

The Long-Term Effects of Cash Assistance[†]

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We investigate the long-term effects of cash assistance on beneficiaries and their children among participants in the Seattle-Denver Income Maintenance Experiment. Treated families in this randomized experiment received thousands of dollars 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, finding that treatment decreased adults' postexperimental annual earnings by \$1,800 and increased their disability benefit applications. Effects were strongest around retirement age; evidence suggests possible occupational mechanisms. In contrast, children experienced no significant effects on earnings or interaction with the disability system. (JEL G51, H55, H75, I32, I38, J12, J14)

Thirty-eight 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 one—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 \$7 billion through Temporary Aid to Needy

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¹See Shrider and Creamer (2023) for US poverty data, World Bank Group (2022) for world poverty data, and Piketty (2013) for data on inequality.

Families (TANF), \$30–\$530 billion through unemployment insurance,² and billions more from other sources. The idea of expanding the role of cash assistance has attracted increasing interest, with proposals for policies or experiments on a basic income around the world. Recently, cash assistance to individuals has been a major part of the economic response to COVID-19 in many developed countries, including stimulus checks and expanded unemployment benefits in the United States.

Given the importance that policymakers attach to cash assistance, it is important to know whether it can sustainably reduce poverty and inequality by raising incomes and improving other outcomes for poorer families. 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 adults 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 and other similar income maintenance experiments (IMEs), described in more detail in Section I, 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). That SIME/DIME was so large and so early makes it an ideal setting to examine well-identified and precise long-term causal effects.

SIME/DIME guaranteed a minimum annual income of up to \$25,900³ 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 percent to 80 percent 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. Short-term effects of this experiment on a “negative income tax” (NIT)—whereby the government gives money to families rather than taking money away—have been studied extensively, though no outcomes were measured after the 1970s. Treatment caused adults to work about 12 percent 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.

²See <https://sgp.fas.org/crs/misc/RL32760.pdf> and <https://fred.stlouisfed.org/series/W825RC1A027NBEA>.

³Unless otherwise noted, dollar values are adjusted for inflation to 2013 values with the personal consumption expenditures (PCE) deflator from <https://research.stlouisfed.org/fred2/series/PCEPI/downloaddata?cid=21>.

We are able to investigate the effects of cash assistance on outcomes by combining SIME/DIME data with administrative records from the Social Security Administration (SSA)⁴ and the Washington State Department of Health (WA DOH). Our main outcome measures come from SSA data, which include annual earned income for 100 percent of workers between 1978 and 2013, information on applications for and awards of disability benefits, and mortality. Described in more detail in Section II, 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; that is why no follow-up has previously been conducted. 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 dataset, in Section III we provide new evidence of significant long-run effects on adults.⁵ Treatment caused adults to earn about \$1,800 less per year (7.4 percent of mean annual earnings) on average throughout the postexperimental period. Most of that effect occurred around retirement age; in fact, treatment caused adults to retire almost a year early. Treated adults were also 6.3 percentage points (20 percent of the mean) more likely to apply for disability benefits (either SSDI or SSI) but were not significantly more likely to be awarded benefits or to have died.

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 hourly wages immediately after the treatment. However, we do find suggestive evidence for a more nuanced human capital story: Treatment caused workers to take jobs that require less education or vocational preparation, use less mental skill or creativity, and confer less prestige. This suggests a story that future research will need to examine more closely: 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 nonmonetary ways. Those worse jobs could have generally made them more dissatisfied in the long run, causing them to retire early.

Long-term effects on children may be of even greater interest than effects on parents, given the longer literature studying it, as outlined below. However, in contrast to the significant effects on parents, in Section IV we find little evidence of an effect on children for earnings, disability applications, or disability receipt. This is despite treatment involving significant shocks to children: Their parents have

⁴ SSA data, as well as data that are commingled with SSA data, were handled on secure SSA computers by SSA personnel authorized to use those data for other purposes, following SSA data procedures.

⁵ 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.

greater financial resources and more time to spend with the children; on the other hand, children see their parents working significantly less. We can rule out (at the 5 percent level) effects on child propensity to work of more than 1.9 percentage points (2.5 percent of the mean), and a change in annual earned income of more than \$1,500 (6.9 percent 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 percent of the mean). There are also no significant effects on marriage, divorce, or mortality. 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 and Neumark and Shirley 2020) analyze outcomes in the first few years after a program ends, while others (for example, Jones and Marinescu 2022) look at long-run effects of permanent programs. However, very few papers consider outcomes even five years after a program ends. (Exceptions are Aizer, Eli, and Lleras-Muney 2020; Deshpande 2016; Hotz, Imbens, and Klerman 2006; Vartanian and McNamara 2004; and Wilde, Rosen, Couch, and Muennig 2014; all examine effects approximately 10–20 years after a program ends, which is substantially less than the roughly 40 years of postexperimental data examined here.) This is an important omission because significant postexperimental effects from SIME/DIME take many years to appear. Many of these prior papers also do not examine effects on the low- and middle-income populations for whom cash assistance is most policy-relevant, as we do.

Our results on adults also relate to the literature on occupation-specific human capital and the effects of occupational change, including studies by Huckfeldt (2022); Kambourov and Manovskii (2009); Poletaev and Robinson (2008); and Shaw (1987). Finally, similarly to studies on lottery winners such as Cesarini, Lindqvist, Notowidigdo, and Östling (2017) and Imbens, Rubin, and Sacerdote (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 (2016) 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 Aizer, Eli, Ferrie, and Lleras-Muney (2016); Bastian and Michelmore (2018); Hoynes, Schanzenbach, and Almond (2016); and Oreopoulos, Page, and Stevens (2008). These papers are part of a growing literature that 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 2021; Currie 2009; Fryer 2017; and Solon 2018.) Other research, though, indicates that parental receipt of government benefits increases the probability that children will receive benefits themselves (for example, Dahl, Kostøl, and Mogstad 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, 152) cites the original findings in asking, “Does welfare undermine the family? As far as we know from the NIT experiment, it does, and the effect is large.” This paper offers the first new evidence against that influential consensus about the IMEs.

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 nonemployed 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 I describes the institutional background of the IMEs. Section II describes the SIME/DIME and administrative data used in this paper. Section III presents the analyses and results on adults, while Section IV does the same for results on children. Section V concludes with a discussion of policy implications and directions for future research.

I. The Income Maintenance Experiments

A. General Background on the Experiments

SIME/DIME and previous smaller IMEs were conceived in the 1960s to test possible changes to the welfare system.⁶ 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 such concerns about the welfare system already in place at the time, for which official effective tax rates could approach 100 percent—though, as we discuss later, actual effective rates were generally substantially lower.) To determine whether 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 to 1972), the Rural IME (in Iowa and North Carolina, from 1969 to 1973), the Gary IME (in Indiana, from 1971 to 1975), 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.⁷

⁶Unless otherwise noted, the background described in this subsection is drawn from Office of Income Security Policy, Office of the Assistant Secretary for Planning and Evaluation, and US Department of Health and Human Services (1983); Spiegelman (1983); or new analysis of SIME/DIME data.

⁷A 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*

TABLE 1—PREEXPERIMENTAL DATA ON SIME/DIME FAMILIES AND ADULTS

Level	Variable	Fraction
Family	Seattle	0.434
Family	Two household heads	0.38
Family	Black	0.389
Family	White	0.415
Family	Chicano	0.196
Family	Positive preexp benefits	0.462
Adult	Positive preexp earned inc	0.656
Adult	Male	0.36
Adult	Education: HS+	0.537
Adult	Education: college+	0.0251

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 preexp 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 preexp 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.

Some characteristics of the SIME/DIME families in our sample are displayed in Table 1. As discussed in Section IIB, 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 socioeconomic status. About half of the adults had fewer than 12 years of schooling, only about two-thirds of adults had any preexperimental earned income, and those who did have such income had average annual earned income of \$23,000. (Adults averaged 33 years old when the experiment began.)

Each family enrolled in SIME/DIME was assigned to a treatment category using stratified random assignment in 1970–1971 (in Seattle) or 1971–1972 (in Denver), as discussed in Section IB. A family of four that was assigned to the financial treatment⁸ would be given an annual transfer of \$17,600, \$22,200, or \$25,900 (depending on the treatment group they were assigned to) if they earned no other income, and payments were adjusted for family size. Lower-income families were more likely to be assigned to lower guarantee levels. Every extra dollar that the family earned would be taxed back at a rate between 50 percent and 80 percent (with the

Human Resources, 15 (4) and an overview in Office of Income Security Policy, Office of the Assistant Secretary for Planning and Evaluation, and US Department of Health and Human Services (1983). A bibliography of contemporary papers from the IMEs is available at https://web.archive.org/web/20160909123006/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.

