The Long-Term Effects of Cash Assistance*

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Abstract

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

JEL Codes: I14, I32, I38, J22

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1 Introduction

Forty-three million people live in poverty in the United States,[1] and over 700 million more live in extreme poverty around the world.[2] A widely-used strategy to combat this poverty, and perhaps the simplest, is to give poor families cash to buy the goods they need.

Every year, tens of billions of dollars are given to families in the United States as cash assistance. This includes about $30 billion through Temporary Aid to Needy Families (TANF),[3] $30 to $140 billion through unemployment insurance,[4] and billions more from other sources. Recently, the idea of increasing the role of cash assistance in poverty reduction is attracting interest, with proposals for policies or experiments on cash assistance in Canada,[5] Kenya,[6] the United States,[7] and other countries. Despite the importance policymakers attach to cash assistance, little is known about the effect of this assistance on outcomes for beneficiaries or their children in the decades after the assistance is received. Identifying these long-term causal effects is difficult largely because families who receive cash assistance generally differ from those who do not, so no control group can easily be identified.

We overcome this difficulty, investigating the long-term effect of cash assistance on future earned income, further government financial assistance (Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI)), mortality, marriage, and divorce. We are able to identify these effects by following up, after four decades, with participants in the Seattle-Denver Income Maintenance Experiment (SIME/DIME), which began in 1970. This experiment, described in more detail in Section **??**, guaranteed a minimum annual income of up to $25,900[8] to about half of the 4,800 low- to middle-income families enrolled. Treated families, randomly chosen from among all enrolled families, received the full guaranteed income if they earned no outside income; they then faced taxes of 50% to 80% on outside income, up to the point where the program no longer benefited them. Treated families received this financial guarantee for three or five years, and treatment enabled an individual to receive, on average, $2,400 extra annually in government benefits during

[1] See http://www.census.gov/content/dam/Census/library/publications/2016/demo/p60-256.pdf.
[2] See https://openknowledge.worldbank.org/bitstream/handle/10986/25078/9781464809583.pdf.
[3] See http://www.cbpp.org/sites/default/files/atoms/files/7-22-10tanf2.pdf.
[4] See https://fred.stlouisfed.org/series/W825RC1A027NBEA.
[5] See https://news.ontario.ca/mcss/en/2016/06/ontario-moving-forward-with-basic-income-pilot.html.
[6] Seehttps://www.givedirectly.org/basic-income.
[7] See https://blog.ycombinator.com/basic-income.
[8] Unless otherwise noted, all dollar values in this paper are adjusted for inflation to 2013 dollars using the personal consumption expenditures (PCE) deflator available at https://research.stlouisfed.org/fred2/series/PCEPI/downloaddata?cid=21.

the experiment, compared to control individuals who did not receive any SIME/DIME guarantee.[9]

SIME/DIME and previous smaller Income Maintenance Experiments (IMEs) were originally proposed to determine how a negative income tax (NIT)[10] would affect labor supply and other outcomes. The IMEs were large undertakings—according to **?**, SIME/DIME alone cost about $275 million—and short-term effects have been studied extensively.[11] As discussed by **?**, treatment caused adults to work about 10 to 20 percent fewer hours during the experiment. They also earned about $1,800 less per year. However, treated adults observed in the two years after the experiment ended did not work significantly different hours than control adults, or earn significantly different incomes.[12]

Although short-term effects have been previously analyzed, no outcomes were measured after the 1970s. Since the IMEs were the first large-scale randomized controlled trials (RCTs) in the social sciences, they offer a unique opportunity to evaluate the long-term effects of a large intervention on participants. Adults from SIME/DIME have now had four decades for effects from the experiment to accumulate or taper off, so any long-term effects are likely to have already occurred. And SIME/DIME children, now adults,[13] have had many of the adult experiences that define their lives.

We are able to determine long-term outcomes by combining SIME/DIME data with administrative records from the Social Security Administration (SSA) and the Washington State Department of Health (WA DOH). Our main outcome measures come from SSA data, which includes annual earned income between 1978 and 2013; information on applications for, and awards of, disability benefits; and mortality. Described in more detail in Section **??**, the administrative data allows us

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

[10]Note that the "negative" in "negative income tax" refers to the tax level: individuals on an NIT gain money rather than losing it. It does not refer to a negative tax *rate*, as experienced by some EITC beneficiaries; indeed, the SIME/DIME tax rate was quite high.

[11]A review of the hundreds of papers discussing the original results from the IMEs is beyond the scope of this paper. Some results, as they relate to the present paper, are discussed throughout, particularly in Section **??**. The most detailed discussion of the original results from SIME/DIME can be found in the final report by **?**, with similar results in *The Journal of Human Resources*, Vol. 15, No. 4 and an overview in **?**. A bibliography of contemporary papers from the IMEs is available at `http://www.irp.wisc.edu/research/nit/NIT_index.htm`. Proceedings from a conference on the IMEs are available in **?**. **?** reviews the literature on the IMEs, including literature from after the 1980s.

[12]Some care must be taken in interpreting the original results, 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 **?** and **?** for misreporting, and by **?** and **?** for attrition. However, underreporting in particular did seem to bias the results toward a stronger labor supply effect.

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

to determine outcomes for participants even after they are no longer being surveyed. The use of administrative data also reduces the chance that results are biased by differential misreporting or attrition.

To our knowledge, no data set exists today that includes SIME/DIME participants' names, but a publicly-available data set does include each participant's birthday, sex, and relationships to other participants. This data is used to identify participants by finding unique patterns in the birth records of multi-child families, including public data from the WA DOH and restricted-use data on parent names from the SSA.[14] Using this method, we are able to match about half of SIME/DIME participants from multi-child families with a high degree of certainty. This matching technique is discussed in more detail in Appendix **??**.

Using this matched data set, we find new evidence of significant effects on adults decades after the experiment ended. On average, treatment decreased the probability that participants work in a given year by 3.3 percentage points (4.6% of the mean probability of working for adults in our sample), and decreased average annual earnings by $1,800 (7.4% of mean annual earnings). Treated adults were also 6.3 percentage points (20% of the mean) more likely to apply for disability benefits (either SSDI or SSI), but were not significantly more likely to be awarded benefits, or to have died. These effects for adults, described in more detail in Section **??**, are large relative to the cash assistance received: for every $1 in additional government transfers, we find that individuals earn discounted lifetime earnings that are $4.50 lower. The effects are mainly evident later in individuals' lives: for example, effects on annual earnings are $2,000 stronger between ages 50 and 60 than before that age.

Combining these results with those originally gathered during the experiment shows a pattern that may be surprising. Treatment decreases earned income during the experiment, causes no effect immediately thereafter, but again decreases earnings much later in life. However, because individuals generally choose to consume more leisure at the end of their lives, it is not surprising that they would consume additional leisure later in life, too. To formalize this idea, Subsection **??** explores a simple model, derived from that developed by **?**, in which agents' wage rate is endogenously determined by past hours of work in a learning-by-doing setup. In this model, we note that SIME/DIME could affect long-run earnings through three channels: it could affect wealth, if agents saved some of the transfer; wage rate if they worked less, lowering their human capital (or,

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

as in **?**, their inferred human capital); or preferences (if leisure is habit-forming, or through other mechanisms). Each of these channels have strongest effects later in life. Agents are less willing to change hours of work earlier in life, when the shadow price of leisure includes lower wage rates in many future periods, in addition to lower earnings today. Although we cannot say conclusively which, if any, of these mechanisms explains the effect, we discuss the evidence for and against each.

Virtually no other research has been conducted on the impact of cash assistance—or, indeed, any other type of government assistance—on beneficiaries themselves long after the assistance has ended. Similar to our research on SIME/DIME's long-term effects on adults, **?** find that the effect of extensions of unemployment benefits fades by 3.5 years after the benefits end, but they do not look at longer-term results. Some other papers analyze outcomes in the first few years after programs end—for example, **?** analyze effects of a work-subsidy program after the program ended and note that there are no significant effects 1.5 years after subsidies end. However, very few papers consider outcomes even 5 years after a program ends. This is an important omission because significant post-experimental effects from SIME/DIME take more than 5 years to appear.

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

The long-term effect of parental cash assistance on children remains an empirical question that has not been adequately explored. Such assistance could increase their children's long-term take-up of government benefits and decrease earnings; it could have the opposite effect; or it could have no significant effect in either direction. Based on adult outcomes, one might expect similar effects on children. Indeed, **?** cites the original IME findings in asking, "Does welfare undermine the family? As far as we know from the NIT experiment, it does, and the effect is large." Some research indicates that parental receipt of government benefits increases the chance that their

children will receive benefits themselves,[15] a concern that is enshrined into law.[16] On the other hand, a growing literature documents that parental income, or factors related to income, can have long-lasting positive effects on children. More income can allow parents to buy more of the goods and services that their children need to succeed, including education, health care, or access to a better neighborhood; or the income guarantee could give parents the ability to be more involved in their children's lives with increased time or decreased stress. Literature reviews on this topic include **?**, **?**, **?**, and **?**. Finally, it is possible that cash assistance—at least, past a certain point—does not affect the factors that matter for children in the long run.[17] Some prior research, including research discussed in the review articles mentioned above, finds little scope for an effect of parental income, or factors related to it, on outcomes for children.

In fact, in contrast to the significant effects on parents, we find little evidence of an effect on children for any variable studied. For example, as discussed in Section **??**, we can rule out at the 95% level treatment changing the probability of a child applying for either SSDI or SSI by more than 3 percentage points (12% of the mean). This null result is consistent with intergenerational welfare transmission being driven by information, as hypothesized by **?**, because information about SIME/DIME would not be helpful for children in applying for SSDI or SSI benefits. (**?** found that parents receiving disability benefits caused their children to receive those benefits, but not other types of benefits.) We can also rule out effects on child propensity to work of more than 1.9 percentage points (2.5% of the mean), and a change in annual earned income of more than $1,500 (6.9% of mean annual earnings for children). These results allow us to rule out effects on earned income of the size found by **?**, who study a similar program and find that treated children earned 14% more earnings. Because **?** studied a population targeted as particularly needy, it may be that cash assistance helps children who are in the most need while it does not improve the lives of others who are in less need. This research also supports other work that has found little evidence that cash for parents benefits their children in the long term, such as that done by **?**, **?**, and **?**. (In Section **??**, we do document some significant effects on some child outcomes for certain subgroups of the population—those whose parents received treatment longer, and the youngest children. As discussed below, however, some caution is necessary in interpreting these

[15] See, for example, **?**, **?**, and **?**.

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

[17] In particular, the cognitive and non-cognitive skills that may be vital for children's later success, as documented in a literature reviewed by **?**, may not be strongly influenced by cash assistance.

results, particularly because of the number of tests that were run on child outcomes.)

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

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

2 The Income Maintenance Experiments

2.1 General background on the experiments

The Income Maintenance Experiments were conceived in the 1960s to test possible changes to the welfare system.[18] Many at the time believed that a more generous welfare program could help families out of poverty. Additionally, because two-parent households 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%.) To determine if these concerns were valid, a series of IMEs were funded by the federal government: the New Jersey IME (in New Jersey and Pennsylvania, from 1968-'72); the Rural IME (in Iowa and North Carolina, from 1969-'73), the Gary IME (in Indiana, from 1971-'75) and SIME/DIME. SIME/DIME, funded by the US Department of Health, Education, and Welfare,

[18] Unless otherwise noted, the background described in this subsection is drawn from **?**, **?**, or SIME/DIME data.

Level	Variable	Fraction
Family	Seattle	.442
Family	Two household heads	.366
Family	Black	.39
Family	White	.432
Family	Chicano	.178
Family	Positive pre-exp benefits	.467
Adult	Positive pre-exp earned inc	.656
Adult	Male	.371
Adult	Education: HS+	.55
Adult	Education: college+	.03

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

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

included more families than all the other IMEs combined, and was also more generous per family.

Some characteristics of the SIME/DIME families are displayed in Table **??**. The 4,800 families enrolled were generally of low socio-economic status. About half of the adults had fewer than 12 years of schooling; only about two-thirds of adults had any pre-experimental earned income;[19] and those who did have such income had average annual earned income of $21,900.

Each of the 4,800 families enrolled in SIME/DIME was assigned to a treatment category using stratified random assignment in 1971 (in Seattle) or 1972 (in Denver),[20] as discussed in Subsection **??**. A family of four that was assigned to the financial treatment[21] 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;[22] lower-income families were more likely to be assigned to lower guarantee levels. Every extra dollar the family earned would be taxed back at a rate between

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

[20]Additionally, a small number of participants were assigned to treatment status up to three months before that year.

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

[22]Families of sizes other than 4 were guaranteed a ratio of this amount, intended to make the treatment comparably beneficial. The ratio for a family of 2: .62; 3: .83; 4: 1.00; 5: 1.12; 6: 1.23; 7: 1.32; 8: 1.38.

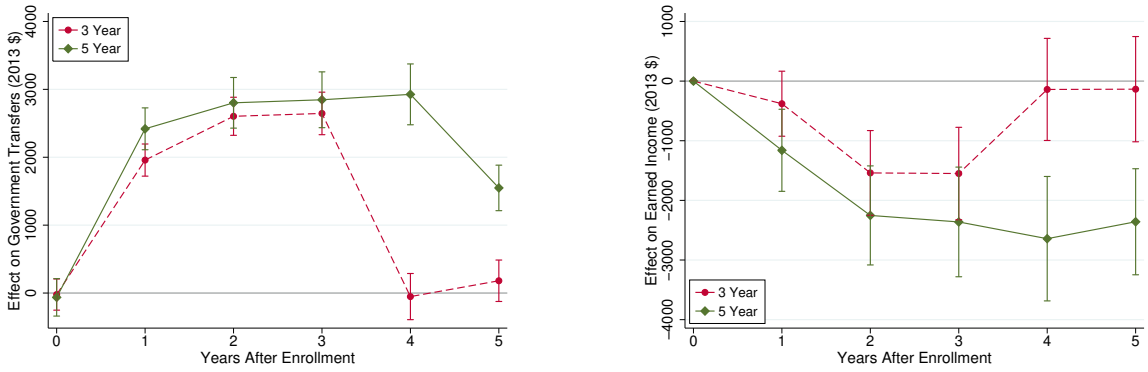
50% and 80% (with the precise rate selected randomly), and treatment lasted 3 or 5 years (again, depending on the randomly-assigned treatment group).[23] Treatments were, on average, weighted to be more generous for those at higher income levels so that everyone would have a similar chance to receive the benefits. To better control treated families' incentives, almost all other government benefits, such as Aid to Families with Dependent Children (AFDC) and Food Stamps, were taxed at a rate of 100%; and any income taxes paid were refunded, up to the point where the treated family would have the same income whether on or off the treatment. The transfer was paid monthly and, if a family split up, both new families would be eligible. These benefits were generally much more generous than existing assistance programs, though 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.

The Income Maintenance Experiments have been extensively studied; for more details, see citations in Footnote **??**. As might be expected, financial treatment greatly increased the amount of benefits families received from the government; Figure **??** shows the effect of treatment on total government transfers received each year, for the first 5 years after assignment to treatment.[24] As shown in Figure **??**, earned income declined during treatment. This was driven by moderately decreased hours of work: treated husbands reduced their hours of work by 9%; treated wives by 20%; and treated single mothers by 14%. Most of this change was on the extensive margin: individuals generally had longer non-employment spells. Labor supply responses were generally larger for families in the 5-year treatment group and for those with more generous guarantees, though different tax rates did not appear to cause significantly different effects. As shown in Figure **??**, no significant effects are observed on either unearned or earned income after the experiment ended for those treated for 3 years (post-experiment data is not available for the 5-year treatment group).

A second important set of results were that the treatment decreased marital stability. Treatment caused black and white families to be approximately 40% more likely to split up; no significant

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

[24] Families not receiving SIME/DIME benefits could receive government transfers from a variety of programs, including AFDC, unemployment insurance, food stamps, Social Security benefits (including disability, old age benefits, and aid to the blind), veterans' or survivors' benefits, and general assistance.



