PCE, the earliest value available) ranges from $1,300 to $3,900; our best estimate of the effect of SIME/DIME on family after-tax income, adjusting for misreporting, is $2,000 per year.

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.

Some evidence suggests that these long-term effects for adults were driven by changes in human capital: treated adults spent more time out of work during treatment, which may have lowered their human capital and caused them to get worse jobs when they returned. These worse jobs could have led them to choose to retire early, potentially because a more strenuous job could lead to worse health, which would also explain the increased disability applications.

Whatever the cause, we 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 contexts, cash assistance may not have a large intergenerational effect.

More research is needed 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. Any findings can help put the present results in their proper context, inform our understanding of the mechanisms driving these results, 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. Researchers should particularly focus on the way policies can change the occupations that workers choose, and the long-term implications of that choice. 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.

Taken together, the long-term result we find suggest that policymakers should be cautious in increasing the use 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 do not find support for some concerns about the consequences of cash assistance, with no evidence that it creates a welfare culture that is passed down to future generations.

Additionally, our results suggest that policies should do more to encourage workers to be in occupations that have good long-term career prospects. For example, job search assistance could focus not just on careers that are a good fit now, but will also be a good fit as the worker approaches retirement, even at the cost of a lower current salary or a more difficult commute. Such assistance could particularly focus on finding a job in the same occupation as the previous job held. Unemployment benefits could also be structured to encourage workers to go back to the same occupation—for example, by paying higher benefits for a longer period of time if the worker is searching for a job in the same occupation. This focus on getting a worker back to the same occupation might be tempered if the occupation is declining or the worker is clearly not qualified for it, but could still be helpful for many workers.

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. In a time of rising inequality, policymakers may need to look beyond cash assistance to help families that are struggling to get by.

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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.[20] 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.[21]

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.[22] 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

[20] 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).

[21] 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.

[22] 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 20). For a specific parent name that occurs with frequency f,[23] 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.[24] (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

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

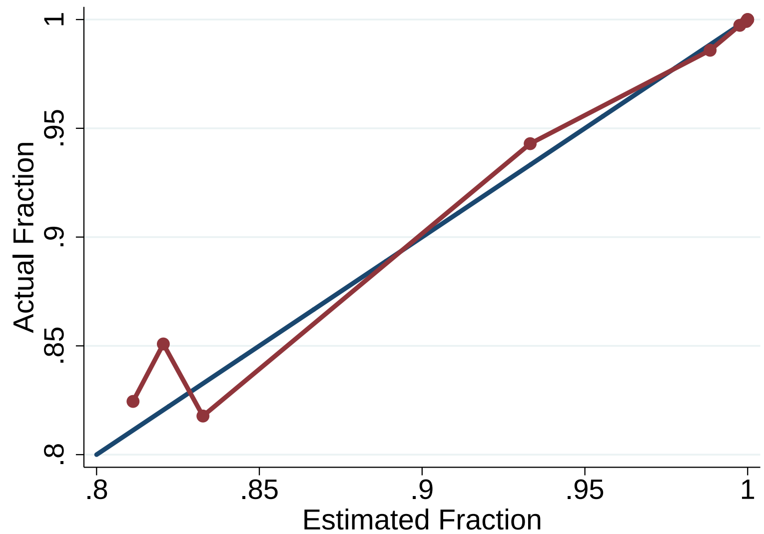
where $\beta \equiv \frac{\prod_i (n_i)}{\tau}$. We can now estimate α and β with MLE using one observation for all matched people (with placebo birthdays or with real birthdays), and set an indicator variable to 1 if the observation is from a real birthday, and 0 if it is from a placebo birthday.[25] From this we calculate

[23] 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.

[24] 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.)

[25] 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).[26] 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.[27] We can use a simple model to show that this duplicate

[26]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.

[27]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,[28] so we can match that with the names of participants we find using the SSA Numident file. Using Bayes' Theorem,

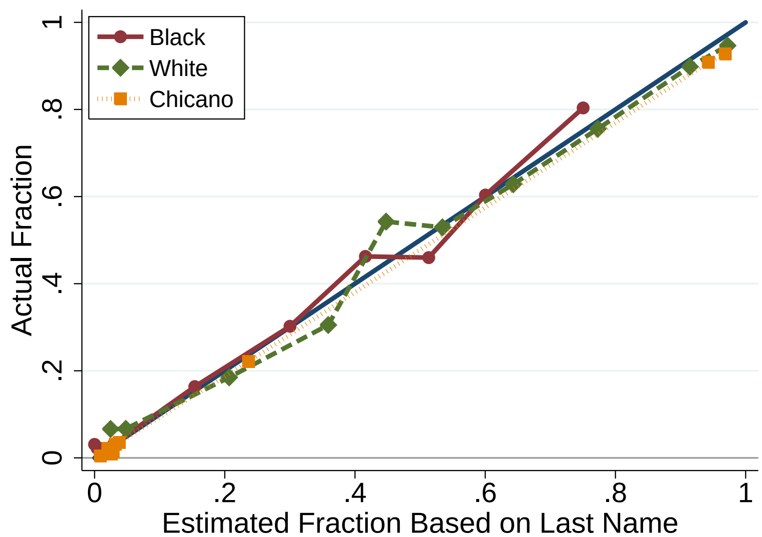
$$\mathbf{P}(R|N) = \frac{\mathbf{P}(N|R)\mathbf{P}(R)}{\sum_r \mathbf{P}(N|\text{race} = r)\mathbf{P}(\text{race} = r)}, \tag{5}$$

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.

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

Figure A.2: Correspondence between SSA and SIME/DIME 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.[29] 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

[29] 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.