⁸SIME/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 percent or 100 percent subsidies for education. In our main specification, we control for manpower treatment status; however, we do not analyze its effect in detail as it is likely to be less generalizable to economic questions and policies today. Unless otherwise specified, “treatment” refers to financial treatment.

precise rate selected randomly),⁹ and treatment lasted three or five years (again, depending on the randomly assigned treatment group). (To fix ideas, suppose that a family received the \$22,200 guarantee with a tax rate of 50 percent. 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 percent, 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 1, panel A shows the effect of treatment on total government transfers received each year for the first five years after assignment to treatment.

Control families not receiving government benefits generally faced combined federal and state marginal income tax rates of around 20 percent to 35 percent.¹⁰ 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.)

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 percent in Washington State in 1971, much less than the statutory 67 percent. Moffitt (1979) estimates effective tax rates of around 50 percent for some low-income families in Indiana in 1973, combining income taxes, AFDC, food stamps, and Medicaid. Halsey (1978) uses SIME/DIME data

⁹For 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 percent earned a nominal \$2,000, their actual average tax rate would be $80 - 5 = 75$ percent, so they would pay a nominal \$1,500 in taxes (after receiving the full guarantee).

¹⁰See Tax Foundation (2013); Social Security Administration (2016a); and Schrock (2010).

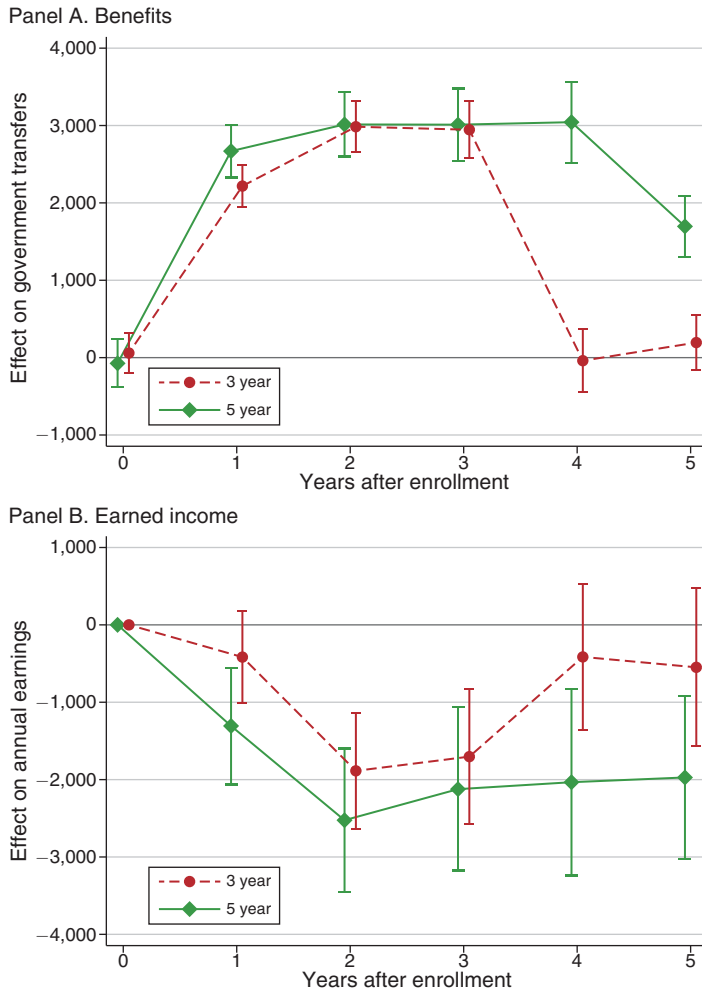


FIGURE 1. EFFECTS ON GOVERNMENT TRANSFERS AND EARNINGS DURING AND IMMEDIATELY AFTER TREATMENT

Notes: Based on public SIME/DIME data for original families with at least two children. Each data point represents the estimate and 95 percent 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 nontreated individuals. The dependent variable in Figure 1, panel A is total government benefits, including SIME/DIME payment; the dependent variable in Figure 1, panel B is earned income. Benefit levels are apportioned equally to each household head for comparability with earned income data. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental earned income, sex, and a cubic polynomial of date of birth.

to estimate effective tax rates for program participants that vary widely but are generally between around 30 percent and 50 percent.

Treatment should therefore be thought of as simultaneously increasing transferred cash to treated families, increasing effective tax rates, and simplifying the budget set. Increases in tax rates and in wealth are both generally thought to decrease labor

supply, and, as shown in Figure 1, panel B, treatment reduced earned income by an average of \$1,600 per year.¹¹ This was driven by the fact that treated individuals reduced their hours of work by an average of 12 percent during treatment, mostly taken as longer nonemployment spells (that is, the effect is driven by more months with zero earnings). Labor supply responses were generally larger for families in the five-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 three years (postexperiment data are not available in public data for the five-year treatment group).

Combining all forms of income in a household—including earned and unearned income for all family members—estimating taxes using TAXSIM (as provided by Feenberg 2016), and adjusting for the biases found by Greenberg and Halsey (1983), treatment increased families' after-tax income by an average of \$2,000 per year. The additional monetary resources came at the same time as additional nonworking 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, Hannan, and Tuma (1983) found that treatment caused Black and White families to be approximately 40 percent 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 Supplemental 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 nonlabor supply effects, which “were not given the same degree of attention” in the design of the IMEs. Broadly, however, few systematic significant effects were observed on types of goods consumed, fertility, child academic outcomes, or other indicators.

B. 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),¹² family type (headed by one or two

¹¹ 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.

¹² We follow official SIME/DIME terminology in the names for these groups and in calling them all races.

TABLE 2—PREEXPERIMENTAL BALANCE AMONG MATCHED SAMPLE

Variable	Whole sample (1)	Denver only (2)
Earned income	−1,445 (560)	−444 (723)
Hours worked	−75.4 (34.9)	−17.9 (47.9)
Government benefits	466 (277)	−101 (319)
Years of ed	−0.117 (0.0994)	−0.141 (0.13)
Kids age 0–5	0.0311 (0.0511)	−0.00792 (0.0687)
Kids age 6–15	−0.0635 (0.0741)	−0.104 (0.101)
People age 16+	−0.0602 (0.0621)	−0.0148 (0.0822)

Notes: Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. 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 preexperimental income category). “Earned income,” “Hours worked,” and “Government 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. “People age 16+” measures the number of nonhousehold heads age 16 and above, before the experiment began. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE.

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 (regardless of whether we match them) are shown in Table 1 and Supplemental Appendix Figure C.1, along with other details about the sample. According to published accounts, within these groups, treatment was assigned randomly.

A balance test on preexperimental variables for all adults in our analysis sample (that is, those matched to SSA records, as described in Section IIB) is shown in Table 2; based on this test, treatment and control do not seem to be balanced in our overall sample. Controlling for assignment groups, treated individuals earned about \$1,400 less than controls in annual preexperimental earned income (compared to an average of \$16,000). Significant differences also exist in preexperimental hours worked. Supplemental Appendix Table C.1—based on all adults, regardless of whether we match them—shows that there is a lack of balance in the experiment itself even before our match takes place. Such a preexperimental difference could occur by chance, though that is unlikely.

One potential cause of the imbalance is incomplete take-up: As noted by Christophersen (1983a), 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 is unlikely to lead to biased estimates. However, 7 percent 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 refusal rate is fairly low, it is difficult to quantify any bias that 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 preexperimental 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 (1983a) 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 Supplemental Appendix D. Estimated effects are similar in Denver only (compared to the combined Seattle-Denver sample), though they are less precisely estimated.

II. Data and Methods

A. 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 for any biological children who were under 18 years old when the experiment began. These data include demographic data, including age and education level; variables related to the experiment, including treatment status and stratification group; key outcomes during the experiment and immediately after, including earnings and hours worked; and a wealth of other data gathered in surveys, including on assets, occupations, and attitudes.

We measure labor market outcomes using income data from the Social Security Administration’s (2016b) 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 and 2013.¹³ Earned income data (along with all other monetary data in this paper, as noted in footnote 3) 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

¹³ Prior to 1978, income data are only available for a small fraction of the population, which would not be suitable for this analysis. Unfortunately, SIME/DIME ended soon before MEF data begin, so we cannot use the MEF to analyze earnings during the experiment.

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 percent of annual observations for children and 1.7 percent for adults are above this level.

To explore the effect of SIME/DIME on interactions with the disability system, we use the Social Security Administration's (2016b) 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 percent of applications between 1978 and 1989, with no known systematic difference between denied applications included or not included in the datasets.¹⁴

We are able to study mortality using data from the Social Security Administration (2016b) 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 percent 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 Washington State Department of Health 2015) from 1979 to 2013 (matched to SSA records with Social Security numbers (SSNs)) for Seattle participants 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 for Seattle participants. These records are matched to SSA 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.

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 percent 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 that 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.