(a) Benefits

(b) Earned income

Figure 1: Effects on total government transfers and earned income during and immediately after treatment

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

effect was observed for Chicano families. (There is some disagreement about the robustness of the results on marital stability; see, for example, **??** and **?**.) Because of the importance of these findings, we explore effects on marriage and divorce for both parents and children. Evidence of effects on families beyond labor supply or marital stability is somewhat limited by what **?** calls the "tag-on nature" of research about non-labor supply effects, which "were not given the same degree of attention" in the design of the IMEs. Broadly, however, few systematic significant effects were observed on types of goods consumed, fertility, child academic outcomes, or other indicators.

2.2 Assignment to treatment groups in SIME/DIME

Assignment to treatment in SIME/DIME was based on the "Conlisk-Watts Assignment Model," a stratified random design described by **?** and others. Families were stratified into groups on the basis of their site (Seattle or Denver), race (black, white, or Chicano),[25] family type (headed by one or two adults), and "normal income" level.[26] Statistics about the site, race, family type, and

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

[26] Families were assigned to one of eight normal income levels based on a subjective assessment of their long-term family income, adjusted for family size. Only those with normal income in the six lowest categories were enrolled

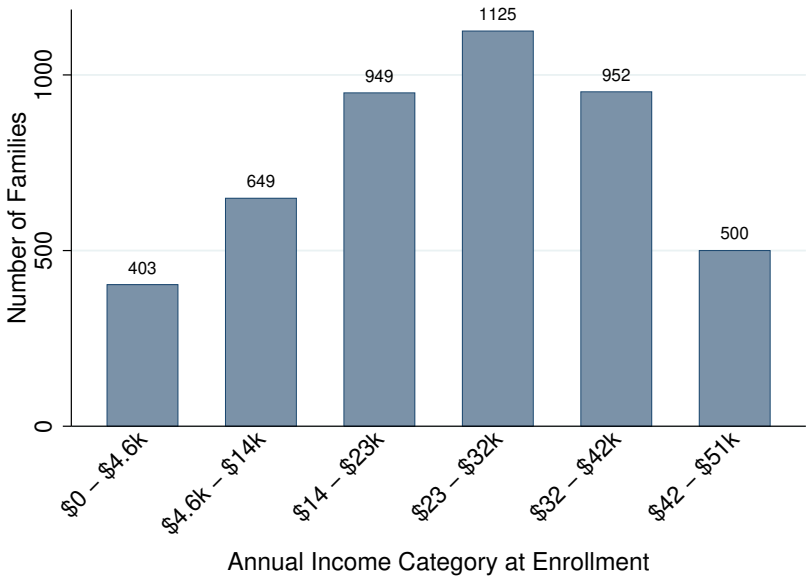


Figure 2: Normal income levels

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

normal income level of participating families are shown in Table **??** and Figure **??**, along with other details about the sample. According to published accounts, within these groups, treatment was assigned randomly.

A balance test on pre-experimental variables is shown in Table **??**; based on this test, treatment and control may not have been balanced in Seattle. Controlling for assignment groups, treated Seattle individuals earned $1,400 less than controls in annual pre-experimental earned income (compared to an average of $15,000). Significant differences also exist in pre-experimental hours worked. Such a pre-experimental difference could occur by chance, though that is unlikely. Because we do not have more information about the cause of the imbalance, we attempt to correct for it by controlling for adult pre-experimental earned income in all regressions. Because a simple control may not eliminate all bias, we also present all results in Appendix **??** restricting the sample to Denver only, where there is no evidence of a statistically significant lack of balance. Additionally, because assignment to treatment status was done in Denver separately and at a later date, it is possible that lessons learned from the Seattle assignment process may have been applied to Denver.

for possible treatment. These income levels were not based only on prior year's income, but instead were based on a subjective evaluation of the family's typical income, as described by **?**.

Variable	Whole Sample	Seattle Only	Denver Only
Earned income	-635**	-1403***	29.1
	(256)	(413)	(317)
Hours worked	-22.5	-63.1**	12.4
	(17.6)	(26)	(23.9)
Gov't benefits	115	397	-128
	(174)	(287)	(210)
Years of ed	-.0383	.0757	-.127
	(.0659)	(.104)	(.0846)
Kids age 0-5	.0298	.03	.0296
	(.0297)	(.0433)	(.0407)
Kids age 6-15	-.00877	.0435	-.0537
	(.0425)	(.0609)	(.0591)
People age 16+	-.061	-.0787	-.0457
	(.0378)	(.0555)	(.0516)

Table 2: Pre-experimental balance test

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

Estimated effects are similar in Denver only (compared to the combined Seattle-Denver sample), though they are less precisely estimated.

3 Data and methods

3.1 Data

Data from SIME/DIME itself is almost exclusively from **??**, though some data, particularly for robustness checks, was derived from **?**. We report outcomes for adults who were household heads at the start of SIME/DIME, and any biological children who were under 18 years old when the experiment began.[27]

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

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

To explore the effect of SIME/DIME on interactions with the disability system, we use the SSA's administrative data on SSDI and SSI applications and awards. This data comes from the SSA's Master Beneficiary Record, the SSA 831 file, and the Supplemental Security Record file. Together, these files represent a comprehensive record of SSDI and SSI beneficiaries. They are also a comprehensive record of applications for SSDI and SSI benefits beginning in 1990, and include about 81% of applications between 1978 and 1989, with no known systematic difference between denied applications included or not included in the data sets.[28]

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

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

results are similar in that data.[29]

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

Finally, to understand how SIME/DIME outcomes relate to the performance of the economy at the time, we use decennial census data from 1970 and 1980, as provided by **?**. We define 16 industries that overlap between census data and SIME/DIME data, and determine the log change in employment in each industry, within each state in the experiment (Washington and Colorado).

3.2 Matching experimental families to outcome data

Individuals from SIME/DIME are matched to outcomes using the procedure described in detail in Appendix **??**. To summarize, we look at patterns of family birthdays in SIME/DIME records and match them to similar patterns in the SSA's Numident and WA DOH birth records. For example, suppose a Seattle single mother born January 1, 1930 has three male children born on February 1 of 1960, 1961, and 1962. It is unlikely that another family has exactly the same birth pattern. Thus if we find three male births, on those days, with the same mother name, we can be reasonably confident that they are the same family. We then perform a placebo test by adding a certain number of days to each birthday and rerunning the match; we can use the number of matches found using the real and placebo birthdays in a maximum likelihood procedure to estimate the probability that a match is correct. In our baseline specification, we include all SIME/DIME individuals who are matched to exactly one SSN with at least 95% confidence. With this algorithm we match 45% of parents, and 59% of children. There is no significant effect of treatment on the probability that we find either parents or children overall, or within various subgroups, as shown in Tables **??** and

[29]SSA death data, rather than WA DOH death data, is used as our baseline for several reasons. First, of course, WA DOH data can only be reasonably used to study outcomes for the Seattle half of our sample. Further, even within the Seattle sample, SSA death data includes more death records for both adults and children than WA DOH data. Only 4.6% of adult deaths and 13% of child deaths from WA DOH data are not recorded in the SSA; meanwhile, 22% and 25% of adult and child deaths, respectively, recorded in SSA data for Seattle participants are not recorded in WA DOH data. Additionally, results based on WA DOH death data could also be biased if treatment caused individuals to differentially leave the state, which we have no way of testing.

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

Table 3: Summary statistics based on outcome variables

Notes: "Sample" refers to the same SIME/DIME matched sample described in Section **??**. 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.

??. As discussed in Appendix **??**, we estimate that 5.2% of matched adults and 1.3% of matched children are matched to an incorrect SSN. The rate of false matches for adults is comparable to that if SSA data is matched on name and date of birth, while the false match rate for children is better.

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

These differences are all reminders that results may be difficult to generalize, for several reasons. First, the participating families were only those who volunteered, among low- to middle-income families in specific neighborhoods in two cities in the 1970s. Further, we are only able to study families with at least two children, and we are only able to use data on about half of these families, who are more likely to be in larger families and families with rare last names. These families studied here may react differently to cash assistance than families that would be affected by current policies. Furthermore, we are only able to measure effects of the SIME/DIME treatment itself rather than any policy currently under consideration. For example, the policy and experiment proposals

in Canada, Kenya, and the United States mentioned in the Introduction are for "basic income" guarantees, which are similar to the NITs studied here but with tax rates of 0%. Proposed policies would also likely last longer, and include a far greater portion of the population, than SIME/DIME. We are able to analyze the effect of small variations in treatment because SIME/DIME included different treatments (among other variations, treated individuals were randomly allocated to different treatment lengths, guarantee levels, and tax rates). However, these variations do not cover all treatments we would be interested in. In particular, we can say little about general equilibrium effects of this treatment because there were very few treated families relative to the Seattle and Denver metropolitan areas. Finally, SIME/DIME control families were also able to use AFDC, food stamps, unemployment insurance, and other welfare programs. For this reason, our paper compares generous cash assistance to a standard welfare program, rather than comparing a welfare program to a lack of such a program. Despite these limitations, long-term outcomes for SIME/DIME families are important to study because there are so few other settings where long-term effects of similar interventions can be analyzed.

3.3 Empirical methods

All causal effects reported in this paper, unless otherwise specified, are based on a regression of the outcome of interest against a dummy variable for initial assignment to treatment. Regressions include a dummy variable for each stratification group: unique combinations of site (Seattle or Denver), race (black, white, or Chicano), family type (headed by one or two adults), and the adult's pre-experimental "normal income" level. Regressions also include pre-experimental earned income, sex, manpower treatment status, and a cubic polynomial in date of birth as independent variables. Standard errors are clustered at the level of the original family, which is the level at which randomization occurred.[30] When the outcome of interest is available at an annual frequency, as with most variables based on income, we include one observation per person per year. We also include year fixed effects in these regressions. 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. (All main results are robust to using a Cox proportional hazard model instead.) For children, data on adult pre-experimental income, as well as any other data on adults, is based on the primary breadwinner: the parent who earned the most pre-experimental income.

[30]The "original family" refers to the family units that were present at the start of the experiment. Families could split due to divorce or a child leaving home; because these new units may exhibit correlated behavior, we cluster at the higher level.

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

We also examine how the treatment effect for adults varies with the change in employment in the adult's state, in the first industry that adult was employed during the nine months at the start of the experiment, before treatment began. Some adults were not employed during those nine months; they are dropped from this analysis. The parameter of interest is the regression coefficient on treatment interacted with log change in employment. To ensure that pre-experimental differences do not drive these results, these regressions also include interactions between log change in employment and dummy variables for the assignment groups, and between log change in employment and pre-experimental income.

4 Outcomes for adults

4.1 Results

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

Effects on these five outcomes for adults are shown in Table **??**; in Table **??**, we explore the intensive margins associated with several of these outcomes. Treatment caused adults to be 3.3 percentage points less likely to work in a given year. In column 1 of Table **??**, we see that this effect is not explained by any differential mortality: the effect is nearly identical if we only include years in which the individual is not known to have been dead.[31] Partially because they work fewer years, treatment caused individuals to earn $1,800 less per year; this decrease represents 7.4%

[31] We use labor market outcomes unconditional on mortality as our baseline because mortality could be an endogenous variable.

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

Table 4: Parents, effects on main outcomes

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

of the participants' $24,000 mean annual earnings (for both treated and untreated participants). As shown in column 2 of Table **??**, there is no significant effect of treatment on annual earnings conditional on working in a given year; however, the large negative point estimate indicates that there could be an important effect on this intensive margin. The effect on lifetime earnings is quite large relative to the initial cash assistance shock. Discounting future earnings at 3% (after adjusting for inflation), individuals earned, on average $4.50 less in lifetime earned income for every dollar of extra government transfers during the experiment.[32] This marginal propensity to consume leisure out of unearned income is substantially higher than that estimated by **?** or **?**. We return to this point below.

Treatment also caused adults to be 6.3 percentage points more likely to *apply* for disability benefits, an increase of 20% on the 32% chance that the average (treated or untreated) participant would apply for such benefits. This effect does not appear to be related to underlying medical conditions: treated individuals are no more likely to have been *awarded* disability benefits, or to have died by 2013. In fact, as shown in column 3 of Table **??**, treatment increased the chance of

[32]This number includes earned income both during and after treatment. The number is somewhat sensitive to the discount rate, particularly because post-treatment effects are strongest later in life. However, the effect on lifetime earnings is large relative to the shock for any plausible discount rate. In particular, a 5% discount rate implies lifetime earnings that are $3.20 lower, and a 10% rate implies $1.60 lower earnings. Even a 20% rate implies $0.85 lower earnings, implying that the vast majority of extra wealth is spent on leisure.

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

Table 5: Parents, intensive margins

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

being rejected for disability benefits, among those who did apply. Thus the individuals who are induced to apply for disability benefits are judged by the SSA upon application to be less deserving of those benefits than the average applicant from our sample. Columns 4 to 8 of Table **??** show that treatment also somewhat changed the mix of impairments that individuals applied under, making them less likely to apply based on a mental disorder or a less-common impairment.[33] As noted by **?**, the increase in SSDI beneficiaries with mental disorders (along with those with musculo-skeletal disorders) is an important component of the rapidly increasing disability rolls. That treatment actually decreased the probability of applying based on a mental disorder suggests that the mechanism at work here may be unlikely to be related to the increase in SSDI receipt over the past few decades.

These results are generally quite robust to alternative specifications. Table **??** presents a variety of robustness checks on these results, while Appendix **??** presents these results, along with all others, for Denver only. Point estimates for each variable under different specifications remain similar, and remain statistically significant under almost any alternative specification. Estimates are also generally similar among different subgroups of the population, as shown in Table **??**. Although some differences are significant, there are few systematic differences among the groups. For example, the effect on annual earned income is significantly higher (or less negative) at the 10% level for those in the lowest pre-experimental income category (under $14,000) than the middle category (between $14,000 and $32,000); and the effect on whether they earned income is higher (at the 10% level) for the lowest pre-experimental income category than for the highest category (above $32,000). However, other differences among these groups for these variables are not significant, and the point estimate of the effect on disability applications is actually higher among those in the lowest category than those in the highest category. Thus, although there is some evidence that the effects we observe are weaker for those who start out earning less, it is far from conclusive.[34]

Some heterogeneity does exist by the performance of the individual's industry, as shown in Table **??**. In particular, the effects we find are strongest for individuals who start the experiment in growing industries. For example, individuals in industries that grew by 10 log points (or about 10%) had treatment effects on annual earnings that were $1,400 stronger (that is, more negative). One

[33] Effects on impairment category need not add up to zero because individuals can apply under multiple impairments.

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

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Treated	−.0297*	−1915	.0731***	.0404*	.0296
	(.016)	(1266)	(.0257)	(.0243)	(.0258)
Treated ×	−.283***	−14254**	.269*	.283**	−.11
Industry Δ Empl	(.0874)	(6555)	(.141)	(.132)	(.134)
Dep var summary stats					
Mean	.763	29292	.296	.238	.411
Std. Dev.	.425	26771	.457	.426	.492
N	32716	32716	1469	1469	1469
People	1448	1448	1469	1469	1469
Clusters	1271	1271	1288	1288	1288

Table 6: Parents, differential effects by industry performance

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. "Industry Δ Empl" is the log change in employment in the individual's original industry, in the individual's state (Washington or Colorado), between 1970 and 1980 (according to the Decennial Census). The regression also includes this log change multiplied by dummy variables for the assignment groups, and multiplied by pre-experimental income.

potential explanation for this difference is that the long-term effect we see is driven by individuals who worked less when it was easier to find a job, as opposed to individuals who worked less when they might not have been employed anyway. If that is the case, long-term effects might not be as strong if cash assistance is tied to economic conditions, as with unemployment insurance extensions during recessions.