B. Matching Experimental Families to Outcome Data

To our knowledge, no dataset exists today that includes SIME/DIME participants' traditional identifying information, such as names or SSNs, 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 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 (for Seattle participants) WA DOH birth records. For example, suppose that a mother has two male children born on February 1 of 1960 and 1961. It is unlikely that another family has exactly the same birth pattern. Thus, we look for one male birth on each day with the same mother name; if we find a record from each day with a matching parent name, we provisionally assume that these records correspond to the SIME/DIME family. (Finding multiple children with a parent name in common 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 dataset to single records on another; our technique gains power by matching multiple records at once, which can improve matching efficiency in other settings.

Second, after the initial match, we eliminate many spurious matches by dropping matches made due to frequently used names. (In our example above, if the common mother name was Jane Smith, that is likely to be different mothers with the same name.) In particular, we drop all matches where the parent name is more common than some threshold; exactly where to draw the name frequency threshold, however, depends on family size, among other factors: It is more likely that different people named Jane Smith gave birth on two particular days than on seven particular days. To optimally set this threshold, we estimate the probability that each match is correct and (in our baseline procedure) include all SIME/DIME individuals who are matched to exactly one SSN with at least 95 percent confidence. This probability is estimated with a placebo test: We add a certain number of days to each birthday and rerun 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: Roughly, the number of nonspurious matches is equal to the number of matches found based on the true birthday, minus matches found based on a placebo birthday. This technique of optimally choosing a name frequency threshold can improve matches made using almost any matching algorithm.

With this algorithm, we match 45 percent of parents and 59 percent of children. There is no significant effect of treatment on the probability that we find either parents or children, overall or in various subgroups, as shown in Table 3, panel A; each cell of that table shows the results of a different regression testing the effect of treatment on match rate within a subgroup of the population. (Null results for other subgroups are shown in column 1 of Supplemental Appendix Table C.4 for parents and C.7 for children.) Different treatment arms also each have no significant effect on the match rate, as shown in Table 3, panel B. In Appendix A4, we explore the

TABLE 3—EFFECT OF TREATMENT ON MATCH RATE

Sample	Parents (1)	Children (2)
<i>Panel A. Overall and within subgroups</i>		
All	−0.00125 (0.0158)	−0.00623 (0.0167)
Fam inc < \$14k	−0.0402 (0.0354)	−0.00441 (0.0344)
Fam inc \$14k–32k	0.000592 (0.0232)	−0.0138 (0.0245)
Fam inc \$32k +	0.0146 (0.0272)	0.00546 (0.0303)
Female	−0.00774 (0.0176)	−0.0206 (0.0194)
Male	0.0106 (0.0239)	0.00689 (0.0198)
Black	−0.00983 (0.0251)	−0.00177 (0.0271)
White	−0.00201 (0.0255)	−0.007 (0.026)
Chicano	0.0208 (0.0325)	−0.00773 (0.0356)
Denver	−0.00734 (0.0204)	−0.00952 (0.0221)
Seattle	0.0047 (0.0248)	−0.00605 (0.0253)

(continued)

three pathways through which treatment could affect the match rate and thereby confound the results: fertility, mobility, and SSN applications. In that section, we conduct robustness checks showing that our main results are unaffected by these confounds by running our matching algorithm in ways that the confounds could not affect.

As discussed in Appendix A3, we estimate that 5.2 percent of matched adults and 1.3 percent 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. Indeed, this combination of high true match rate and low false match rate (especially for children) is well beyond the frontier for matching algorithms discussed by Abramitzky et al. (2021), suggesting that this algorithm can help advance the literature on record linkage.

Summary statistics for main outcome variables for these matched individuals (in both treated and control families) are shown in Table 4. 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. In these ways, they may be more

TABLE 3—EFFECT OF TREATMENT ON MATCH RATE (*continued*)

Sample	Parents (1)	Children (2)
<i>Panel B. Different treatments</i>		
Treated	−0.00143 (0.0158)	−0.00577 (0.0167)
5-year trtmnt	0.00454 (0.0214)	0.00259 (0.0223)
Guar level	5.53e−07 (3.35e−06)	−7.80e−07 (3.48e−06)
Tax rate, \$0	0.0105 (0.126)	0.194 (0.13)
Tax decline?	−0.00358 (0.0285)	−0.034 (0.0292)
Manpower	0.0171 (0.0156)	0.0102 (0.0163)
<i>Dep var summary stats</i>		
Mean	0.45	0.589
Std. dev.	0.498	0.492
Observations	5,185	9,676
People	5,185	9,676
Clusters	3,400	3,345

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental earned income, sex, and a cubic polynomial of date of birth. The dependent variable is an indicator for the individual being matched to an SSN with at least 95 percent certainty. There is one observation per child or parent in any IME family with at least two children; this regression, unlike most others in this paper, includes individuals in the experiment who were not matched to administrative data. Results are shown separately for parents and children. In Table 3, panel A, each cell reports the results of one regression with the sample given by the column, for the subgroup given by the row. “Fam inc” levels are based on preexperimental normal income categories. In Table 3, panel B, independent variables are variations on possible treatments. “5-year trtmnt” is an indicator for being in the treatment for five years, as opposed to three 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 de-meanned, 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.

representative of average welfare beneficiaries, who are likely to be the beneficiaries of the policies that this experiment would inform.

These differences are all reminders that, as with almost any experiment or quasi-experiment, attention should be paid to external validity 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. As

TABLE 4—SUMMARY STATISTICS BASED ON OUTCOME VARIABLES

	Parents			Children		
	Sample mean (1)	Comp mean (2)	<i>p</i> -value (3)	Sample mean (4)	Comp mean (5)	<i>p</i> -value (6)
Positive annual earnings	0.709	0.701	0.332	0.769	0.798	0.000
Annual earnings	23,748	27,143	0.000	22,281	27,704	0.000
Applied disability	0.318	0.17	0.000	0.245	0.145	0.000
Awarded disability	0.247	0.131	0.000	0.13	0.0873	0.000
Died	0.385	0.298	0.000	0.0728	0.0556	0.000

Notes: Outcomes based on SSA data. “Sample” refers to the same SIME/DIME matched sample described in Section II. 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.

noted by Christophersen (1983b), though, neighborhoods were selected to include as many eligible families as possible, few families refused enrollment (unsurprisingly, given the generous treatment), and the eligible income ranges match those of likely beneficiaries of the policies that this experiment would inform. Further, we are only able to study families with at least two children (which includes 74 percent of adults and 92 percent of children in the original families), 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. As shown in Supplemental Appendix Table C.4, effects are somewhat larger for adults (more disability applications and less likely to earn income) in smaller families, while Supplemental Appendix Table C.7 shows few differences for children, suggesting that including smaller families might strengthen the measured effects on adults. Other results (available upon request) show little difference by name frequency for adults, while the effect for children with more common names is closer to the adult results (more applications and less income)—suggesting that including those with more common names could tilt results in that direction. Furthermore, we are only able to measure effects of the SIME/DIME treatment itself rather than any policy currently under consideration. Thus, for example, these results may be more informative about welfare policies targeted at the poor than “basic income” proposals with tax rates of 0 percent. Proposed policies would also likely last longer; as discussed below, evidence from the five-year treatment arm of SIME/DIME suggest that longer-term treatments may have stronger effects on both adults and children. We can say little about general equilibrium effects of a similar policy because there were very few treated families relative to the Seattle and Denver metropolitan areas; if treatment were more widespread, that could increase wages and shift norms (as adults work less), and increase prices (as labor becomes more expensive). Finally, SIME/DIME control families were also able to use AFDC, food stamps, unemployment insurance, and other welfare programs. For this reason, our results should be used to understand what would happen if a standard welfare program became more generous

rather than to compare a welfare program to a lack of such a program. Additionally, publication bias can increase the chance that a paper would be written if, like this one, effects are surprisingly large. 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—and, of course, few to none without similar external validity concerns.

C. 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

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

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 7 and 12; 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 preexperimental "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 preexperimental earned income (for children, data on adult preexperimental income, as well as any other data on adults, are based on the primary breadwinner: the parent who earned the most preexperimental income). When the outcome of interest is available at an annual frequency, as with most variables based on income, we estimate

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

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 those in Figure 2 and Figure 3, show data at an annual frequency. Each point on these graphs represents the results of a single regression using the methodology described above. For example, Figure 2, panel C 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 one in 1971 (for Seattle) or 1972 (for Denver). Each point in Figure 3, panel B represents

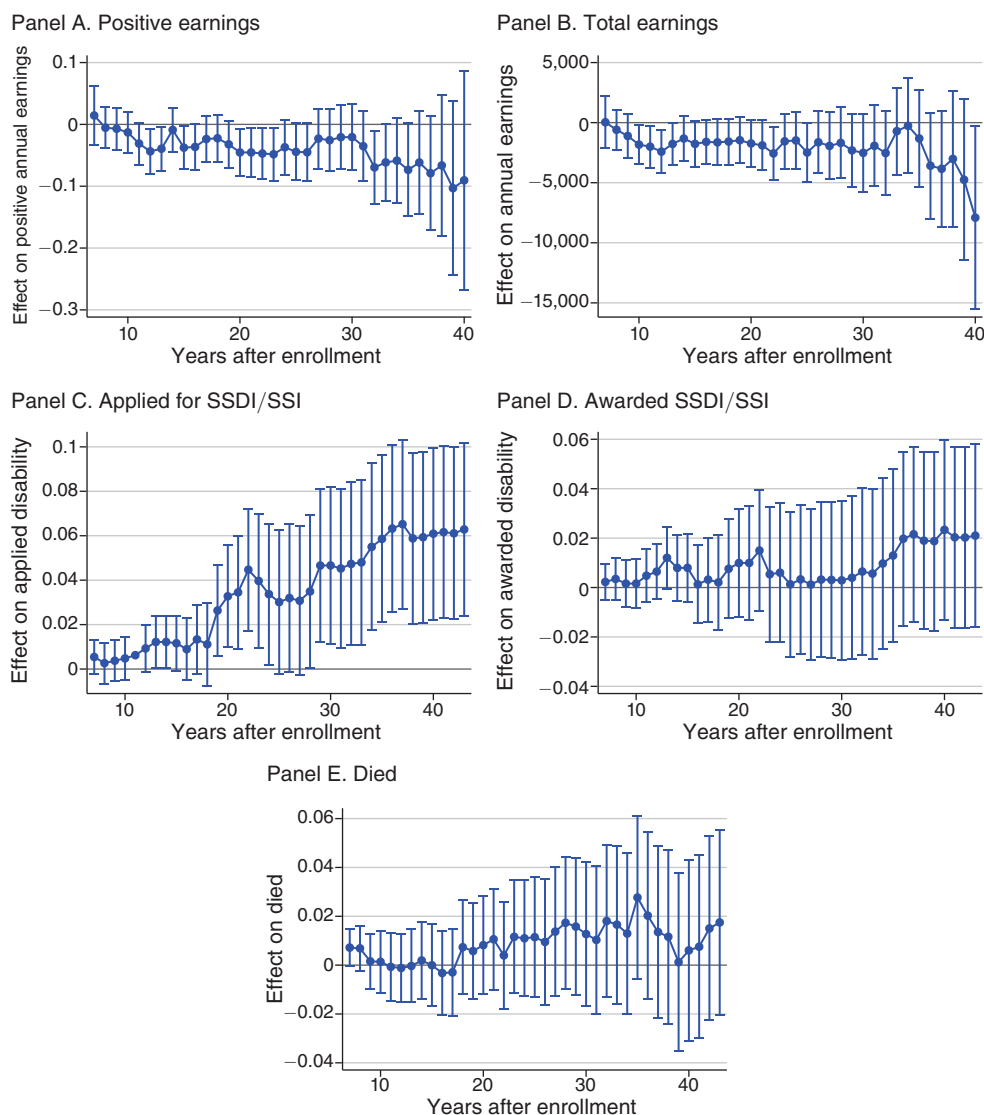


FIGURE 2. PARENTS, EFFECTS BY YEARS AFTER START OF EXPERIMENT

Notes: Each data point represents the estimate and 95 percent 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. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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. 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.

the estimate and 95 percent confidence interval for the regression coefficient on treatment status, where the dependent variable is earned income; data are restricted to that from the year that an individual turned a given age.

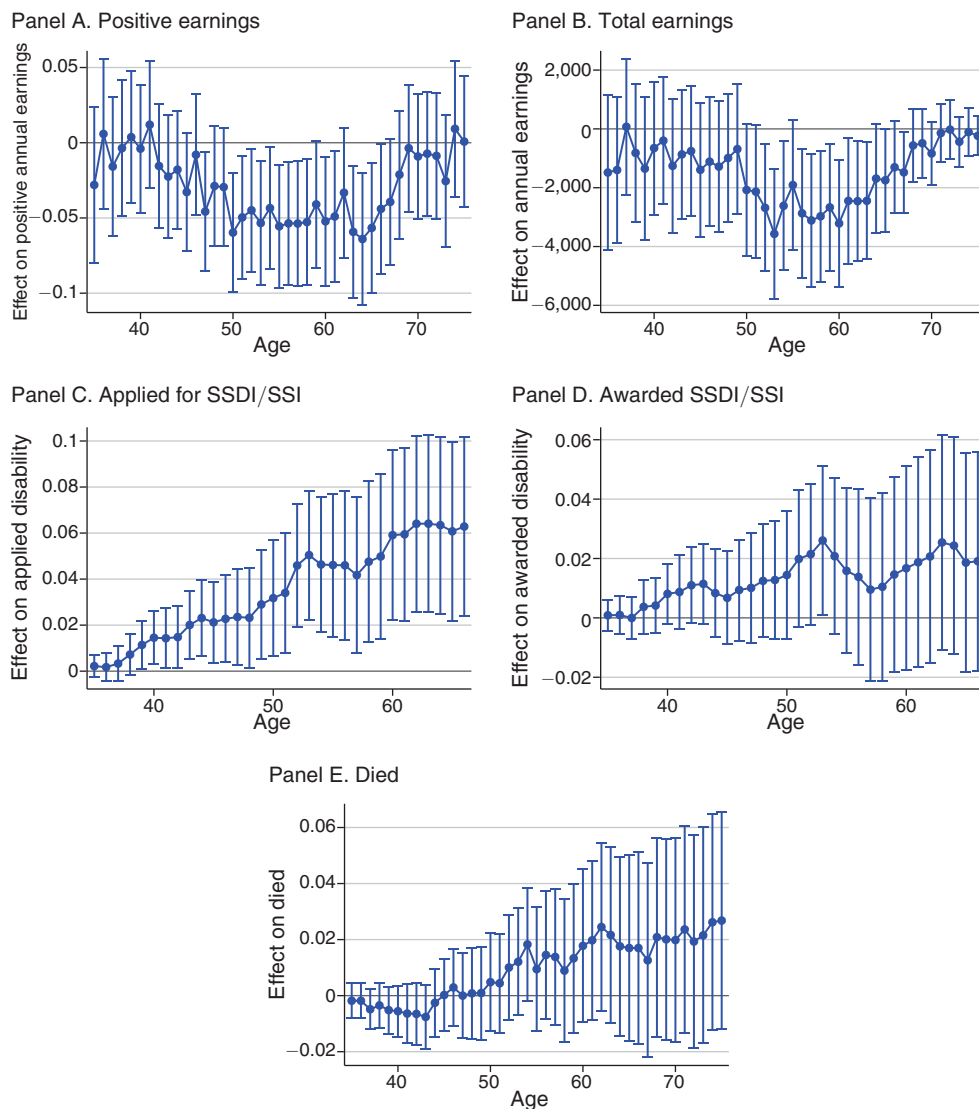


FIGURE 3. PARENTS, EFFECTS BY AGE

Notes: Each data point represents the estimate and 95 percent 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event occurred by the time indicated.

TABLE 5—PARENTS, EFFECTS ON MAIN OUTCOMES

	Positive annual earnings (1)	Annual earnings (2)	Applied disability (3)	Awarded disability (4)	Died (5)
<i>Panel A. Whole sample</i>					
Treated	−0.0329 (0.0136)	−1,761 (816)	0.0628 (0.0199)	0.0216 (0.019)	0.0138 (0.0196)
<i>Dep var summary stats</i>					
Mean	0.709	23,748	0.318	0.247	0.385
Std. dev.	0.454	25,161	0.466	0.432	0.487
Observations	52,867	52,867	2,280	2,280	2,280
People	2,252	2,252	2,280	2,280	2,280
Clusters	1,699	1,699	1,720	1,720	1,720
<i>Panel B. Denver</i>					
Treated	−0.029 (0.0184)	−1,765 (1,003)	0.056 (0.0282)	0.00972 (0.0266)	0.0275 (0.0264)
<i>Dep var summary stats</i>					
Mean	0.711	22,269	0.341	0.257	0.356
Std. dev.	0.454	23,402	0.474	0.437	0.479
Observations	31,286	31,286	1,283	1,283	1,283
People	1,276	1,276	1,283	1,283	1,283
Clusters	987	987	993	993	993

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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, panel A includes data from both cities; Table 5, panel B includes data from Denver only.