Regardless of the mechanism, the significant results on long-term outcomes may be surprising in the context of the original finding that there was no effect on earned income in the two years after the experiment ended, as shown in Figure **??**. Indeed, in many contexts—such as the Self Sufficiency Project studied by **?**—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 **??** plots the effect on earned income, and on (cumulative) disability applications,

at each age from 35 to age 65 or 75.[35][36] As shown in Figure **??**, effects are strongest between approximately ages 50 and 60, which corresponds to the time when most people leave the labor force and retire. The difference between effects in the 50s and at younger ages occurs within individual treated workers, and is significant: controlling for individual fixed effects, the effect on earned income is $2,000 stronger between ages 50 and 60 than at ages younger than 50 (a difference that is significant at the 5% level). Figure **??** shows that many of the applications for disability benefits that are induced by the experiment occur between ages 50 and 60 as well. As noted above, treatment induced individuals to apply for—but not receive—disability benefits; thus we might think of disability applications mostly as a signal of wanting to leave the labor market. Taken together with the fact that only 15% of adults were 50 or over by the fifth year of the experiment, these results suggest that the reason there were not effects on earned income immediately after the experiment is that the effect is driven by individuals retiring earlier. We explore this idea further—that effects may be delayed due to a retirement motive—in Subsection **??**.

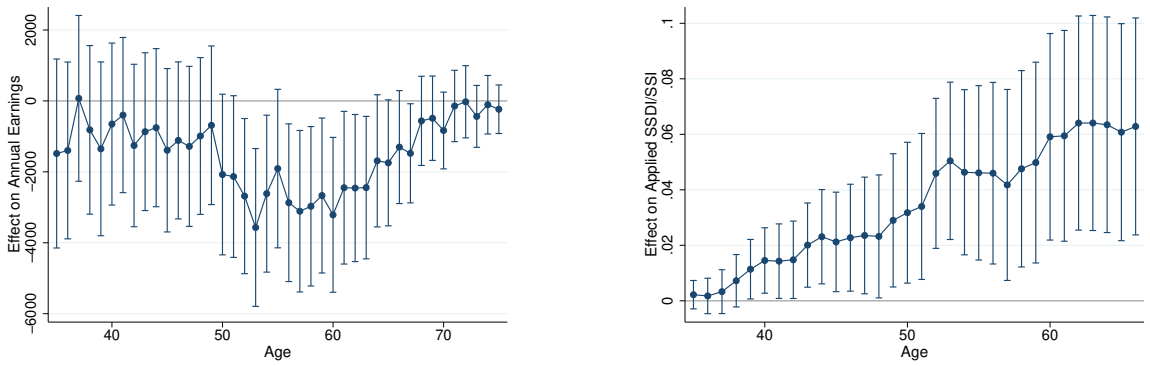
An important issue with these results concerns external validity: how would the results be different if the treatment had been different? For some variations on the treatment, we cannot know the answer. For example, SIME/DIME treated only a few thousand of the millions of people living in their metropolitan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

However, different SIME/DIME treated families did receive somewhat different treatments. Families' treatments could vary in the length of treatment (3 or 5 years); their guarantee level (the money received if no income is earned); the tax rate when $0 is earned (either 50%, 70%, or 80%); whether that tax rate declined as more money is earned;[37] and whether they were in the manpower treatment (discussed in Footnote **??**). Table **??** shows how results vary when we include these variables in a regression. In general, few of these effects are statistically significant,

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

[36] Similar graphs for other main variables, as well as graphs looking at effects for given numbers of years after the experiment, are available in Figures **??** and **??**. Annual averages for these variables are shown in Figures **??** and **??**.

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



(a) Earned income

(b) Disability applications

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

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

suggesting that these results are somewhat generalizable. Most significantly, individuals on the longer 5-year treatment experienced a stronger effect on disability applications, indicating that a policy—which would likely be longer-lasting—might have stronger effects. A priori, we might also expect that the high tax rate, rather than the cash assistance itself, might have caused people to leave the workforce, which could lead to long-run effects. However, there was little evidence during the experiment that variations in the tax rate caused adults to work less. Consistent with that, we do not find that a higher tax rate is associated with stronger effects; if anything, point estimates lead to the opposite conclusion. Of course, much caution is needed in predicting the effect of, for example, a 0% tax rate or a permanent program because such predictions require extrapolation beyond the domain of treatments tested. This is particularly so here, given the large standard errors on the estimates in Table **??**. Regardless, these results provide some suggestive evidence.

In addition to the five main variables discussed above, the richness of the SSA and WA DOH data allow us to study several other outcomes; see Table **??**. These results show, first, that the effect on disability applications acts through both the SSDI and SSI programs. Applications to each are induced to increase by treatment. There is also marginally significant evidence that treatment increased the rate of SSDI awards, though not of SSI awards.

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

Table 7: Parents, different treatments

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

We find no significant effect on marriage or death, both measured using WA DOH data. In column 6, we explore effects on marital breakup or divorce. This dependent variable, available only for Seattle adults who were initially married, is an indicator that takes a value of 1 if the adult was in the same relationship at the end of the experiment as the beginning, and also never appears in WA DOH divorce records.[38] As discussed above, some researchers found that treatment significantly increased split-ups during the study period. These results, which were debated in the literature, contributed to SIME/DIME's effects on policy. Overall, we find no significant effect on couples splitting up or divorcing. However, this may not indicate that the initial results faded over time: although the overall effect on split-ups was significantly positive, it was only insignificantly so in our sample in Seattle. Further, treatment is associated with fewer divorces, but only insignificantly so. Thus there is no clear causal story of the difference between the null marital dissolution results here and the significant results found by **?**.

Finally, we can look at other margins along which earned income can adjust. As shown in columns 8 and 9, there is no significant effect on self-employment income, likely because the overall level of such income was so low. Thus it seems unlikely that treatment had much effect on long-term entrepreneurial activities. Columns 10 through 13 show that annual earned income generally declined by several different measures, though we have limited power to find such an effect at higher levels of income.

4.2 Model of adults' reactions to SIME/DIME shocks

As discussed above, treatment caused no significant effect on earnings immediately after cash assistance ended, but did cause significant effects much later in life, as adults approached retirement. This pattern may be surprising, as economists sometimes take a lack of effects immediately after a treatment ends as evidence that the treatment has no long-term effect. However, individuals generally consume much more leisure toward the end of their lives, with drops in earned income occurring soon before retirement; see, for example, Figures **??** and **??**, which show the fraction of people earning income, and the average income earned, for SIME/DIME adults at different ages. Therefore, it may not be surprising that treated adults also consume most additional leisure toward the end of life, which could be accomplished by simply taking an early retirement.

[38] Due to data constraints, this measure conflates two definitions of splitting up. In SIME/DIME, "marriage" was defined as cohabitation, while the WA DOH divorce records deal only with legal marriage. However, this is the best long-term data available on couples splitting up, as legal marriage data is not available during the experiment and cohabitation data is not available after it. Because of the importance of the original results on marital break-ups for policy and the subsequent debate about these results, this imperfect measure could have been an important indicator.

To formalize this idea, in this subsection we introduce a life cycle labor supply model that includes learning by doing. In this model, individuals endogenously consume more leisure as they get older; some other models that include this feature might also generate delayed effects, but this model shows the delayed effects particularly clearly. The model we introduce below is closely based on that used by **?** (itself a fairly standard life cycle model that adds learning by doing); but, for simplicity, we drop the stochastic component of preferences and wages. (We also simplify notation where possible.)

In our model, rational, forward-looking agents (with preference paramters β, C_0, a_1, a_2, and ϕ) maximize their expected utility, a time-separable function of their consumption of market goods C_τ and hours of labor supply h_τ at any age τ between a starting age t (which we set to 35) and a final working age of T (which we set to 65):

$$E_t \sum_{\tau=t}^{T} \beta^\tau \left[C_0 \frac{C_\tau^{a_1}}{a_1} - b \frac{h_\tau^{a_2}}{a_2} \right] + V_{T+1}(A_{T+1}), \tag{1}$$

with terminal value function given by

$$V_{T+1}(A_{T+1}) = \begin{cases} 3\log(A_{T+1}+\phi) - 1 - 3\log(\phi) & \text{if } A_{T+1} > 0 \\ \left(\frac{A_{T+1}-\phi}{\phi}\right)^3 & \text{otherwise.} \end{cases} \tag{2}$$

Agents are subject to an intertemporal budget constraint on A_t, the agent's asset holdings at age t:

$$A_{t+1} = (1+r)A_t + W_t h_t - C_t, \tag{3}$$

where W_t is the wage rate, which is equal to the stock of human capital. This human capital evolves according to

$$W_{t+1} = w_0 + \delta W_t + A_0(1 + A_1(t-19))(B_1 + W_t)[(h_t + d_1)^\alpha - B_2(h_t + d_1)]. \tag{4}$$

(As described in **?**, this human capital production function is designed to take into account various features of the National Longitudinal Survey of Youth data they use to estimate the parameters.) All parameters in this model are drawn from those measured by **?** for high school graduates, which represents both the median and modal education level of SIME/DIME participants. As with all other values in this paper, results are adjusted for inflation to 2013 values. Initial conditions

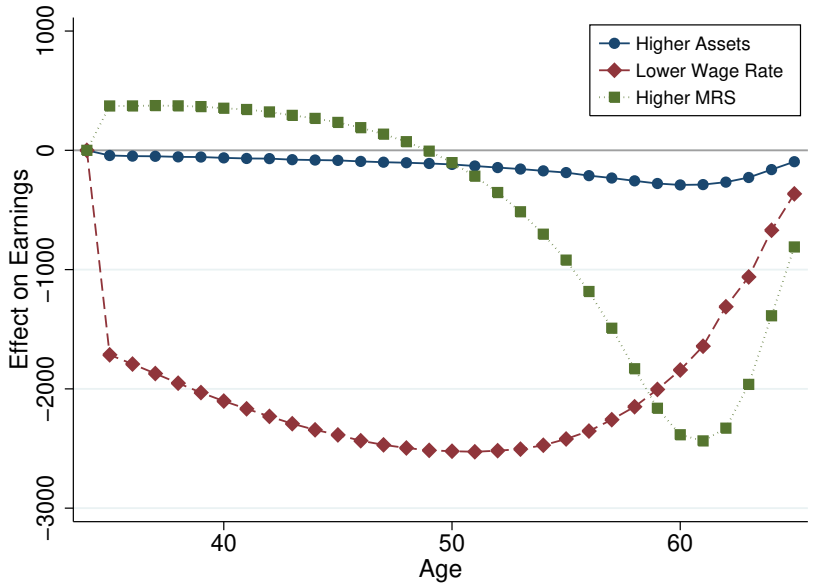


Figure 4: Effects on earnings of shocks to wealth, wage, and preferences

Notes: Each line represents the effect of a shock experienced at age 35 to a parameter of the model described in Subsection **??**. Effects are measured as changes to earnings in 2013 dollars, compared to a baseline where no shock was experienced. "Higher Assets" indicates that the agent receives $2,800 greater wealth. "Lower Wage Rate" indicates that the agent's hourly wage rate declined by $0.59. "Higher MRS" includes a wealth increase of $2,800, an hourly wage rate increase of $0.30, and a marginal rate of substitution between consumption and labor that is 3% higher.

are based on population averages in **?**. We solve the model for policy functions with backward induction, beginning with the retirement age of 65.[39]

In general, effects of shocks on earned income tend to grow over time in this model. That is because the shadow wage rate is much higher than the actual wage rate early in life, because it includes the human capital that could be accumulated. We model three potential channels through which treatment could affect long-term earnings: through wealth, wage rate, or preferences. Each channel is modeled, as a reduced form, as a shock to a parameter of the model experienced at age 35. The effect of each shock is plotted in Figure **??**. With the data we have, we cannot conclusively say which, if any, of these mechanisms drive the results that we see. However, we discuss evidence for and against these potential mechanisms below.

First, treatment could increase total assets because treated families received large cash transfers. We model this as an increased wealth of $2,800, which is the treatment effect on combined earned

[39]There is technically no retirement in this model until the final period, because agents generally work a positive, though declining, amount until age 65. This model allows us to match the average decline in earnings as individuals age, but does not match fact that many individuals work full time until retirement. A similar model with limited choices of hours (for example, full time, half time, or not at all) might better match this feature, but would not feature the smooth decline we see here.

and unearned income during the experiment. This is chosen because it represents a plausible upper bound on the treatment effect on wealth: the additional wealth treated adults would have if they spent none of their additional income on consumption (except of leisure). As shown in Figure **??**, such a wealth shock results in effects that are strongest as the individual approaches retirement. However, the scale of the effect is substantially smaller than that observed in Figure **??**. This is not surprising; as discussed above, the long-term earnings response is 4.5 times larger than the initial shock to unearned income, and it is implausible that a shock to wealth would cause a large decrease in consumption of market goods. This is particularly true here because SIME/DIME includes individuals earning a wide range of incomes—between $0 and $51,000 per year—so the effect is unlikely to be explained by a constraint that a small amount of money can overcome. Indeed, **?** analyze a pure wealth shock and find a lifetime marginal propensity to consume leisure out of unearned income of approximately 0.11 (though their estimate is based on an extrapolation of only 10 years of post-treatment data).

A few considerations may make it more plausible that the effect seen in SIME/DIME families is purely driven by wealth.[40] First, our estimate of the effect on initial wealth could be low if misreporting or attrition biased the original results to indicate a stronger effect on earned income during the experiment. Using administrative data, **?** found that the true effect on earned income would be about 30% lower if there were no misreporting.[41] That difference would approximately double the total change in income during the first five years after treatment began. Additionally, we do not take into account unearned income. Although we see no effect on disability benefit awards, the fact that treatment increased applications suggests that there may have been effects on other benefits. Unfortunately, we have no data on post-experimental welfare (AFDC or TANF), food stamps, or other programs, but this other unearned income could help us understand how a positive income shock could lead to lower total observed income.

A second potential channel for the effects on adults would be through wages. The time out of work could have decreased the human capital of the participants, as in the model outlined above. Alternatively, time out of work could decrease inferred ability, as described by **?**. In either case, the

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

[41] This number is based on results from Table 5 of **?**, where the authors estimate the effect sizes if misreporting is equal in sectors where they have data and sectors where they do not. We take the fraction of true effect against estimated effect and weight by the fraction of the household head population in each group (husbands, wives, and single female heads).

additional leisure time during the experiment would result in lower wages once the treatment was no longer in effect. In fact, based on original data from the experiments, we see no evidence of an effect on post-treatment wages. We cannot rule out, however, that treatment reduced hourly wages by as much as \$0.59 in the year after the treatment ended, though; the effect shown in Figure **??** uses this point estimate.[42] The time pattern of this effect also goes against our data. Although the effect grows until the agent is around 50 years old, there is still a strong effect immediately. This is to be expected from the model, as the agent is facing a lower wage immediately; but it goes against our evidence on the effect for those aged below 50.

Finally, we consider a shock to preference parameter b, which is proportional to the marginal rate of substitution between consumption and leisure for a given level of each. The effect on preferences is a reduced form for a variety of possible effects.[43] Because we do not have a robust measure of such preferences, we calibrate the estimated shock to the data as follows. First, we take as given the point estimates for the effect on wealth (an increase of \$2,800) and wage rate (an increase of \$0.30). Then, we calibrate b to match the effect on total earned income after treatment ended and between ages 35 and 60 (discounted at a 3% rate) of \$28,000. This can be matched by an increase in b of 3%. The effect of these combined shocks is also plotted on Figure **??**, which largely matches our data. Of course, changes in preferences can generally account for any change in behavior. However, one might assume that a change in preferences would cause effects that show up immediately. This model shows that, under plausible assumptions, a small change in preferences can account for the full pattern of delayed effects.

[42] We calculate wage rate with the original SIME/DIME data by dividing earned income by total hours. Estimates reported here are based on the fourth year for those in the 3-year treatment who worked at least 520 hours in that year. Effects on wages are also not significant if we include a lower or higher hours threshold.

[43] One example would be habit formation: individuals induced to work less find that they enjoy leisure more. The treatment could also reduce the stigma attached to receiving government benefits, or cause individuals to expect government benefits, because it induced many individuals to receive such benefits who would not have otherwise. Treatment could also cause individuals to become more efficient at home production; thus what we view as leisure time would actually be time engaged in a productive activity. To the extent that the effect on preferences is due to habit formation, we might more consistently model the effect on preferences as increasing over time, as individuals spend more time out of the workforce. However, such a model would not be consistent with all possible effects we are representing with this reduced form. Furthermore, if we did include continuous habit formation in the model, that would only strengthen the case that a short-run intervention can cause effects that appear stronger as time goes on.