III. Outcomes for Adults

A. 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 5. (Given the importance of Denver-only results in understanding the implications of the experiment’s imbalance, we show Denver-only versions of these regressions separately; reassuringly, the results are quite similar.) In Table 6, 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 6, we see that this effect is not explained by any differential mortality: The effect is nearly identical if we

TABLE 6—PARENTS, OTHER MARGINS

	Pos. annual earnings (1)	Annual earnings (2)	Retire age (max 65) (3)	Awarded disability (4)
Condition	Alive	Earn > 0		Applied disability
Treated	−0.0327 (0.0128)	−1,030 (806)	−0.91 (0.343)	−0.0716 (0.0362)
<i>Dep var summary stats</i>				
Mean	0.74	33,514	59.4	0.727
Std. dev.	0.438	23,794	7.08	0.446
Observations	50,458	37,461	1,780	724
People	2,236	2,105	1,780	724
Clusters	1,692	1,609	1,412	651

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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. 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 disability" indicates that the individual ever applied for SSDI/SSI disability benefits.

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 percent of the participants' \$24,000 mean annual earnings (for both treated and untreated participants). As shown in column 2 of Table 6, 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 percent (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 in earnings during the experiment and \$2.40 after. (These numbers are somewhat sensitive to the discount rate, particularly because posttreatment effects are strongest later in life. However, even a 10 percent 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, Lindqvist, Notowidigdo, and Östling (2017) or Imbens, Rubin, and Sacerdote (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 percent on the 32 percent 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 6—treatment increased the chance of being rejected for disability benefits among those who did apply, suggesting that in the judgment of the SSA, those induced to apply for benefits are less deserving of those benefits than the average applicant. (This also 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.)

These results are generally quite robust to alternative specifications. Supplemental Appendix Table C.3 presents a variety of robustness checks on these results, while Supplemental Appendix D presents all results 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 Supplemental Appendix Table C.4. (This table includes subgroups analyzed at the time of the experiment, such as men, single women, and married women.) Although some differences are statistically significant, there are few systematic differences among the groups. For example, the effect on annual earned income is significantly higher (or less negative) at the 10 percent level for those in the lowest preexperimental income category (under \$14,000) than for those in the middle category (between \$14,000 and \$32,000), and the effect on whether they earned income is higher (at the 10 percent level) for the lowest preexperimental 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 represent unintended consequences of SIME/DIME: It is unlikely that policymakers would have intended for treatment to cause adults to work less in the long run or apply more for disability benefits. These results may also 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 1, panel B. 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 2 plots the effect on each main outcome by years since the end of the experiment, while Figure 3 plots the effect at each age. Although there is little clear trend in how effects vary by time after treatment, effects on earnings are strongest between approximately ages 50 and 65, which corresponds to the time when most people leave the labor force and retire. It may be surprising that adults in our sample could retire so early, especially given that many of them had jobs that might make it difficult to save for retirement. However, they do indeed retire that early: By age 61 (before the SSA's

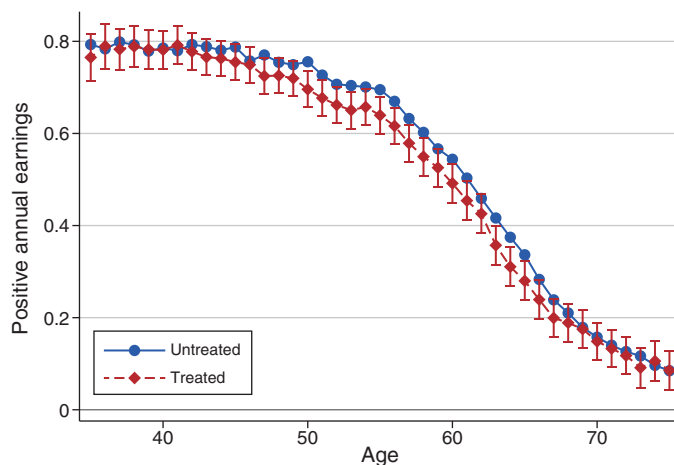


FIGURE 4. PARENTS' POSITIVE EARNINGS AT DIFFERENT AGES

Notes: Each data point represents the estimate and 95 percent 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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. “Untreated” is average outcome minus half of treatment effect; “Treated” is average plus half of treatment effect.

early retirement age), only 48 percent of adults in our sample work—a decline from the 79 percent of adults who worked at age 37. That the effect we find is driven by early retirement is also shown graphically in Figure 4, which shows the fraction working at each age, plus and minus half of the treatment effect; treatment shifts this curve to the left. Indeed, as shown in column 3 of Table 6, treatment causes workers to retire almost a year earlier. (No retirement effect was found in the original experiment, because only 12 percent of adults were 50 or over by the fifth year of the experiment.)

Note that although these results may be surprising, they do not contradict previous research on cash assistance, because no previous research has analyzed such long-run effects. If we examined only effects in the five to ten years after the program ended—as previous studies have done—we likely would not have found any significant effects. Indeed, it might be expected that effects on leisure would be strongest on the retirement margin. Any persistent shock to propensity to work would have the strongest effect when people are close to the margin between working and not. For many individuals, that occurs around retirement age for a variety of reasons—reduced need for precautionary savings or human capital accumulation, declining health, and so on. Indeed, in a related result, Imbens, Rubin, and Sacerdote (2001) find that winning the lottery leads to the largest declines in earnings for those close to retirement age (though these effects occur soon after winning the lottery, and with smaller effect sizes).

An important issue with these results concerns external validity: How would the results be different if the treatment had been different? For some variations on the

treatment, we cannot know the answer. For example, SIME/DIME treated only a few thousand of the millions of people living in their metropolitan areas. If more had been treated, we might expect general equilibrium effects to occur, such as wages rising as individuals work less; these effects could have long-run consequences. Given the scale of this experiment, though, such an analysis is beyond the scope of this paper.

However, different SIME/DIME treated families did receive somewhat different treatments. Families' treatments could vary in the length of treatment (three or five years), their guarantee level (the money received if no income is earned), the tax rate when \$0 is earned (either 50 percent, 70 percent, or 80 percent), whether that tax rate declined as more money is earned, and whether they were in the manpower treatment (discussed in footnote 8). Table 7 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 five-year treatment experienced a stronger effect on disability applications, suggesting that longer-term treatments have stronger effects. (Supplemental Appendix Figure C.9 shows the pattern of this difference by age.) A priori, we might also expect that the high tax rate—rather than the cash assistance itself—has 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, and in fact it may have caused them to earn more.¹⁵ 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 percent 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 7. Regardless, these results provide some suggestive evidence.

Results in Table 7 could also, in theory, allow us to measure long-term income and substitution effects by measuring effects of the guarantee level and tax rate separately. (The substitution effect would likely operate indirectly—for example, by incentivizing adults to have previously worked less.) However, the large standard errors again preclude us from making such a contribution here.

In addition to the five main variables discussed above, the richness of the SSA and WA DOH data allows us to study several other outcomes; see Supplemental Appendix Table C.2. These results show that the effect on disability applications acts through both the SSDI and SSI programs. Applications increased most for musculoskeletal disorders, though most other types of impairments increased insignificantly. 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

¹⁵ This may be because a higher initial tax rate reduces the break-even level—the amount of earnings at which the family stops receiving benefits. Once a family stops receiving benefits, their tax rate goes down to what they would receive absent the program. Thus, counterintuitively, for some families, a higher initial tax rate leads to a lower effective tax rate.

TABLE 7—PARENTS, DIFFERENT TREATMENTS

	Positive annual earnings (1)	Annual earnings (2)	Applied disability (3)	Awarded disability (4)	Died (5)
Treated	−0.0331 (0.0137)	−1,825 (818)	0.0615 (0.02)	0.0182 (0.019)	0.0125 (0.0197)
5-year trtmnt	−0.00882 (0.0187)	−52.5 (1,039)	0.0786 (0.0285)	0.0455 (0.0263)	0.00453 (0.0263)
Guar level	−4.45e−08 (2.97e−06)	0.216 (0.157)	2.70e−08 (4.51e−06)	7.63e−06 (4.31e−06)	4.09e−06 (4.01e−06)
Tax rate, \$0	−0.0026 (0.109)	1,622 (6,071)	−0.276 (0.168)	−0.364 (0.156)	−0.106 (0.157)
Tax decline?	−0.0227 (0.0251)	−1,594 (1,350)	0.0898 (0.0382)	0.0508 (0.0358)	0.0206 (0.0358)
Manpower	0.01 (0.0134)	−11.7 (795)	−0.00549 (0.02)	−0.00064 (0.019)	0.0123 (0.0193)
<i>Dep var summary stats</i>					
Mean	0.709	23,748	0.318	0.247	0.385
Std. dev.	0.454	25,161	0.466	0.432	0.487
Observations	52,867	52,867	2,280	2,280	2,280
People	2,252	2,252	2,280	2,280	2,280
Clusters	1,699	1,699	1,720	1,720	1,720

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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 five years as opposed to three 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 de-meaned, 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.

overall level of such income was so low, and see that annual earned income generally declined by several different measures.

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

B. 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. We first find evidence against effects being driven by wealth accumulation, changes in preferences or beliefs, or changes to hourly wages. Instead, we show evidence suggesting that effects operated through changes in human capital and job quality. (Details on procedures used in this subsection are provided in Supplemental Appendix B.2.)

Although this mechanism seems to be the most promising explanation given the data available, this evidence is not dispositive. There are other mechanisms that we cannot observe but could be driving the results—for example, changes in preferences that are not measured. (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, as discussed above and shown in Supplemental Appendix Table C.4.) For this reason, evidence in this subsection is presented as suggestive of promising avenues for future research.

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 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 three 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, Lindqvist, Notowidigdo, and Östling (2017) 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 ten years of posttreatment data). Further, we have evidence from contemporaneous surveys that there was no change in assets; see Table 8, which investigates several potential mechanisms.¹⁶ 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. Treatment also increased the probability that adults received any government benefits by 22 percentage points.¹⁷ Thus, 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

¹⁶To maximize sample size, Table 8, along with Table 9 discussed below, uses data from all SIME/DIME families with at least two children, not just matched families.

¹⁷This includes any government benefits in all SIME/DIME records. Restricting the analysis to income-dependent cash assistance (that is, AFDC, unemployment benefits, and SIME/DIME payments) increases the effect to 34 percentage points; restricting to only AFDC or SIME/DIME payments increases the effect to 48 percentage points.

TABLE 8—TESTS FOR WEALTH, PREFERENCES, AND WAGES

	Change assets year 1–3 (1)	General female attitude to work (2)	General male attitude to work (3)	Only government benefits if can't support self (4)	Any earnings year 4 (5)	log wage rate year 4 (6)
Treated	–1,691 (2,148)	–0.0198 (0.0359)	0.0824 (0.0475)	0.0378 (0.0268)	–0.00896 (0.015)	0.0199 (0.0242)
<i>Dep var summary stats</i>						
Mean	5,723	0.0324	0.1	2.22	0.728	2.57
Std. dev.	41,115	0.999	0.928	0.656	0.445	0.656
Observations	2,691	3,047	1,696	2,632	3,247	2,279
People	2,691	3,047	1,696	2,632	3,247	2,279
Clusters	1,753	3,041	1,696	2,511	2,213	1,761

Notes: Based on public SIME/DIME data for original families with at least two children. “Change assets year 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. “Only government benefits 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 hourly 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 five-year treatment group.) Data on assets and survey variables are described in more detail in Supplemental Appendix B.2.

have no data on postexperimental welfare (AFDC or TANF), food stamps, or other programs.) For each of these mechanisms, as with the wealth channel, we may not observe effects on earnings immediately after the experiment because individuals at that time are not close to the margin of retirement. Although survey elicitation 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 8 (based on all SIME/DIME adults with at least two children). There is no evidence that treatment increased the value of leisure or reduced 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). These measures are described in more detail in Supplemental Appendix B.2. Note that most of the questions about male and female attitudes on work ask about gender norms, and all questions were asked around the time of the experiment; it’s possible that there was an effect on preferences or beliefs that weren’t measured here or materialized well after the experiments ended.

A third potential channel for the effects on adults would be through hourly 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 demonstrated by Kroft, Lange, and Notowidigdo (2013) and others. 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, LaLonde, and Sullivan (1993) and others that involuntary separations can lower earnings in the long term. In this case, however, the nonworking time was *voluntarily* chosen, as treated individuals could have behaved as if they did not receive treatment.

Based on original data from the experiments, though, we see no significant effect on posttreatment hourly wages (calculated as earnings divided by hours worked). Column 5 of Table 8 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 hourly 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.

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. 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. (We present evidence in favor of this theory below.) Although we do not have occupation in our administrative data, we do have it in public data for control adults and adults in the three-year treatment from immediately before and after treatment; we use that data to test this mechanism in Table 9.¹⁸ (See Supplemental Appendix B.2 for more details.)

Indeed, treatment did cause individuals to change what kind of occupations they worked in. Column 1 of Table 9, panel A (based on all SIME/DIME adults with at least two children) shows that treatment caused a switch to occupations requiring less education, suggesting that they lost human capital in their time out of work. These new occupations did not pay significantly lower earnings (column 2). They also weren't more likely to be declining as the workers aged (column 3).¹⁹ Instead, the new occupations mainly differed in their need for cognitive skills (which could decline due to time out of work) and, particularly, aspects of the occupation that are related to job satisfaction. New occupations were less prestigious (column 4), required less specific vocational preparation (column 1 of Table 9, panel B), less mental skill (column 2), and a temperament less attuned to creativity than repetition (column 3). Other factors

¹⁸It 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. Occupations also allow us to examine data from the Dictionary of Occupational Titles and other sources. However, we examine effects on industry, as well as occupation/industry combinations, in Supplemental Appendix Tables C.8 and C.9.

¹⁹Note that in the Denver-only data, there is somewhat more support for the hypothesis that adults switched to declining occupations requiring more routine tasks and physical demands; see Supplemental Appendix Table D.6. Such an effect could have also driven the increase in disability applications for musculoskeletal disorders. In either case, though, a change in the nonpecuniary aspects of the occupation appears to have led to long-term effects; as noted below, more research is needed to verify how this might work in other contexts.

TABLE 9—TESTS FOR CHANGES IN OCCUPATION

	Occupation average education (1)	Occupation average annual earnings (2)	Occupation change employment 1970–1990 (3)	Prestige (4)	Occupation principal component (5)	
<i>Panel A</i>						
Treated	−0.12 (0.061)	−528 (509)	−0.043 (0.0366)	−1.1 (0.442)	−0.102 (0.0353)	
<i>Dep var summary stats</i>						
Mean	10.9	25,971	0.601	36.8	−0.401	
Std. dev.	1.49	13,360	0.855	10.6	0.839	
Observations	2,482	2,482	2,482	2,434	2,411	
People	2,482	2,482	2,482	2,434	2,411	
Clusters	1,902	1,902	1,902	1,872	1,857	
	Specific vocational prep (1)	Mental skills (2)	Creative Temper (3)	Physical demands (4)	Perception/ physical skills (5)	Environment conditions (6)
<i>Panel B</i>						
Treated	−0.078 (0.0312)	−0.084 (0.0317)	−0.0773 (0.0314)	0.0394 (0.0303)	−0.0158 (0.0301)	0.0292 (0.0296)
<i>Dep var summary stats</i>						
Mean	−0.301	−0.361	−0.29	0.157	−0.0699	0.0896
Std. dev.	0.765	0.754	0.752	0.787	0.721	0.734
Observations	2,467	2,467	2,467	2,467	2,467	2,467
People	2,467	2,467	2,467	2,467	2,467	2,467
Clusters	1,890	1,890	1,890	1,890	1,890	1,890

Notes: 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. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental earned income, sex, and a cubic polynomial of date of birth. All variables are restricted to individuals who were not in the five-year treatment group. Variables are based on the final occupation observed in the data in at least the fourth year after the experiment began. “Occupation average education” measures the average education of the occupation in 1970 census data. “Occupation average annual earnings” measures annual earnings in the 1990 census data in the given occupation. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. “Occupation change employment 1970–1990” measures the log change in total employment in that occupation between 1970 and 1990 census data. Census data are from Ruggles et al. (2017). “Prestige” is from the 1989 General Social Survey, via Davis et al. (2006). Columns in panel B refer to “Specific vocational preparation,” “Mental skills,” “Creative temperament,” “Physical demands,” “Perception/physical Skills,” and “Environmental conditions,” all based on data from the Dictionary of Occupational Titles, via National Academy of Sciences, Committee on Occupational Classification and Analysis (2006).

less related to cognitive skills—such as the physical demands of the occupation (for example, strength—column 4), perceptual or physical skills needed (for example, finger dexterity—column 5), and environmental conditions (for example, hazards—column 6) were not significantly affected. Finally, column 5 of Table 9, panel A estimates the effect on the principal component of all these measures (which is positively correlated with mental skills and education), finding a strongly significant effect. Taken together, this evidence suggests that workers lost human capital during their time out of work.

More than half of adults were out of work or had wages under 1.5 times the federal minimum wage before the experiment began. Consistent with the occupational results being driven by workers who cannot get a lower-wage job due to the minimum wage (as discussed above), the effect on occupations is substantially stronger for these workers who were already unlikely to earn high wages. See Supplemental Appendix Table C.10 for the low-wage group and Supplemental Appendix Table C.11 for the remainder of the sample.

To fix ideas—more, perhaps, as anecdote than data—the major occupation caused by treatment to decline most was social work; the occupation caused to increase most (albeit insignificantly) was sewers and stitchers. Although both occupations offer low pay, social work might be a more interesting and fulfilling occupation that workers would want to continue doing as they age.