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

Table 8: Children, effects on main outcomes

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

5 Outcomes for children

5.1 Results

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

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

Table 9: Children, intensive margins

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

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

The baseline null results are estimated with enough precision to meaningfully inform the literature on the intergenerational effects of cash assistance, as discussed below. Because we do find so many null effects, some caution is needed before digging deeper for effects on various subgroups of the population. When we analyze so many subgroups, we expect about 5% of effects to be significant at the 95% level. It is possible, however, that the overall null result masks important heterogeneity. We therefore consider possible heterogeneity below; significant results, though, should be taken as suggestive evidence of differences.

We first look at whether results vary by the family's type of treatment. Table **??** shows that children in the 5-year treatment have results more similar to their parents: they are significantly less likely to earn money in a given year and are more likely to apply for disability insurance. They are also more likely to be awarded disability insurance. This suggests that a longer-lasting program could have caused significant effects for children. Results for various subgroups of the population are shown in Table **??**. In general, few significant effects are found for any subgroup. One subgroup of particular interest, though, is the youngest children; many believe that early-life interventions may be most influential.[44] Results for children born during the experiment are shown in the row marked "Age ≤ 0." There is evidence that treatment significantly reduced earnings for this group. However, we find no significant effect on other outcomes for this group.

5.2 Relationship to other literature

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

Some research has found that parental receipt of benefits increases children's probability to receive benefits themselves. **?** finds that if they had children themselves, daughters of mothers who

[44] Research supporting the idea that early interventions are most useful include **?**, **?**, **?**, **?**, and **?**. However, as noted by **?**, 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 **?**, **?**, and **?**.

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

Table 10: Children, different treatments

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

received AFDC benefits were approximately 300 to 500% more likely to receive AFDC benefits themselves, and concludes that much of this correlation is causal. Similarly, **?** find that parental receipt of Norwegian disability insurance (DI) causes an approximately 200% increase in a child's later receipt of DI (compared to the mean rate of receipt); **?** are also able to rule out effects on another type of government assistance of more than 100%. SIME/DIME treatment increases the probability that a family receives any government benefits by 21 percentage points.[45] Thus, inflating estimates and standard errors in Table **??** by 1/.21 as a rough estimate, we can rule out (at the 95% level) that receipt of such cash assistance increased SSDI or SSI applications by more than 58% (of the mean application rate), and rule out increases in SSDI/SSI receipt of more than 77%. One possible reason that we do not find significant effects is mentioned by **?** in explaining the difference between their results on DI receipt and their results on other assistance: it may be that much of the intergenerational effect is due to transfers of information about particular government programs. Information about SIME/DIME would not be useful to a child in applying for SSDI or SSI, so the null effect we find may support the hypothesis that any intergenerational effects act more through information transfers than through effects on beliefs about working or the stigma associated with benefits. This is an important distinction: increasing information about benefits may be considered by some policymakers as a good outcome if it helps those who need them receive benefits, while reducing desire to work or the stigma associated with government benefits may be viewed more negatively by some.

Other research indicates that parental benefits can have an effect on employment, or outcomes related to employment. **?** find that parental DI receipt decreases a child's propensity to work by 14 percentage points. Again inflating estimates and standard errors in Table **??** by 1/.21, we can rule out (at the 95% level) that receipt of parental assistance decreases probability to work in a year by more than 7.4 percentage points. On the other hand, **?** find that a $1,000 increase in income from the Earned Income Tax Credit (EITC) raises test scores by 6% of a standard deviation. SIME/DIME treatment increased the amount of government benefits families received by about $3,800; thus inflating estimates and standard errors in Table **??** by 1,000/3,800, we can rule out that $1,000 in benefits increased income by .9% of a standard deviation. One possible reason for the different results is that government benefits that encourage parents to work less also encourage

[45]This includes any government benefits in all SIME/DIME records. Restricting the analysis to income-dependent cash assistance (that is, AFDC, unemployment benefits, and SIME/DIME payments) increases the effect to 33 percentage points; restricting to only AFDC or SIME/DIME payments increases the effect to 46 percentage points. These larger effects on receipt of benefits would imply even more tightly-estimated null effects on children.

children to work less: disability insurance usually leads to complete withdrawal from work, while the EITC is designed to have a negative tax rate for many families to encourage work. (Of course, the EITC also includes a positive tax rate for those with moderate incomes.) Although we find no evidence that parental tax rate affects child earned income, our standard errors on this effect are somewhat large. Also, perhaps related to this disincentive effect, we did find that treatment actually decreased parent lifetime earnings; thus it is not clear that we should take treatment as a positive income shock at all. Another possible reason for differences with **?**'s results is that any effects of parent income on test scores may not carry over to long-term earnings.

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

6 Discussion and conclusion

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

We also find little evidence that any effect was passed on to their children. We can rule out effects in either direction on child applications for SSDI or SSI of more than 3 percentage points, and effects on child annual earned income of more than $1,500. While narrower confidence intervals would

be desirable, these findings—in the context of other literature—can improve our understanding of how cash assistance affects children. Although the standard errors may mask smaller causal effects, these results provide evidence that cash assistance may not have the large effects found in other contexts.

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

Lack of evidence also makes it difficult to compare our significant results for adults to the long-term effects of other government programs and policies, such as food stamps or public housing. More research on the long-term effects of other programs can help put the present results in their proper context, and better inform policymakers of the long-term trade-offs they face.

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

A Creating the sample of SIME/DIME participants

A.1 Finding families with matching patterns

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

To find families matching SIME/DIME patterns, we use public-record birth data from the WA DOH and restricted-use data on parent names from the SSA's Numident file. For each child-parent pair, we determine a set of "possible matches:" individuals from the birth records with individual characteristics matching those of SIME/DIME children.[46] So, to continue our example from above, we would look in WA DOH birth records for all boys born February 1, 1960 to 30-year-old mothers; boys born February 1, 1961 to 31-year-old mothers; and boys born February 1, 1961 to 32-year-old mothers. Then, for each head of household, we look for parent names that are common to possible matches for multiple children. In our example, then, one boy from each of the three lists of possible matches may have a mother named Jane Smith. In this case, we provisionally assume that Jane Smith is the SIME/DIME mother, and the three boys found in this way are her three children. Where necessary, we then match these individuals to SSNs.[47]

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.[48] Crucially, though, as discussed in Subsection **??**, this method allows us to estimate the probability that each match corresponds to an actual SIME/DIME family, thus minimizing spurious matches.

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

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

[48] Individuals in families with zero or one children are dropped from all analyses looking at matched data, including those that analyze the match rate.

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

Not everyone provisionally matched using the procedure discussed in Subsection **??** will be actual participants in SIME/DIME. In addition to actual SIME/DIME families, we may find other individuals through two channels: actual families with similar characteristics, and unrelated individuals with matching birthdays whose parents share a name. To determine which families are actually SIME/DIME families, we create a model of the matching procedure and estimate its parameters via maximum likelihood estimation (MLE). Note that this model does not capture all features of the match process; however, as discussed in Subsection **??**, it succeeds well enough to be useful for the purposes of this analysis.

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

Now, consider unrelated individuals with matching birthdays whose parents share a name. Suppose the match is based on N children, and each child i is found within n_i possible matches, where a "possible match" is defined as an observation where all variables match (as described in Footnote **??**). For a specific parent name that occurs with frequency f,[49] we would expect that name to appear $n_i f$ times. Assuming independence, the expected number of matches with that name would be approximately $\prod_i (n_i f) = f^N \prod_i (n_i)$. Thus from the true match probability and the two spurious match channels, for a specific name with frequency f, we expect to find $\tau f + \alpha\tau f + f^N \prod_i (n_i)$ matches with N individuals in the family.

We can also run a placebo test: add t days to everyone's birthday, and rerun the algorithms; then add $t+1$ days and rerun it; and so on, T times.[50] (In our analysis, we set $T = 50$; based on

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

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

results from the cross-validation procedure discussed below, this number is enough to accurately estimate parameters.) For T placebos, we expect to find $T\left(\alpha\tau f + f^N \prod_i(n_i)\right)$ matches. Thus putting placebos together with matches using the true birthdays, the fraction found using the true birthday would be

$$\text{Frac Sample} = \frac{\tau f + \alpha\tau f + f^N \prod_i(n_i)}{\tau f + \alpha\tau f + f^N \prod_i(n_i) + T\left(\alpha\tau f + f^N \prod_i(n_i)\right)} = \frac{1+\alpha+\beta f^{N-1}}{1+(T+1)(\alpha+\beta f^{N-1})}, \quad (5)$$

where $\beta \equiv \frac{\prod_i(n_i)}{\tau}$.[51] 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.[52] From this we calculate the probability that each match is a SIME/DIME family as

$$\text{Prob SIME/DIME} = \frac{1}{1+\alpha+\beta f^{N-1}} \quad (6)$$

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

A.3 Matching results

In our main sample, we only include matches if we calculate that they are true SIME/DIME participants with at least 95% probability. (We also show robustness checks with 75% and 99% probability thresholds.) With this threshold, 45% of parents and 59% of children in SIME/DIME

[51] In principle, n_i could be calculated using the data by observing how many potential matches there are. In practice, this would be difficult because, for computational ease, the sample is selected across multiple programs, and the number of potential matches depends on the sample selection procedure. Additionally, without prior knowledge of τ, that knowledge is not useful.

[52] Because the parameters could be different for different types of matches, the algorithm is run separately for each site; for each data set (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.

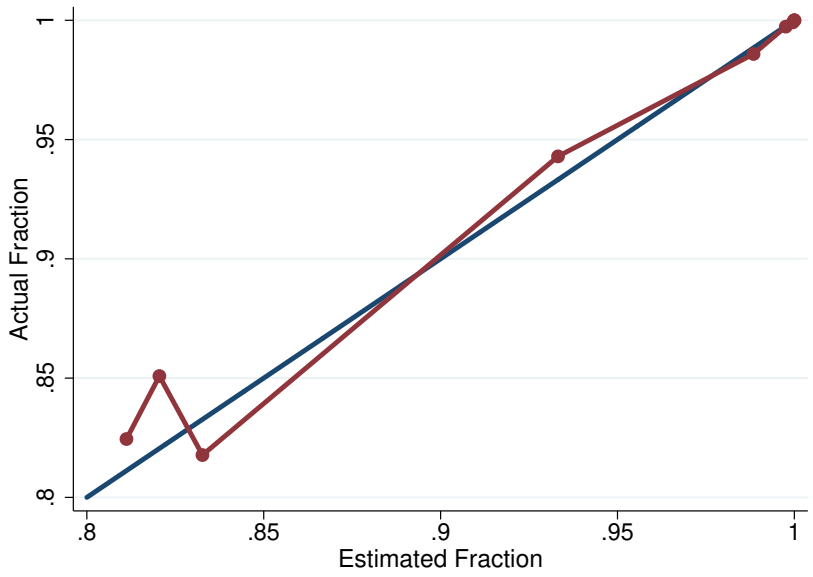


Figure A.1: Cross-validation of MLE predictions

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

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

Table A.1: Parents, effect on match rate

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

are matched to a Social Security number (SSN).[53] Importantly, as shown in Table **??**, treatment is not significantly correlated with match probability, after controlling for assignment group. Tables **??** and **??** show that treatment is also not correlated with match probability within various subgroups of the population. Indeed, it is unlikely that treatment would have much effect on match probability, which is based on administrative records. However, we do discuss this possibility in Subsection **??**.

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 would attenuate results toward zero, which could lead to the null results we find. Two pieces of evidence convince us that there is little attenuation. First, only 1.1% of adults and .45% of children are matched to multiple SSNs.[54] 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

[53] 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.

[54] 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.

matched to multiple SSNs as D; this is the duplicate rate reported above. Assuming that $Y \ll X$ and that the sample size is large, we can approximate $C \approx Y/X$ and $D \approx XY$, so $C \approx D/X^2$. With the same assumptions, we can approximate X as our match rate, so that the fraction of spurious matches is just the duplicate rate divided by the match rate squared. From this, we estimate that only 5.2% of matched adults and 1.3% of matched children are matched to an incorrect SSN. That fraction for parents is comparable to a match to SSA data based on name and birthday alone, and for children it is better than such a match. (In our regression analyses, we exclude any duplicate matches.)

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

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

Suppose that, within each race R, participants are drawn randomly from the general population. That would mean that, for any name N, the probability $\mathbf{P}(\text{name} = N|\text{race} = R)$ that a SIME/DIME participant of race R has name N is the same as in the general population. For common names, $\mathbf{P}(N|R)$ is available from the Census,[55] so we can match that with the names of participants we find using the SSA Numident file. Using Bayes' Theorem,

$$\mathbf{P}(R|N) = \frac{\mathbf{P}(N|R)\mathbf{P}(R)}{\sum_r \mathbf{P}(N|\text{race} = r)\mathbf{P}(\text{race} = r)}, \tag{7}$$

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

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

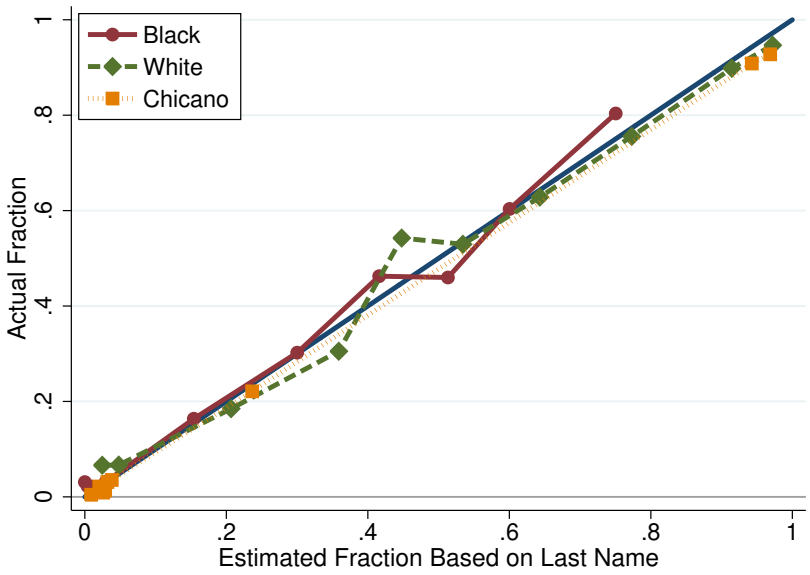


Figure A.2: Correspondence between SSA and MLE data

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

A.4 Differential attrition

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

There are three basic reasons that treatment could affect the probability of a match: fertility, mobility, and SSN applications. First, treatment could affect the probability that a new child appears in the SIME/DIME data, either through changes in actual fertility or because treated and control families have different incentives to report births. Based on original SIME/DIME data on all original household heads, 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. **?** 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.[56] A third confounding factor is SSN applications. Although each participating adult needed an SSN, it is possible that the treatment could affect changes to SSA records, such as name changes. Additionally, many participating children likely did not have SSNs when the experiment began, particularly because all children studied were under 18 at that time and SSN enumeration at birth did not begin until about twenty years later. Thus treatment could have affected the probability that a child would apply for an SSN. Although there is no evidence for such an effect, if it did occur, it could change the probability that a match would be made.

To account for most of these possibilities, we can rerun the match algorithm in two ways. First, we can leave out all records of children born after the experiment began; see Tables **??** and **??**, 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.

[56] 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.

These restrictions reduce the sample size and thus increase standard errors of the estimates. They also change the population that it is possible to match, so the underlying parameters might be different for this group. However, the estimates are very similar under either restriction.

We do not have a robustness check excluding SSA data on children collected after the experiment began because excluding this data allows us to match only about 1% of participants. However, it is unlikely that this channel will affect our results because almost all children likely got an SSN at 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 **?**, SSNs have increasingly been required for many other government and private services, from food stamps and welfare to bank accounts and student loans. Thus it is unlikely that a substantial fraction of the SIME/DIME child population would have avoided getting an SSN.

B Additional results

This appendix includes additional tables and figures for which there was not enough space in the paper. Table **??** displays causal effects on outcomes other than the main outcomes for parents, while Table **??** does the same for children.

A variety of robustness checks are shown in Tables **??** (for parents) and **??** (for children). First, as noted in Subsection **??**, there is better evidence for treatment/control balance in Denver; thus the row labeled "Denver Only" restricts the sample to that site. In our baseline specification, we include a variety of controls, both to make estimates more precise and, in the case of controlling for pre-experimental earnings, to improve exogeneity. As a robustness check, regressions listed as including "No ___ Control" do not control for the given variable. Our main data source, from **??**, includes data on 9 months of pre-experimental income for all families. Additionally, **?** includes pre-experimental income data for the previous 3 years for some families, though it is not clear why data is missing for others. "Control for Earn in All Years" includes controls for each of these four years of pre-experimental income, where that data is available. "Control for Any Pre-exp Earn" controls for the level of the pre-experimental income (only from the main 9-month interval) and a dummy for having any such income. As discussed in Subsection **??**, it is theoretically possible that treatment could affect birth rates, or propensity to change SSA records, in a way that would affect the match rate. To control for this, "No Post-Exp Births" does not use children born after the experiment began in matching; "No Post-Exp Births Or Parent Records" additionally does not use adult SSA records from after the experiment began for matching. In our baseline specification, we include individuals if we are at least 95% confident that they are correct matches; "75% Conf Sample" and "99% Conf Sample" include individuals matched to SSNs with the given different confidence level. To test whether treatment changed the hazard rate of effects, rather than simply their probability of occurrence, "Cox Model" uses a Cox proportional hazard mode— rather than OLS—for the first time that an event occurred in our sample. Finally, as noted in Footnote **??**, some families were told they would receive treatment for 20 years, but in fact received a much shorter treatment. In our baseline specification, we drop these families; "Include 20-Yr Sample" includes them.

Effects for various subgroups of the population are shown in Tables **??** (for parents) and **??** (for children). Effects a given number of years after the experiment began are shown in Figures **??** (for parents) and **??** (for children), while effects at a given age are shown in Figures Figures **??** (for

parents) and **??** (for children). Average values of dependent variables a given number of years after the experiment began are shown in Figures **??** (for parents) and **??** (for children), while average values at a given age are shown in Figures **??** (for parents) and **??** (for children).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Dep Var	**Applied SSDI**	**Applied SSI**	**Awarded SSDI**	**Awarded SSI**	**Married**	**Ever Split or Divorced**	**Died (WA DOH)**	**Positive Annual Self-Empl Earnings**	**Annual Self-Empl Earnings**	**Earn > 10k**	**Earn > 20k**	**Earn > 50k**	**Ln(Earn +1k), by Year**
Treated	.0577***	.0308**	.0335*	−.0013	.0099	.0105	−.00544	.00331	197	−.0335**	−.027*	−.0219	−.129**
	(.0192)	(.0155)	(.0178)	(.0124)	(.0275)	(.0438)	(.028)	(.0063)	(211)	(.0152)	(.0153)	(.0144)	(.0542)
Dep var summary stats													
Mean	.272	.148	.2	.0934	.196	.412	.346	.0456	704	.579	.474	.359	9.17
Std. Dev.	.445	.355	.4	.291	.397	.492	.476	.209	6686	.494	.499	.48	1.68
N	2280	2280	2280	2280	997	758	997	52867	52867	52867	52867	52867	52867
People	2280	2280	2280	2280	997	758	997	2252	2252	2252	2252	2252	2252
Clusters	1720	1720	1720	1720	727	488	727	1699	1699	1699	1699	1699	1699

Table B.1: Parents, other variables

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Usual	−.00125	−.0329**	−1761**	.0628***	.0216	.0138
	(.0158)	(.0136)	(816)	(.0199)	(.019)	(.0196)
Denver Only	−.00734	−.029	−1765*	.056**	.00972	.0275
	(.0204)	(.0184)	(1003)	(.0282)	(.0266)	(.0264)
No Manpower	.000921	−.0314**	−1756**	.0616***	.0214	.0155
Control	(.0156)	(.0135)	(803)	(.0198)	(.0188)	(.0194)
No Age/Sex	.0036	−.0323**	−1535*	.0693***	.0248	.0094
Control	(.016)	(.0139)	(835)	(.0204)	(.0191)	(.0226)
No Earnings	−.00219	−.0398***	−2418***	.0649***	.0224	.0179
Control	(.0158)	(.0138)	(842)	(.0199)	(.019)	(.0197)
Control for Earn	.0000964	−.0373***	−1853**	.0509**	.0128	.0065
in All Years	(.0162)	(.014)	(844)	(.0206)	(.0196)	(.0202)
Control for Any	−.00116	−.033**	−1763**	.0628***	.0216	.0138
Pre-exp Earn	(.0158)	(.0136)	(817)	(.0199)	(.019)	(.0196)
No Post-Exp	−.00031	−.0192	−1612*	.0412*	.00331	−.0126
Births	(.0151)	(.0172)	(959)	(.0233)	(.0222)	(.0236)
No Post-Exp Births	.000398	−.02	−1914*	.0511**	.00921	−.00424
Or Parent Recs	(.0141)	(.0183)	(1003)	(.0248)	(.0236)	(.0256)
75% Conf	−.00463	−.0286**	−1514*	.0615***	.0231	.0129
Sample	(.0159)	(.0132)	(798)	(.0195)	(.0186)	(.0191)
99% Conf	−.00431	−.0303**	−1598*	.0616***	.0154	.0137
Sample	(.0155)	(.014)	(843)	(.0205)	(.0195)	(.0202)
Cox				.255***	.0863	.0538
Model				(.0842)	(.0933)	(.0732)
Include	.000286	−.0313**	−1833**	.0615***	.0236	.00798
20-Yr Sample	(.0154)	(.0133)	(799)	(.0195)	(.0185)	(.0193)

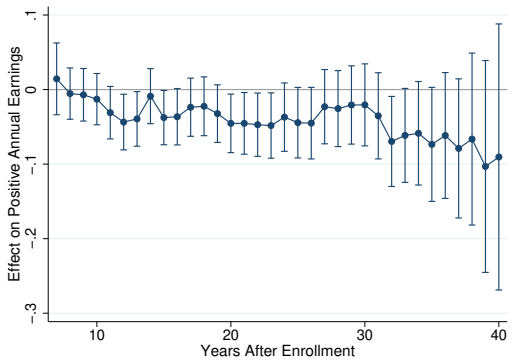
Table B.2: Parents, robustness checks

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, with the methodology given by the row. Regressions listed as including "No ___ Control" do not control for the given variable. "Control for Earn in All Years" includes controls for four years of pre-experimental income, where that data is available. "Control for Any Pre-exp Earn" controls for the level of pre-experimental income and a dummy for having any such income. "No Post-Exp Births" does not use children born after the experiment began in matching; "No Post-Exp Births Or Parent Records" additionally does not use adult SSA records from after the experiment began for matching. "75% Conf Sample" and "99% Conf Sample" include individuals matched to SSNs with the given confidence level, rather than the standard 95%. "Cox Model" uses a Cox proportional hazard model rather than OLS for the first time that an event occurred. "Include 20-Yr Sample" does not drop families who were told they would receive financial treatment for 20 years.

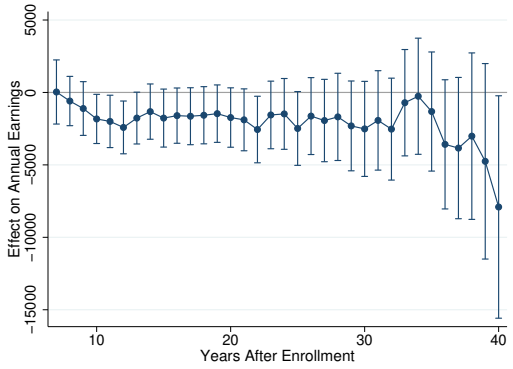
	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
All	−.00125	−.0329**	−1761**	.0628***	.0216	.0138
	(.0158)	(.0136)	(816)	(.0199)	(.019)	(.0196)
Fam Inc	−.0402	.0223	942	.0703	−.0124	−.0414
< $14k	(.0354)	(.0361)	(1609)	(.0515)	(.0492)	(.0462)
Fam Inc	.000592	−.041**	−2942**	.0812***	.0518*	.0134
$14k - 32k	(.0232)	(.0207)	(1270)	(.0302)	(.0292)	(.0294)
Fam Inc	.0146	−.0494**	−1670	.0354	.000887	.0386
$32k +	(.0272)	(.0202)	(1347)	(.0309)	(.0286)	(.0322)
Female	−.00774	−.0425**	−2074**	.0498*	.00257	−.00799
	(.0176)	(.0188)	(922)	(.0265)	(.0252)	(.026)
Male	.0106	−.0194	−1585	.0807***	.0464	.0439
	(.0239)	(.0193)	(1495)	(.0304)	(.0289)	(.0302)
Black	−.00983	−.033	−2646*	.0655*	.0658*	.0227
	(.0251)	(.0275)	(1552)	(.0387)	(.0366)	(.0376)
White	−.00201	−.0512***	−2688**	.0712***	.00924	.0398
	(.0255)	(.0172)	(1191)	(.0264)	(.0254)	(.0264)
Chicano	.0208	.00699	1484	.0408	−.0166	−.052
	(.0325)	(.0329)	(1555)	(.0496)	(.0469)	(.0455)
Single	−.0163	−.0469	−4828***	.0658	.0437	−.0339
Parents	(.0271)	(.0301)	(1715)	(.0428)	(.0399)	(.0425)
Married	.00421	−.0304**	−1078	.0605***	.0129	.0246
Parents	(.0191)	(.0152)	(920)	(.0226)	(.0216)	(.0221)
2 Child	−.00167	−.0437**	−2254*	.085**	.0551*	.0175
Family	(.0254)	(.0217)	(1348)	(.0336)	(.0319)	(.0329)
3 Child	.014	−.0295	349	.0044	−.00796	−.0237
Family	(.0278)	(.0234)	(1487)	(.0371)	(.0343)	(.0342)
4+ Child	−.0271	−.0198	−2520*	.0291	−.0377	.0342
Family	(.0288)	(.0273)	(1424)	(.0378)	(.0365)	(.0409)
Denver	−.00734	−.029	−1765*	.056**	.00972	.0275
	(.0204)	(.0184)	(1003)	(.0282)	(.0266)	(.0264)
Seattle	.0047	−.0365*	−1469	.0699**	.0335	−.00236
	(.0248)	(.0203)	(1368)	(.0279)	(.027)	(.0293)

Table B.3: Parents, effects within subgroups

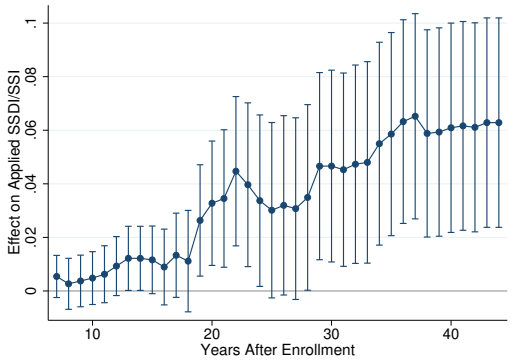
Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, for the subgroup given by the row. "Fam Inc" levels are based on pre-experimental normal income categories. Marital status is based on pre-experimental data. Number of children in the family is based on all children whom it would be possible to match with our methodology.



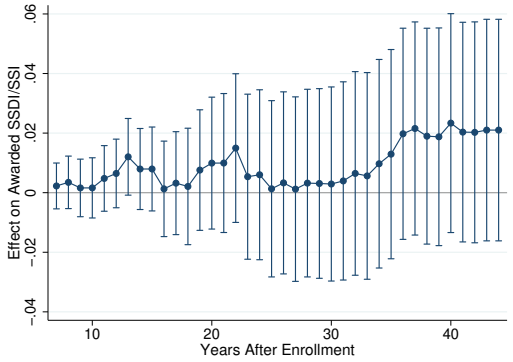
(a) Positive earnings



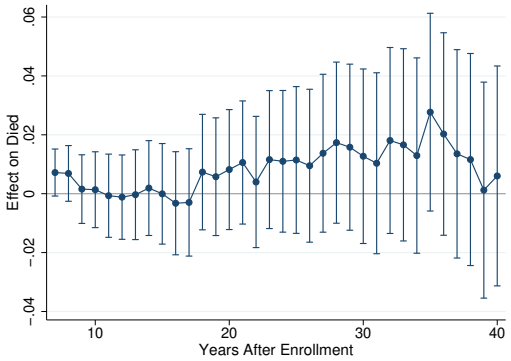
(b) Total earnings



(c) Applied for SSDI/SSI



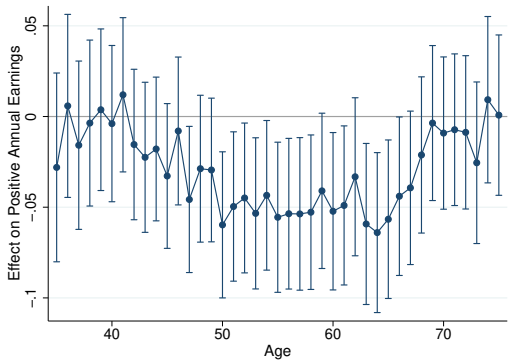
(d) Awarded SSDI/SSI



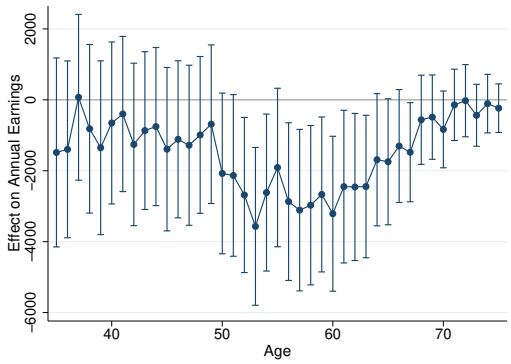
(e) Died

Figure B.1: Parents, effects by years after start of experiment

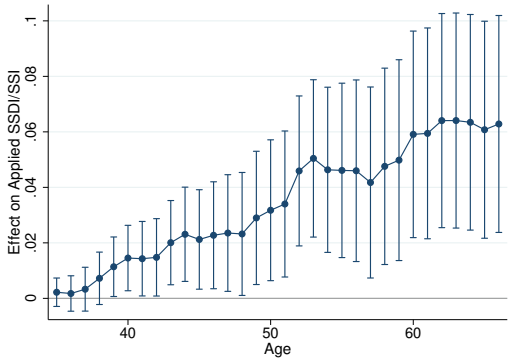
Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from a certain number of years into the experiment. Confidence intervals are based on standard errors that are clustered 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 pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. 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.