Alternatively, although earning less money is often framed as a negative outcome, it is possible that the decision to retire early—and, perhaps, to have a less prestigious or intellectually stimulating job—is something that treated workers welcome. This may be particularly true if time out of work for treated adults changed preferences in ways that the preference surveys couldn't measure, as discussed above. Indeed, following the anecdote above, social work may be more stressful than sewing, and those who took time out of work might prefer a more restful career. More research is needed to understand why treated workers change jobs and retire in order to know whether this is a positive or negative outcome.

Although the evidence presented here on changes in occupation is suggestive, it is not definitive. Instead, it suggests a potentially promising avenue for future research: analyzing occupational changes, including their nonpecuniary characteristics, after shocks to workers. Such analysis might find effects that monetary measures miss, including factors that could predict long-term consequences. If similar results appear in other settings, it suggests 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 on careers that are not just 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. Unemployment benefits could also be structured to encourage workers to go back to work in an occupation where the worker can succeed in the long run, which may be the worker's original occupation or a different one.

IV. Outcomes for Children

A. Results

There are several reasons that we might expect to see substantial effects on children's long-term outcomes. Treatment involves a substantial (though temporary) increase in income for many low- to middle-income families; additional resources could lead to significant gains for children. Concurrently, because treatment caused adults to work less, they might have more time to spend with their children (which could benefit the children). The additional time out of work might also model nonworking behavior, and additional government support might change the way

children view it (which could cause children to work less). Given these countervailing forces, the overall effect is theoretically ambiguous.

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 10. (We again include Denver-only results separately.) There are no significant effects on any of these outcomes. Based on the 95 percent 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 percent 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 Supplemental Appendix Table C.6, point estimates are similar under alternative specifications, and under no alternative tested is any estimate significantly different from zero. Effects are also generally insignificantly different from zero on an annual basis, as shown in Supplemental Appendix Figures C.4 and C.5. Additionally, as shown in Table 11, there are no effects on several other margins.

Estimated effects on a variety of other variables are shown in Supplemental Appendix Table C.5. 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, an important finding given the concerns about the effects on marital stability after Groeneveld, Hannan, and Tuma (1983). There is 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 in Section IVB. Despite these null results, it is important to note that the treatment may affect other child outcomes that we cannot measure, such as physical or mental health, happiness and life satisfaction, or civic engagement; unfortunately, examining these effects is work for future research. It is also possible 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 12 shows that children in the five-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. It is also suggestive of information transfer about disability benefits between adults and children, because effects were strongest for the same five-year treatment group on adults' applications for SSDI/SSI—and, marginally significantly, their receipt of that benefit. (We discuss information transfer further below.) Results for various subgroups of the population are shown in Supplemental Appendix Table C.7. In general, few significant effects are found for

TABLE 10—CHILDREN, EFFECTS ON MAIN OUTCOMES

	Positive annual earnings (1)	Annual earnings (2)	Applied disability (3)	Awarded disability (4)
<i>Panel A. Whole sample</i>				
Treated	0.00177 (0.00872)	−356 (601)	0.00537 (0.0125)	0.0018 (0.00962)
<i>Dep var summary stats</i>				
Mean	0.769	22,281	0.245	0.13
Std. dev.	0.422	24,384	0.43	0.336
Observations	163,340	163,340	5,658	5,658
People	5,658	5,658	5,658	5,658
Clusters	2,101	2,101	2,101	2,101
<i>Panel B. Denver only</i>				
Treated	0.00588 (0.0111)	−428 (768)	−0.00519 (0.0159)	−0.00461 (0.0115)
<i>Dep var summary stats</i>				
Mean	0.789	21,804	0.226	0.112
Std. dev.	0.408	23,297	0.419	0.315
Observations	92,789	92,789	3,273	3,273
People	3,273	3,273	3,273	3,273
Clusters	1,208	1,208	1,208	1,208

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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, panel A includes data from both cities; Table 10, panel B includes data from Denver only.

any subgroup. One subgroup of particular interest, though, is the youngest children; many believe that early-life interventions may be most influential.²⁰ 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. We also find no significant effects on any outcome for any other age group.

B. 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

²⁰ Research supporting the idea that early interventions are most useful includes Cascio and Schanzenbach (2013); Cunha, Heckman, Lochner, and Masterov (2006); Cunha, Heckman, and Schennach (2010); Duncan, Ludwig, and Magnuson (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).

TABLE 11—CHILDREN, OTHER MARGINS

Condition	Pos. annual earnings	Annual earnings	Awarded disability
	(1)	(2)	(3)
	Alive	Earn > 0	Applied disability
Treated	0.00541 (0.00815)	−492 (570)	−0.000502 (0.0295)
<i>Dep var summary stats</i>			
Mean	0.79	28,991	0.511
Std. dev.	0.408	24,064	0.5
Observations	158,763	125,530	1,385
People	5,636	5,556	1,385
Clusters	2,101	2,097	976

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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. 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 disability” indicates that the individual ever applied for SSDI/SSI disability benefits.

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, Kostøl, and Mogstad (2014) find that parental receipt of Norwegian disability insurance (DI) causes an approximately 200 percent increase in a child’s later receipt of DI (compared to the mean rate of receipt); Dahl, Kostøl, and Mogstad (2014) are also able to rule out effects on another type of government assistance of more than 100 percent.

SIME/DIME treatment increased the probability that a family receives government benefits by at least 22 percentage points (see footnote 17; larger effects on receipt of benefits would imply even more tightly estimated null effects on children). Thus, inflating estimates and standard errors in Table 10 by $1/0.22$ as a rough estimate, we can rule out (at the 5 percent level) that receipt of such cash assistance increased SSDI or SSI applications by more than 56 percent (of the mean application rate), and rule out increases in SSDI/SSI receipt of more than 73 percent. One possible reason that we do not find significant effects is mentioned by Dahl, Kostøl, and Mogstad (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

TABLE 12—CHILDREN, DIFFERENT TREATMENTS

	Positive annual earnings (1)	Annual earnings (2)	Applied disability (3)	Awarded disability (4)
Treated	0.00186 (0.00874)	−339 (601)	0.00522 (0.0125)	0.00221 (0.00965)
5-year trtmnt	−0.0339 (0.0122)	−1,276 (816)	0.0392 (0.0178)	0.0349 (0.013)
Guar level	−8.02e−08 (1.87e−06)	−0.0767 (0.124)	−4.03e−07 (2.72e−06)	−1.70e−06 (1.94e−06)
Tax rate, \$0	−0.0167 (0.0712)	−1,244 (4,801)	−0.0581 (0.103)	0.0236 (0.0753)
Tax decline?	0.00456 (0.0162)	368 (1,074)	0.0426 (0.023)	0.0277 (0.0172)
Manpower	−0.0147 (0.00854)	−710 (587)	0.00537 (0.0124)	−0.00604 (0.00965)
<i>Dep var summary stats</i>				
Mean	0.769	22,281	0.245	0.13
Std. dev.	0.422	24,384	0.43	0.336
Observations	163,340	163,340	5,658	5,658
People	5,658	5,658	5,658	5,658
Clusters	2,101	2,101	2,101	2,101

Notes: 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 preexperimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, preexperimental 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 five years as opposed to three 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 de-meaned, 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.

the null overall 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. (As discussed above, the fact that effects on both child and adult SSDI/SSI are strongest for the five-year treatment group is also suggestive of the importance of information.) 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, Eli, Ferrie, and Lleras-Muney (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.²¹ They estimate that receipt of MP benefits increases a child's later income by at least 14 percent, whereas we can rule out treatment increasing earned income by more than 3.7 percent. This difference may occur because the SIME/DIME treatment discouraged parental work, whereas Aizer, Eli, Ferrie, and Lleras-Muney (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 have been 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.

V. Discussion and Conclusion

The IMEs 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 treated, this cash assistance caused earned income for treated adults to be \$1,800 lower per year and increased the probability that 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 could lower their human capital and cause them to get worse jobs when they return. These worse jobs could have led them to choose to retire early.

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

²¹ Aizer, Eli, Ferrie, and Lleras-Muney (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.

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. Future research should particularly focus on the way that policies can change the occupations that workers choose—and the long-term implications of that choice—to understand whether the suggestive results we find are robust and broadly applicable. 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.

Policymakers often worry about the effect of cash assistance on contemporaneous labor supply, or believe that effects on children—positive or negative—outweigh any impact on parents. Our results indicate that, instead, policymakers may need to pay more attention to long-run effects on adults, which in this context are larger than any contemporaneous or intergenerational effects. Indeed, in SIME/DIME, assistance does not cause large observable benefits for children and may lead to unintended consequences for adults. Given the mechanisms discussed above, this research suggests a new reason why policymakers should be wary of programs (such as unemployment insurance) that cause a temporary drop in labor supply, after which adults might need to call upon their deteriorated human capital. 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.

Poverty and other aspects of low socioeconomic status often last a lifetime and are passed down from parent to child. Guaranteeing a minimum income above the poverty line ensures that a family is not in poverty while the guarantee is in place, but such a guarantee, by itself, may not be a panacea to break the cycle of poverty or reduce inequality in a sustainable way.

APPENDIX A. CREATING THE SAMPLE OF SIME/DIME PARTICIPANTS

A1. Finding Families with Matching Patterns

To the best of our knowledge, no traditional identifying information such as names or SSNs exists from SIME/DIME or the other IMEs. However, public-use SIME/DIME data include date of birth, sex, and relationship to household head for each family member in the experiment. From these data, we can find patterns that identify families. For example, suppose that a Seattle single mother born on 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 (Washington State Department of Health 2015) and restricted-use data on parent names—for both the mother and father—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.²² So to continue our example from above, we would look in WA DOH birth records for all boys born on February 1, 1960, to 30-year-old mothers; boys born on February 1, 1961, to 31-year-old mothers; and boys born on February 1, 1962, 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 that the three boys found in this way are her three children. Where necessary, we then match these individuals to SSNs.²³

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.²⁴ Crucially, though, as discussed in Appendix Section A2, this method allows us to estimate the probability that each match corresponds to an actual SIME/DIME family, thus minimizing spurious matches.

A2. Determining Which Matching Families Were in SIME/DIME

Not everyone provisionally matched using the procedure discussed in Appendix Section A1 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 the probability that each family is actually a SIME/DIME family, we create a model of the matching procedure and estimate its parameters via maximum likelihood estimation (MLE). The model includes these two channels; crucially, while the actual families with similar characteristics will not vary by name frequency, the probability that unrelated individuals have parents who share a name will vary by parent name frequency, which allows us to separately identify each channel. By doing so, we can find a name frequency threshold at which—if we exclude all those with more frequent names—we can exclude most spurious matches. Note that this model does not capture all features of the match process; however, as discussed in Appendix Section A3, 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

²²For 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).

²³Parent 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.

²⁴Individuals 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.

births matches the pattern of the IME family (where α may depend on dataset used but is assumed not to depend on name frequency).

Now, consider unrelated individuals with matching birthdays whose parents share a name. Suppose that the match is based on N children and that 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 22). For a specific parent name that occurs with frequency f ,²⁵ we would expect that name to appear $n_i f$ times. Assuming independence, the expected number of matches with that name would be approximately $\prod_i (n_i f)$. Assuming that the number of possible matches is approximately the same regardless of date of birth and sex (but can vary by dataset used and state), we can approximate $\prod_i (n_i f) \approx f^N n^N$. Thus, from the true match probability and the two spurious match channels, for a specific name with frequency f , we expect to find $\tau f + \alpha \tau f + f^N n^N$ 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.²⁶ (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 $T(\alpha \tau f + f^N n^N)$ matches. Thus, putting placebos together with matches using the true birthdays, the fraction found using the true birthday would be

$$(A1) \quad \text{FracSample} = \frac{\tau f + \alpha \tau f + f^N n^N}{\tau f + \alpha \tau f + f^N n^N + T(\alpha \tau f + f^N n^N)} \\ = \frac{1 + \alpha + \beta f^{N-1}}{1 + (T + 1)(\alpha + \beta f^{N-1})},$$

where $\beta \equiv \frac{n^N}{\tau}$. 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.²⁷ From this, we calculate the probability that each match is a SIME/DIME family as

$$(A2) \quad \text{ProbSIME/DIME} = \frac{1}{1 + \alpha + \beta f^{N-1}}.$$

²⁵ 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 United States as a whole, or in the state of interest.

²⁶ 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 on 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.)

²⁷ 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 dataset (SSA and WA DOH birth 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.

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 that 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 A1, this procedure predicts probabilities out of sample well.

A3. Matching Results

In our main sample, we only include matches if we calculate that they are true SIME/DIME participants with at least 95 percent probability. (We also show robustness checks with 75 percent and 99 percent probability thresholds.) With this threshold, 45 percent of parents and 59 percent of children in SIME/DIME are matched to an SSN.²⁸ Importantly, as shown in Table 3, panel A, treatment is not significantly correlated with match probability, after controlling for assignment group, overall or within various subgroups of the population. Supplemental Appendix Tables C.4 and C.7 show that treatment is also not correlated with match probability in other subgroups; see column 1 of each table, which is equivalent to results in Table 3, panel A, but within a wider variety of subgroups.) Table 3, panel B shows that different treatment arms do not differentially affect match rate. 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 Appendix Section A4.

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 percent of adults and 0.45 percent of children are matched to multiple SSNs.²⁹ We can use a simple model to show that this duplicate 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

²⁸ 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.

²⁹ 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 situations where one IME participant is matched to more than two SSNs.

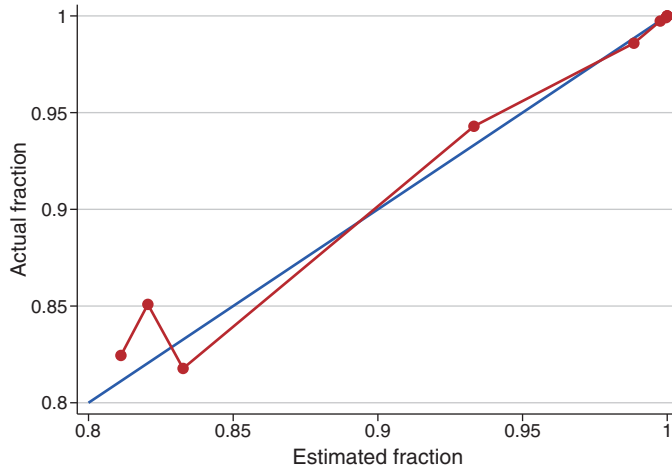


FIGURE A1. CROSS VALIDATION OF MLE PREDICTIONS

Notes: Outcomes based on SSA data. Families are randomized into two groups; MLE parameters are estimated with one group, and probability of being in the nonplacebo 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 percent 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).

spurious matches is just the duplicate rate divided by the match rate squared. From this, we estimate that only 5.2 percent of matched adults and 1.3 percent 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 (rather than the less strict two-family restriction that we impose in this paper), we match only 24 percent of this smaller set of parents (of whom 37 percent are matched spuriously) and 41 percent of the smaller set of children (of whom 13 percent 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 were not used in the match. Unfortunately, there are no variables common to the two datasets. However, we do know an individual's race from SIME/DIME data and their 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 $P(\text{name} = N | \text{race} = R)$ that a SIME/DIME participant of race R has name N is the same as in the general population. For common names, $P(N|R)$ is available from the

census,³⁰ so we can match that with the names of participants we find using the SSA Numident file. Using Bayes' theorem,

$$(A3) \quad P(R|N) = \frac{P(N|R)P(R)}{\sum_r P(N|race = r)P(race = r)},$$

where $P(race = r)$ 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 (A3) may not hold perfectly, the estimated probability does predict actual race quite well; see Figure A2.

A4. 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 that 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 zero children or one 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.³¹ 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 20 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.

³⁰ See <https://www2.census.gov/topics/genealogy/2000surnames/names.zip>.

³¹ 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.

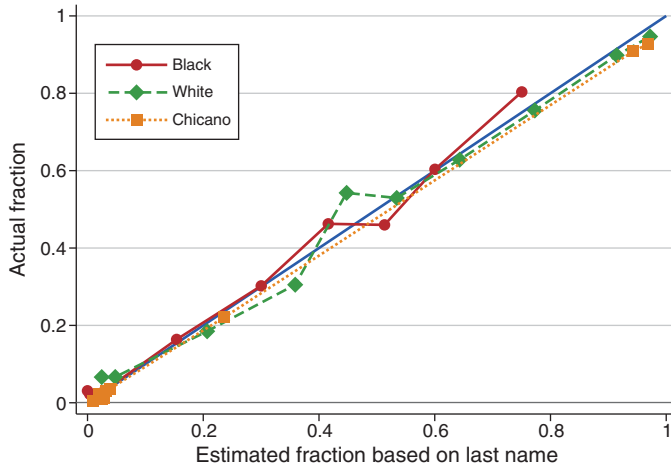


FIGURE A2. CORRESPONDENCE BETWEEN SSA AND SIME/DIME DATA

Notes: Outcomes based on SSA data. 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 $P(R|N) = \frac{P(N|R)P(R)}{\sum_r P(N|race=r)P(race=r)}$, where, for any name n and race r , $P(n|r)$ is based on 2000 census data on last names and race (Black, White, or Hispanic), while $P(r)$ 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.

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 Supplemental Appendix Tables C.3 and C.6, 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 these data allows us to match only about 1 percent of participants. However, it is unlikely that this channel will affect our results, because almost all children likely got an SSN at 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.

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