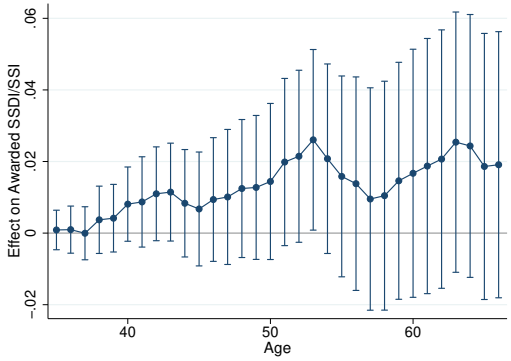
(a) Positive earnings



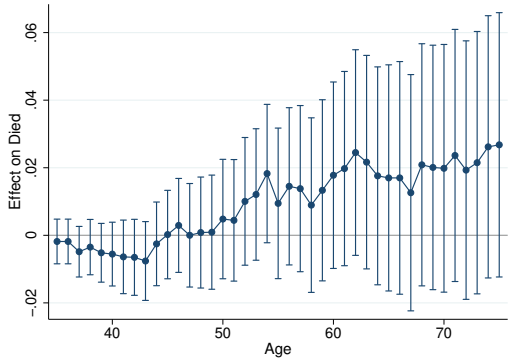
(b) Total earnings



(c) Applied for SSDI/SSI



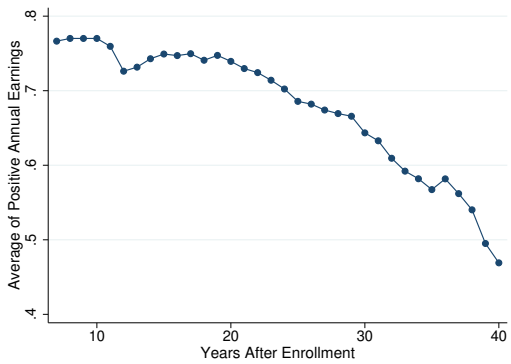
(d) Awarded SSDI/SSI



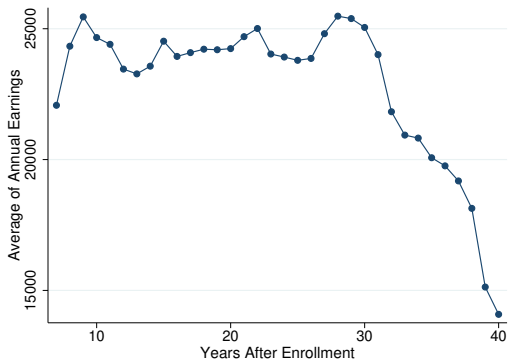
(e) Died

Figure B.2: Parents, effects by age

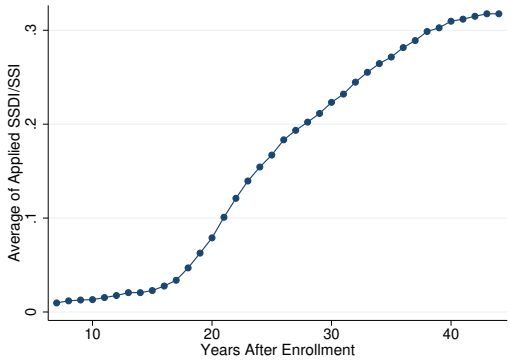
Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from individuals when they are a certain age. Confidence intervals are based on standard errors that are clustered at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



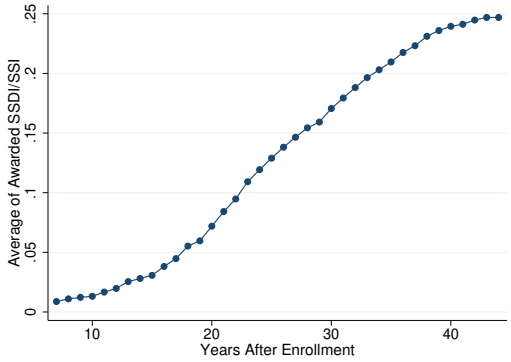
(a) Positive earnings



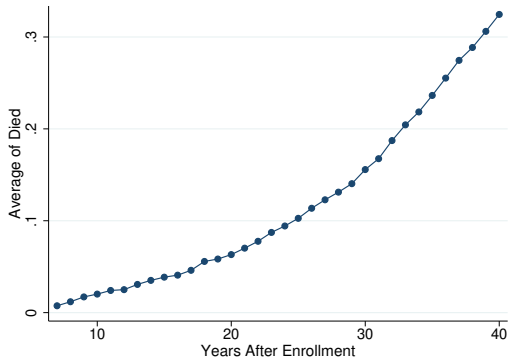
(b) Total earnings



(c) Applied for SSDI/SSI



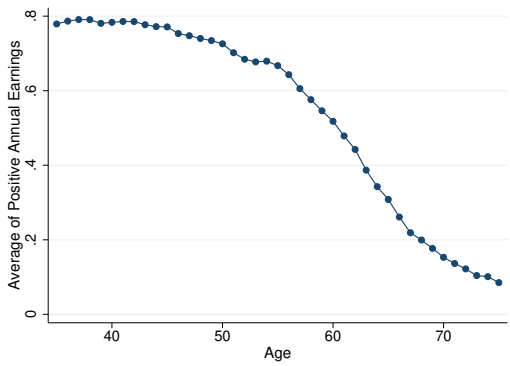
(d) Awarded SSDI/SSI



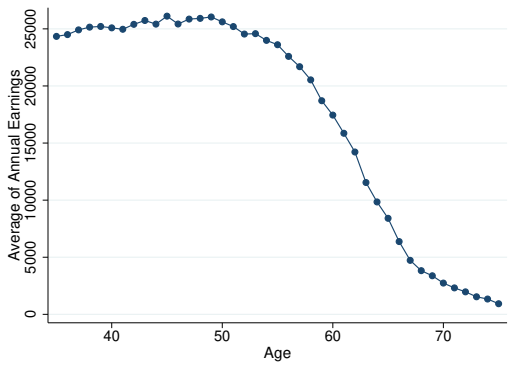
(e) Died

Figure B.3: Parents, average values by years after start of experiment

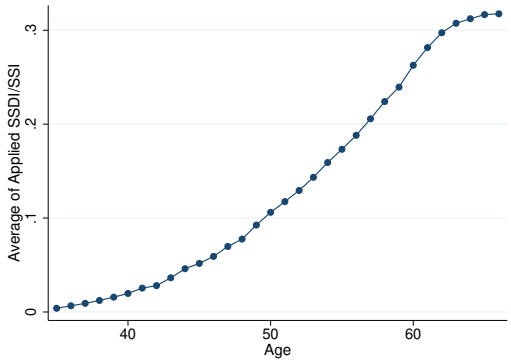
Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from a certain number of years into the experiment.Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. All 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.



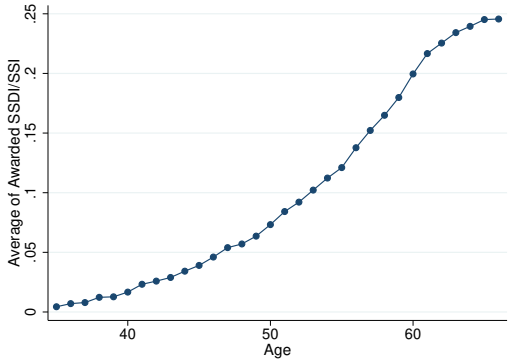
(a) Positive earnings



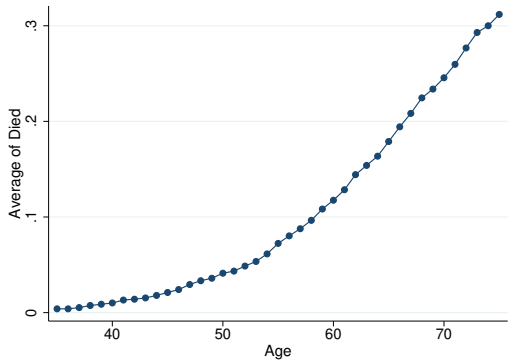
(b) Total earnings



(c) Applied for SSDI/SSI



(d) Awarded SSDI/SSI



(e) Died

Figure B.4: Parents, average values by age

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from individuals when they are a certain age.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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Dep Var	**Applied SSDI**	**Applied SSI**	**Awarded SSDI**	**Awarded SSI**	**Married**	**Divorced**	**Died (WA DOH)**	**Died**	**Positive Annual Self-Empl Earnings**	**Annual Self-Empl Earnings**	**Earn > 10k**	**Earn > 20k**	**Earn > 50k**	**Ln(Earn +1k), by Year**
Treated	.0148	−.00711	.00729	−.00793	.0219	−.0148	.0116	.00724	.00223	−134	.00312	.000385	−.0048	.00177
	(.0112)	(.0113)	(.00853)	(.00653)	(.0231)	(.0204)	(.0102)	(.00729)	(.00313)	(127)	(.011)	(.0109)	(.00996)	(.039)
Dep var summary stats														
Mean	.184	.192	.102	.0571	.459	.247	.0629	.0728	.041	625	.574	.438	.31	9.19
Std. Dev.	.388	.394	.303	.232	.498	.432	.243	.26	.198	8252	.495	.496	.463	1.57
N	5658	5658	5658	5658	2385	2385	2385	5658	163340	163340	163340	163340	163340	163340
People	5658	5658	5658	5658	2385	2385	2385	5658	5658	5658	5658	5658	5658	5658
Clusters	2101	2101	2101	2101	893	893	893	2101	2101	2101	2101	2101	2101	2101

Table B.4: Children, other variables

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

	(1)	(2)	(3)	(4)	(5)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
Usual	−.00623	.00177	−356	.00537	.0018
	(.0167)	(.00872)	(601)	(.0125)	(.00962)
Denver Only	−.00952	.00588	−428	−.00519	−.00461
	(.0221)	(.0111)	(768)	(.0159)	(.0115)
No Manpower	−.0049	−.0000389	−447	.00575	.000677
Control	(.0165)	(.00868)	(595)	(.0123)	(.00954)
No Age/Sex	−.0035	.00377	−272	.00304	−.000805
Control	(.0168)	(.00885)	(605)	(.0128)	(.00988)
No Earnings	−.00711	−.000356	−506	.00602	.00146
Control	(.0166)	(.00874)	(602)	(.0125)	(.00964)
Control for Earn	.00232	.00256	−213	.00274	.00241
in All Years	(.0171)	(.00888)	(614)	(.0127)	(.00978)
Control for Any	−.00594	.00166	−363	.00554	.00196
Pre-exp Earn	(.0166)	(.00872)	(601)	(.0124)	(.00963)
No Post-Exp	−.00232	.00226	55.9	−.00871	−.00274
Births	(.0157)	(.0102)	(706)	(.0156)	(.0123)
No Post-Exp Births	−.0105	−.00228	−510	.00589	−.00367
Or Parent Recs	(.0154)	(.0115)	(804)	(.0168)	(.0132)
75% Conf	−.00442	.000108	−774	.012	.000764
Sample	(.0163)	(.00865)	(590)	(.0123)	(.0096)
99% Conf	−.00344	.000918	−415	.00935	.00153
Sample	(.0169)	(.00895)	(616)	(.0126)	(.00956)
Cox				.0148	.0106
Model				(.0622)	(.0837)
Include	−.00412	.0000804	−457	.00341	.00123
20-Yr Sample	(.0159)	(.00853)	(590)	(.0122)	(.00934)

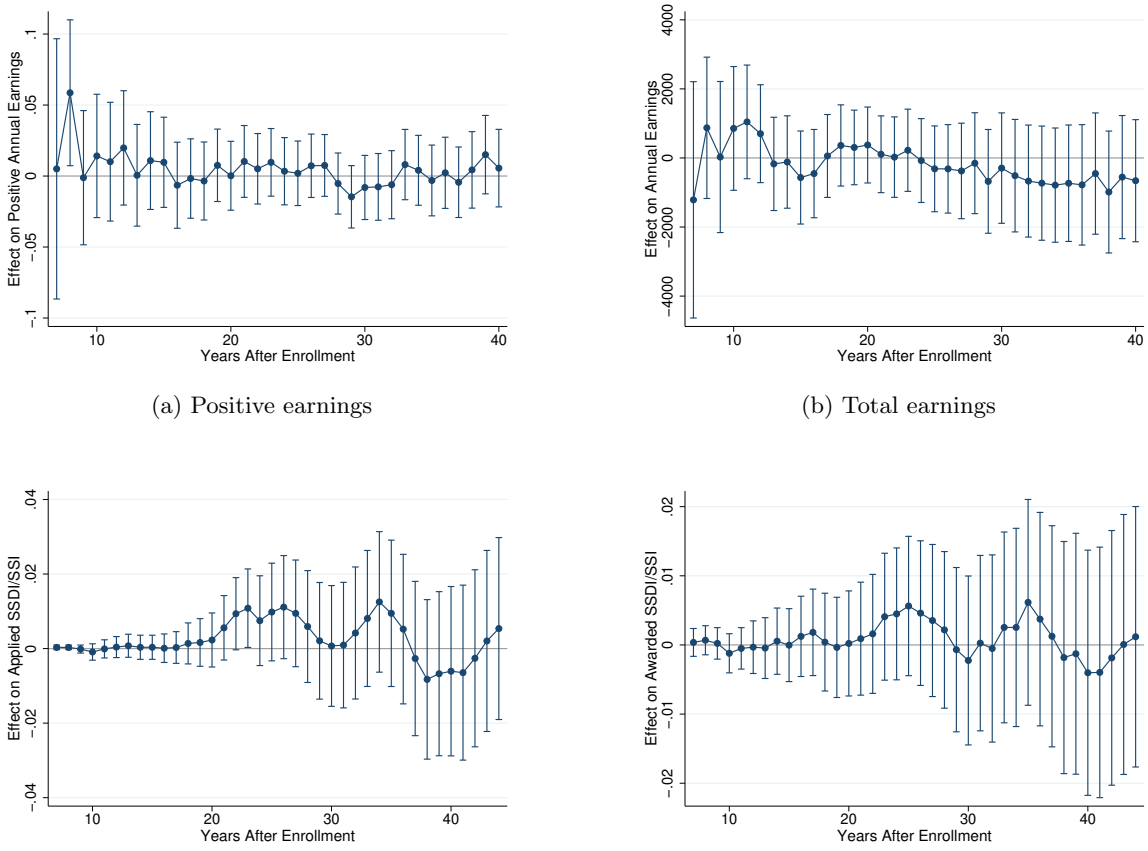
Table B.5: Children, robustness checks

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, with the methodology given by the row. Regressions listed as including "No ___ Control" do not control for the given variable. "Control for Earn in All Years" includes controls for four years of pre-experimental income, where that data is available. "Control for Any Pre-exp Earn" controls for the level of pre-experimental income and a dummy for having any such income. "No Post-Exp Births" does not use children born after the experiment began in matching; "No Post-Exp Births Or Parent Records" additionally does not use adult SSA records from after the experiment began for matching. "75% Conf Sample" and "99% Conf Sample" include individuals matched to SSNs with the given confidence level, rather than the standard 95%. "Cox Model" uses a Cox proportional hazard model rather than OLS for the first time that an event occurred. "Include 20-Yr Sample" does not drop families who were told they would receive financial treatment for 20 years.

	(1)	(2)	(3)	(4)	(5)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
All	−.00623	.00177	−356	.00537	.0018
	(.0167)	(.00872)	(601)	(.0125)	(.00962)
Fam Inc	−.00441	−.00378	−197	−.00814	.0176
< $14k	(.0344)	(.02)	(1063)	(.029)	(.0222)
Fam Inc	−.0138	.00209	−562	.00581	−.000668
$14k - 32k	(.0245)	(.0128)	(905)	(.0181)	(.0138)
Fam Inc	.00546	.005	−245	.0138	−.00546
$32k +	(.0303)	(.0145)	(1145)	(.0207)	(.0165)
Female	−.0206	.0116	88	−.00631	−.00931
	(.0194)	(.0113)	(704)	(.0175)	(.0131)
Male	.00689	−.00443	−557	.0186	.0128
	(.0198)	(.0114)	(831)	(.0171)	(.0133)
Black	−.00177	−.00607	−531	.0141	.0211
	(.0271)	(.0158)	(995)	(.0233)	(.0181)
White	−.007	−.00699	−1549	.0237	.00664
	(.026)	(.0125)	(950)	(.0174)	(.0137)
Chicano	−.00773	.0345*	2631**	−.0461*	−.0392**
	(.0356)	(.0178)	(1143)	(.0261)	(.019)
Single	−.0209	−.0224	−1702*	−.0109	.000956
Parents	(.0275)	(.0148)	(996)	(.0228)	(.0178)
Married	.00393	.014	320	.0132	.00269
Parents	(.0209)	(.0107)	(748)	(.0147)	(.0114)
2 Child	−.0143	.0169	467	.0253	.00905
Family	(.0287)	(.016)	(1173)	(.0228)	(.0167)
3 Child	−.00632	−.00774	−533	.0045	−.00459
Family	(.0285)	(.0151)	(1066)	(.0213)	(.0164)
4+ Child	−.00782	.0076	−58.7	−.00506	−.00479
Family	(.0273)	(.0136)	(886)	(.0201)	(.0163)
Denver	−.00952	.00588	−428	−.00519	−.00461
	(.0221)	(.0111)	(768)	(.0159)	(.0115)
Seattle	−.00605	−.00229	−176	.0226	.0111
	(.0253)	(.0139)	(961)	(.02)	(.0165)
Age	−.0209	−.0283	−2855**	.03	−.00934
≤ 0	(.0321)	(.019)	(1379)	(.0262)	(.0191)
Age	−.00314	.00987	−445	−.000717	−.00978
1 - 5	(.024)	(.0134)	(917)	(.0201)	(.0155)
Age	.0164	.00579	53.5	.00788	.0179
6 - 10	(.0259)	(.0162)	(1050)	(.0259)	(.0189)
Age	−.0142	.00151	412	−.0117	−.0000487
11+	(.0259)	(.016)	(1095)	(.0259)	(.0218)

Table B.6: Children, effects within subgroups

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, for the subgroup given by the row. "Fam Inc" levels are based on pre-experimental normal income categories. Marital status is based on pre-experimental data. Number of children in the family is based on all children whom it would be possible to match with our methodology. Age is counted from the start of the experiment in each site.



(a) Positive earnings

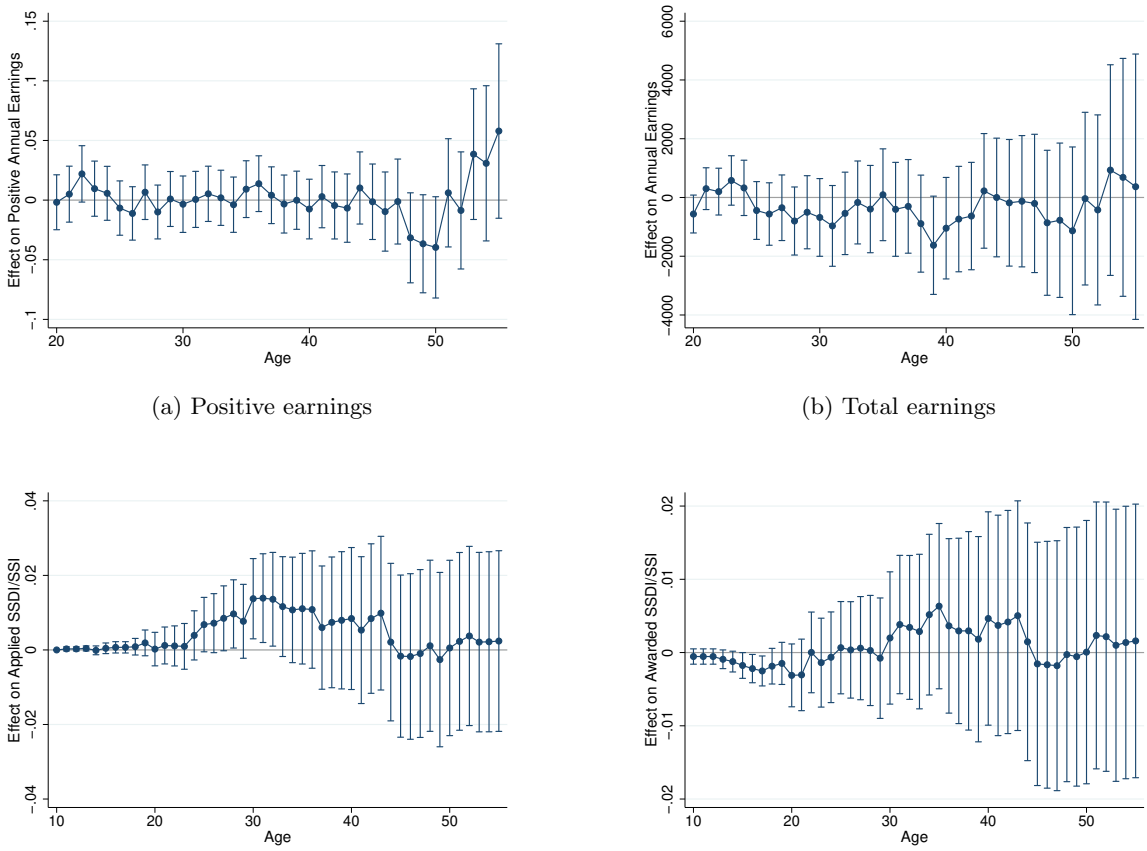
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure B.5: Children, effects by years after start of experiment

Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from 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 pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. 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.



(a) Positive earnings

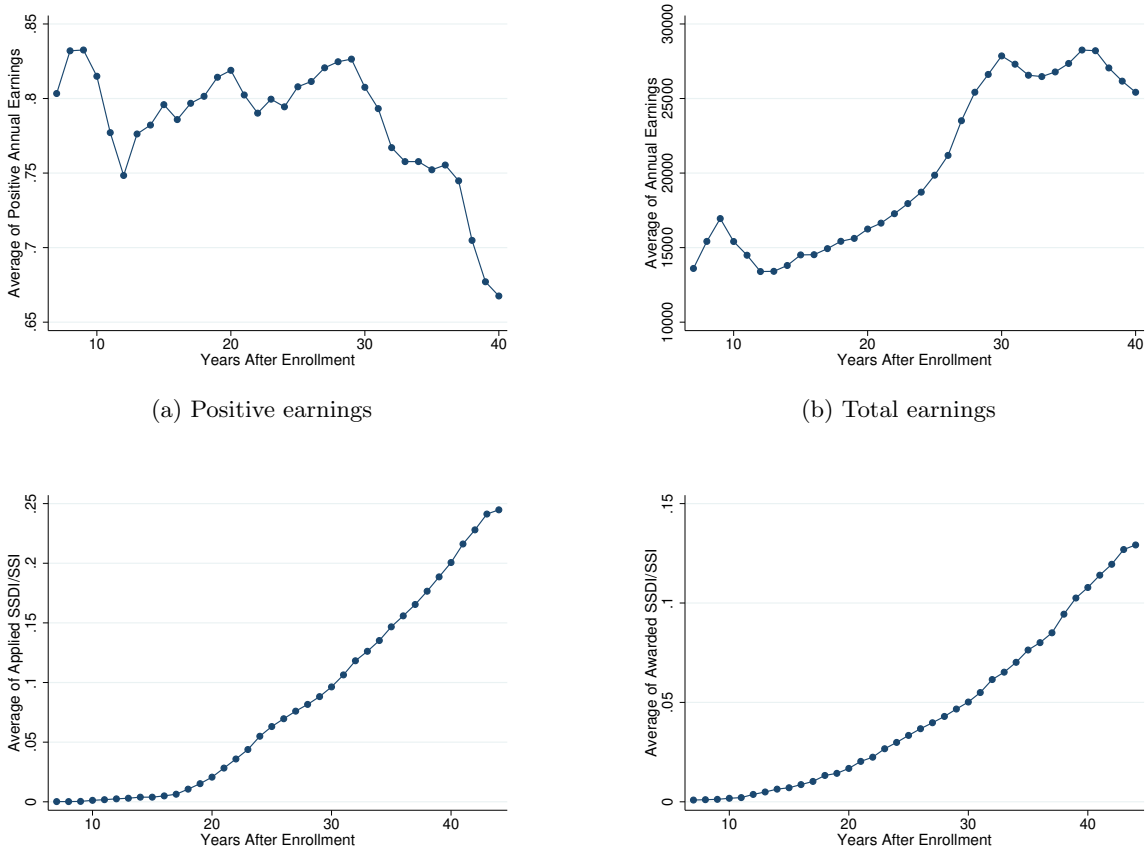
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure B.6: Children, effects by age

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



(a) Positive earnings

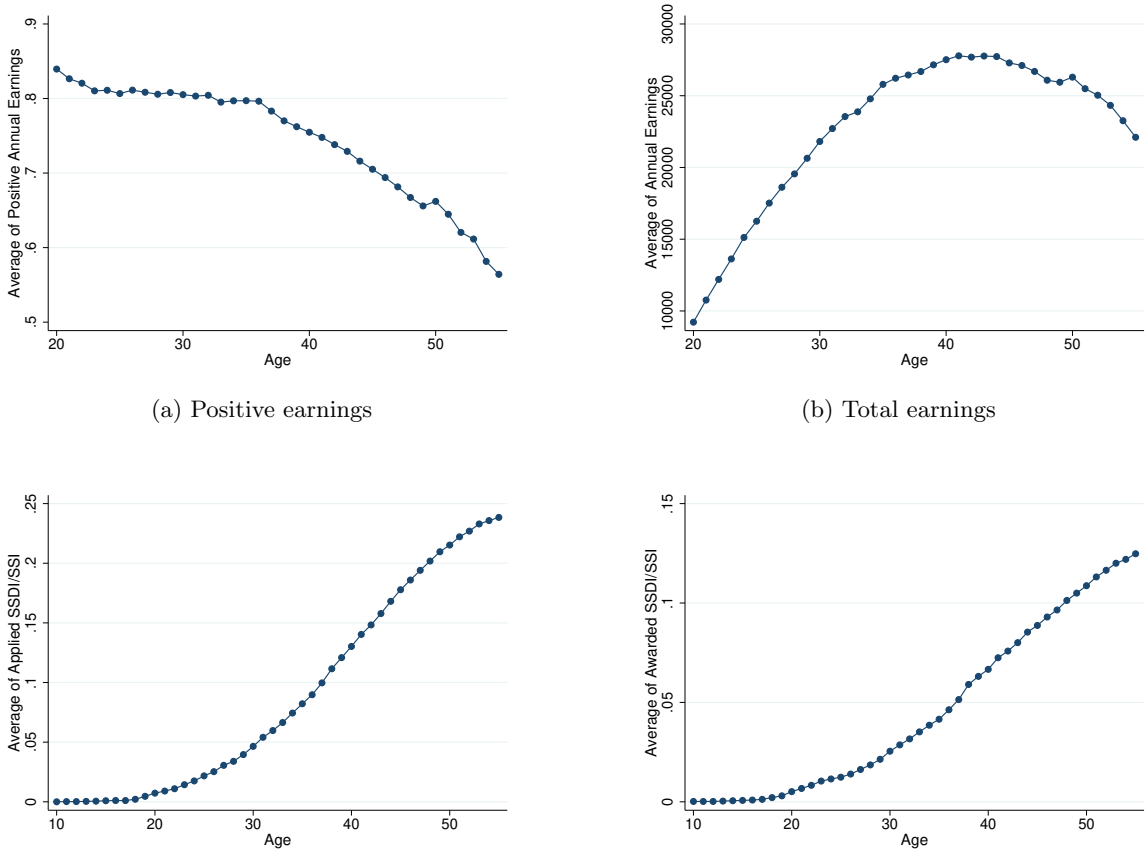
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure B.7: Children, average values by years after start of experiment

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from a certain number of years into the experiment.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.



(a) Positive earnings

(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure B.8: Children, average values by age

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from individuals when they are a certain age.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.

C Results from Denver only

As noted in Subsection **??**, there is some evidence that assignment to treatment status was not random in Seattle. No such evidence exists for lack of random assignment in Denver, where randomization occurred separately, at a later date. Because of that, results in Denver are taken as a check on overall results, because they may be less affected by pre-existing differences. Of course, results in Denver may differ because of other differences between the two cities or the experimental population used in each, or due to chance. Additionally, the sample size in Denver is only about half as large as the entire sample, so measurements are less precise. For this reason, we use the full sample as the primary measure.

Results restricted to Denver appear in the tables and figures of this appendix. In general, results restricted to Denver are very similar to results for the full sample: there is evidence for treatment causing adults to apply for disability benefits and earn lower incomes, with no significant effects on children. These tables and figures are described below.

Table **??** shows summary statistics for participants and demographically similar comparisons in Colorado. Tables **??** and **??** show main results for parents and children, respectively. Tables **??** and **??** show intensive margin results for a variety of variables. Table **??** shows how adult results vary by the performance of the individual's industry in the 1970s. Tables **??** and **??** show how effects vary with different types of treatment.

We next turn to Denver matching results, comparable to results shown in Appendix **??**. Figure **??** shows the results of a cross-validation exercise for the MLE procedure. Table **??** shows the matching results, and Figure **??** shows the correspondence between actual race (based on SIME/DIME data) and predicted race (based on last names from matched SSA data).

The remaining tables and figures are comparable to the additional results shown in Appendix **??**. Table **??** displays causal effects on outcomes other than the main outcomes for parents, while Table **??** does the same for children.

A variety of robustness checks are shown in Tables **??** (for parents) and **??** (for children).

Effects for various subgroups of the population are shown in Tables **??** (for parents) and **??** (for children). Effects a given number of years after the experiment began are shown in Figures **??** (for parents) and **??** (for children), while effects at a given age are shown in Figures Figures **??** (for parents) and **??** (for children). Average values of dependent variables a given number of years after the experiment began are shown in Figures **??** (for parents) and **??** (for children), while average

Variable	Parents			Children		
	Sample Mean	Comp Mean	p-value	Sample Mean	Comp Mean	p-value
Positive Annual Earnings	.711	.703	0.458	.789	.8	0.098
Annual Earnings	22269	26617	0.000	21804	26908	0.000
Applied SSDI/SSI	.341	.19	0.000	.226	.145	0.000
Awarded SSDI/SSI	.257	.147	0.000	.112	.0842	0.000
Died	.356	.284	0.000	.073	.0587	0.019

Table C.1: Summary statistics based on outcome variables, Denver only

Notes: "Sample" refers to the same SIME/DIME matched sample described in Section **??**. 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. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.

values at a given age are shown in Figures **??** (for parents) and **??** (for children).

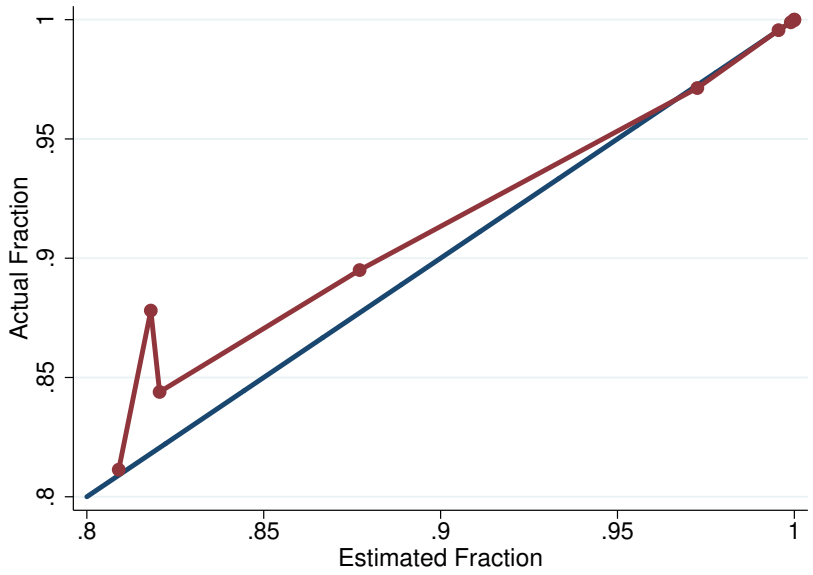


Figure C.1: Cross-validation of MLE predictions, Denver only

Notes: Families are randomized into two groups; MLE parameters are estimated with one group and probability of being in the non-placebo sample (i.e., matched with correct birthday rather than birthday plus an offset) is assigned to the other group based on these parameters. There may be multiple observations per person if one person is matched with multiple strategies (for example, using data from both the father and mother). Only observations with at least 75% chance of being from SIME/DIME are included. Observations are placed into deciles by probability of being in the sample; within each decile, average estimated probability of being in sample and fraction actually in sample are recorded. In this sample, the coefficient (and standard error) in a regression of actual fraction on estimated fraction is 0.830 (0.065). Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.

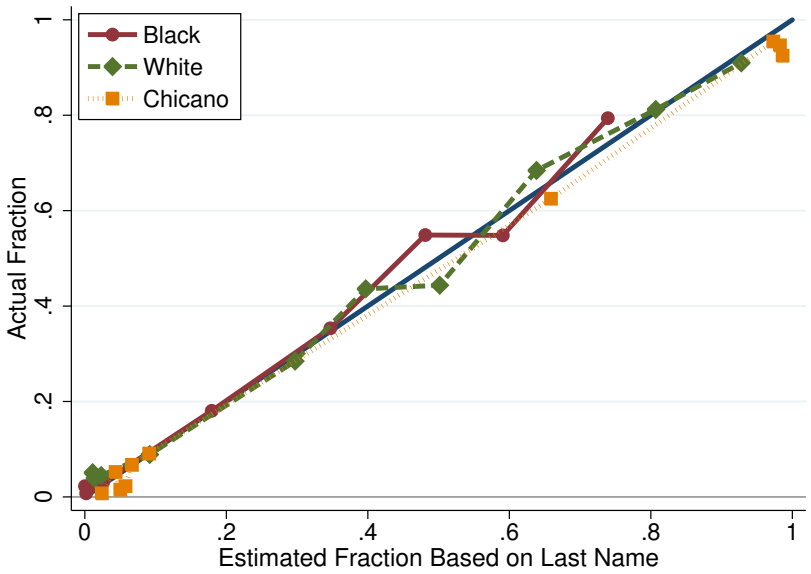


Figure C.2: Correspondence between SSA and MLE data, Denver only

Notes: Estimated fractions are based on the assumption that, within each race R, participants are drawn randomly from the general population. Based on that assumption, for an individual with name N, the estimated probability of being of a given race R is $\mathbf{P}(R|N) = \frac{\mathbf{P}(N|R)\mathbf{P}(R)}{\sum_r \mathbf{P}(N|\text{race}=r)\mathbf{P}(\text{race}=r)}$, where, for any name n and race r, $\mathbf{P}(n|r)$ is based on Census 2000 data on last names and race (black, white, or Hispanic), while $\mathbf{P}(r)$ is based on racial composition of the matched SIME/DIME sample (black, white, or Chicano). Only adults are considered. Coefficients (and standard errors) in a regression of actual fraction against estimated fraction are 1.000 (0.038) for black adults, 0.970 (0.026) for white adults, and 0.970 (0.015) for Chicano adults. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Treated	−.029	−1765*	.056**	.00972	.0275
	(.0184)	(1003)	(.0282)	(.0266)	(.0264)
Dep var summary stats					
Mean	.711	22269	.341	.257	.356
Std. Dev.	.454	23402	.474	.437	.479
N	31286	31286	1283	1283	1283
People	1276	1276	1283	1283	1283
Clusters	987	987	993	993	993

Table C.2: Parents, effects on main outcomes, Denver only

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Awarded SSDI/SSI**	**Cancer**	**Circul-atory Disorder**	**Musculo-skeletal Disorder**	**Mental Disorder**	**Other Impair-ment**
Condition	**Alive**	**Earn>0**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**
Treated	−.0308*	−1017	−.0945*	−.000926	−.0139	−.0104	−.0884**	−.0731
	(.0166)	(974)	(.0483)	(.0308)	(.0443)	(.0543)	(.0441)	(.0544)
Dep var summary stats								
Mean	.748	31342	.708	.0822	.199	.397	.205	.543
Std. Dev.	.434	22055	.455	.275	.399	.49	.405	.499
N	29631	22229	438	438	438	438	438	438
People	1268	1205	438	438	438	438	438	438
Clusters	983	943	390	390	390	390	390	390

Table C.3: Parents, intensive margins, Denver only

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

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Treated	−.0498**	−3286**	.0736**	.0293	.0443
	(.0218)	(1476)	(.0363)	(.035)	(.0347)
Treated ×	−.438***	−17788**	.316*	.29	−.132
Industry Δ Empl	(.118)	(7376)	(.19)	(.186)	(.177)
Dep var summary stats					
Mean	.759	27037	.31	.247	.396
Std. Dev.	.428	24792	.463	.432	.489
N	19705	19705	846	846	846
People	840	840	846	846	846
Clusters	743	743	748	748	748

Table C.4: Parents, differential effects by industry performance

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. "Industry Δ Empl" is the log change in employment in the individual's original industry, in the individual's state (Washington or Colorado), between 1970 and 1980 (according to the Decennial Census). The regression also includes this log change multiplied by dummy variables for the assignment groups, and multiplied by pre-experimental income. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.

	(1)	(2)	(3)	(4)	(5)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
5-Year Trtmnt	.00188	299	.119***	.0743**	.0236
	(.023)	(1282)	(.0376)	(.0347)	(.0347)
Guar Level	$-1.03e-06$	.11	$1.79e-06$	$9.84e-06^{*}$	$5.92e-06$
	(3.72e-06)	(.187)	(5.80e-06)	(5.51e-06)	(5.03e-06)
Tax Rate, $0	.0625	−472	−.133	−.237	−.181
	(.132)	(7366)	(.22)	(.202)	(.203)
Tax Decline?	−.00783	−595	.0642	.0549	.027
	(.0316)	(1646)	(.0493)	(.0464)	(.0462)
Manpower	.0161	1395	−.0279	−.0314	.00936
	(.0179)	(990)	(.0283)	(.0268)	(.0258)
Dep var summary stats					
Mean	.711	22269	.341	.257	.356
Std. Dev.	.454	23402	.474	.437	.479
N	31286	31286	1283	1283	1283
People	1276	1276	1283	1283	1283
Clusters	987	987	993	993	993

Table C.5: Parents, different treatments, Denver only

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

	(1)	(2)	(3)	(4)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
Treated	.00588	−428	−.00519	−.00461
	(.0111)	(768)	(.0159)	(.0115)
Dep var summary stats				
Mean	.789	21804	.226	.112
Std. Dev.	.408	23297	.419	.315
N	92789	92789	3273	3273
People	3273	3273	3273	3273
Clusters	1208	1208	1208	1208

Table C.6: Children, effects on main outcomes, Denver only

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Awarded SSDI/SSI**	**Cancer**	**Circulatory Disorder**	**Musculoskeletal Disorder**	**Mental Disorder**	**Other Impairment**
Condition	**Alive**	**Earn>0**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**	**Applied SSDI/SSI**
Treated	.00978	−627	−.0104	.0137	−.0062	−.0178	.00692	.00255
	(.0102)	(729)	(.0402)	(.0172)	(.023)	(.0394)	(.0408)	(.0401)
Dep var summary stats								
Mean	.811	27646	.479	.054	.0796	.363	.433	.574
Std. Dev.	.391	22949	.5	.226	.271	.481	.496	.495
N	90088	73181	741	741	741	741	741	741
People	3261	3220	741	741	741	741	741	741
Clusters	1208	1207	531	531	531	531	531	531

Table C.7: Children, intensive margins, Denver only

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

	(1)	(2)	(3)	(4)
Dep Var	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
5-Year Trtmnt	−.0429***	−1536*	.0377*	.0321**
	(.0142)	(897)	(.0211)	(.0147)
Guar Level	$1.37e-06$	.0154	$-2.44e-06$	$-1.43e-06$
	(2.26e-06)	(.148)	(3.36e-06)	(2.20e-06)
Tax Rate, $0	.0107	−2246	.0567	.0608
	(.0902)	(5712)	(.128)	(.0858)
Tax Decline?	.00724	721	.0142	.0331
	(.0202)	(1230)	(.0295)	(.0206)
Manpower	−.012	−296	.0128	−.00286
	(.0108)	(745)	(.016)	(.0116)
Dep var summary stats				
Mean	.789	21804	.226	.112
Std. Dev.	.408	23297	.419	.315
N	92789	92789	3273	3273
People	3273	3273	3273	3273
Clusters	1208	1208	1208	1208

Table C.8: Children, different treatments, Denver only

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

	(1)	(2)
Sample	**Parents**	**Children**
Treated	−.00734	−.00952
	(.0204)	(.0221)
Dep var summary stats		
Mean	.446	.607
Std. Dev.	.497	.488
N	2937	5416
People	2937	5416
Clusters	1921	1886

Table C.9: Parents, effect on match rate, Denver only

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep Var	**Applied SSDI**	**Applied SSI**	**Awarded SSDI**	**Awarded SSI**	**Positive Annual Self-Empl Earnings**	**Annual Self-Empl Earnings**	**Earn > 10k**	**Earn > 20k**	**Earn > 50k**	**Ln(Earn +1k), by Year**
Treated	.0623**	.0358	.0265	−.00371	−.0019	−58.7	−.0383*	−.0269	−.0172	−.128*
	(.027)	(.0224)	(.0251)	(.0168)	(.00782)	(170)	(.0205)	(.0202)	(.0184)	(.0716)
Dep var summary stats										
Mean	.291	.173	.21	.0951	.0423	517	.576	.464	.339	9.13
Std. Dev.	.454	.378	.407	.293	.201	4682	.494	.499	.474	1.65
N	1283	1283	1283	1283	31286	31286	31286	31286	31286	31286
People	1283	1283	1283	1283	1276	1276	1276	1276	1276	1276
Clusters	993	993	993	993	987	987	987	987	987	987

Table C.10: Parents, other variables, Denver only

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
Usual	−.00734	−.029	−1765*	.056**	.00972	.0275
	(.0204)	(.0184)	(1003)	(.0282)	(.0266)	(.0264)
No Manpower	−.00701	−.0278	−1653*	.0535*	.00738	.0282
Control	(.0203)	(.0185)	(999)	(.0281)	(.0265)	(.0263)
No Age/Sex	−.00233	−.0272	−1623	.0605**	.011	.0237
Control	(.0207)	(.019)	(1033)	(.0286)	(.0266)	(.0303)
No Earnings	−.00744	−.0314*	−2010*	.0582**	.0107	.0301
Control	(.0204)	(.0186)	(1034)	(.0283)	(.0266)	(.0266)
Control for Earn	−.0066	−.0285	−1666	.0463	.00356	.0236
in All Years	(.021)	(.0191)	(1032)	(.029)	(.0274)	(.0274)
Control for Any	−.00701	−.0296	−1746*	.0559**	.00908	.0263
Pre-exp Earn	(.0204)	(.0184)	(1001)	(.0282)	(.0265)	(.0263)
No Post-Exp	−.0171	−.00472	−2194*	.0258	−.0208	−.00363
Births	(.0197)	(.0234)	(1185)	(.0327)	(.0311)	(.0315)
No Post-Exp Births	−.0159	−.00904	−2589**	.0379	−.0163	.0109
Or Parent Recs	(.0185)	(.0253)	(1235)	(.0354)	(.0332)	(.0341)
75% Conf	−.0166	−.0205	−1262	.0508*	.0105	.0232
Sample	(.0206)	(.0177)	(971)	(.0274)	(.0258)	(.0255)
99% Conf	−.0126	−.0268	−1731*	.0598**	.00338	.0352
Sample	(.02)	(.0192)	(1043)	(.0293)	(.0276)	(.0274)
Cox				.222**	.039	.118
Model				(.113)	(.125)	(.103)
Include	−.00357	−.0266	−1940**	.0551**	.0152	.0165
20-Yr Sample	(.0196)	(.0177)	(974)	(.0271)	(.0253)	(.0258)

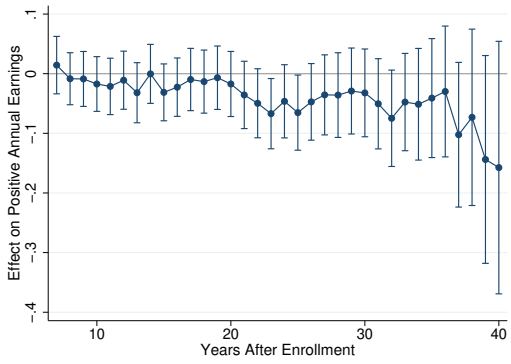
Table C.11: Parents, robustness checks, Denver only

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, with the methodology given by the row. Regressions listed as including "No ___ Control" do not control for the given variable. "Control for Earn in All Years" includes controls for four years of pre-experimental income, where that data is available. "Control for Any Pre-exp Earn" controls for the level of pre-experimental income and a dummy for having any such income. "No Post-Exp Births" does not use children born after the experiment began in matching; "No Post-Exp Births Or Parent Records" additionally does not use adult SSA records from after the experiment began for matching. "75% Conf Sample" and "99% Conf Sample" include individuals matched to SSNs with the given confidence level, rather than the standard 95%. "Cox Model" uses a Cox proportional hazard model rather than OLS for the first time that an event occurred. "Include 20-Yr Sample" does not drop families who were told they would receive financial treatment for 20 years. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.

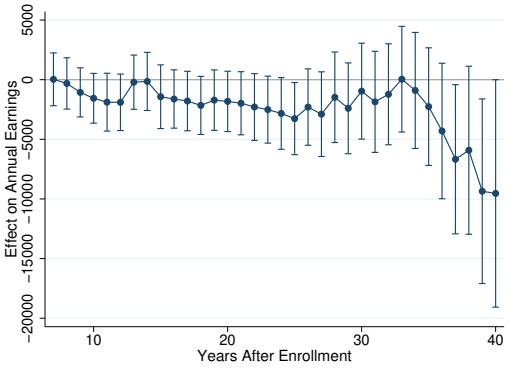
	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**	**Died**
All	−.00734	−.029	−1765*	.056**	.00972	.0275
	(.0204)	(.0184)	(1003)	(.0282)	(.0266)	(.0264)
Fam Inc	−.0892*	.0465	1804	.0348	−.0866	−.0268
< $14k	(.0492)	(.0512)	(2164)	(.0806)	(.0751)	(.0655)
Fam Inc	.0121	−.0362	−3127**	.061	.0434	.0399
$14k - 32k	(.0294)	(.0288)	(1551)	(.0429)	(.04)	(.0404)
Fam Inc	.00296	−.0475*	−2177	.0624	.00947	.0386
$32k +	(.0347)	(.0263)	(1609)	(.0431)	(.0403)	(.0428)
Female	−.0234	−.0208	−1037	.0322	−.00775	.00743
	(.0236)	(.0263)	(1176)	(.0374)	(.0351)	(.0345)
Male	.0211	−.0444*	−3004*	.0849**	.0286	.0558
	(.0318)	(.0252)	(1756)	(.0411)	(.0393)	(.0417)
Black	−.0403	−.0285	−3119	.0452	.042	.026
	(.034)	(.0392)	(2098)	(.0557)	(.0517)	(.0512)
White	−.00228	−.0677***	−3886**	.0717	.00659	.0922**
	(.0397)	(.0257)	(1659)	(.044)	(.0419)	(.0414)
Chicano	.0208	.00699	1484	.0408	−.0166	−.052
	(.0325)	(.0329)	(1555)	(.0496)	(.0469)	(.0455)
Single	−.0574	−.00413	−2852	−.0192	−.0194	−.0189
Parents	(.0365)	(.0423)	(2309)	(.0612)	(.0563)	(.0552)
Married	.0102	−.035*	−1597	.0739**	.0123	.0376
Parents	(.0244)	(.0205)	(1107)	(.0319)	(.0303)	(.0302)
2 Child	−.0125	−.0435	−1507	.0729	.0501	.0427
Family	(.0324)	(.0278)	(1541)	(.0469)	(.0436)	(.0445)
3 Child	.0305	−.0113	−308	.0156	−.0144	.000478
Family	(.0373)	(.0347)	(1888)	(.054)	(.0485)	(.0494)
4+ Child	−.0348	−.00545	−2722	−.00523	−.0792	.0257
Family	(.0372)	(.0364)	(1807)	(.0502)	(.0488)	(.0558)

Table C.12: Parents, effects within subgroups, Denver only

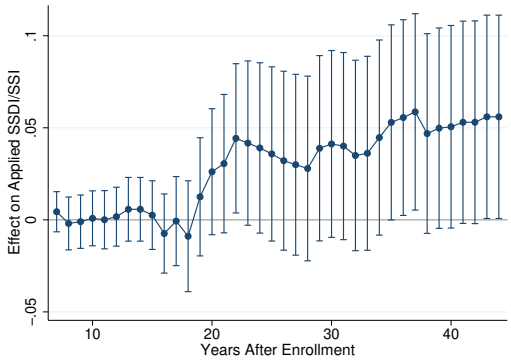
Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, for the subgroup given by the row. "Fam Inc" levels are based on pre-experimental normal income categories. Marital status is based on pre-experimental data. Number of children in the family is based on all children whom it would be possible to match with our methodology. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.



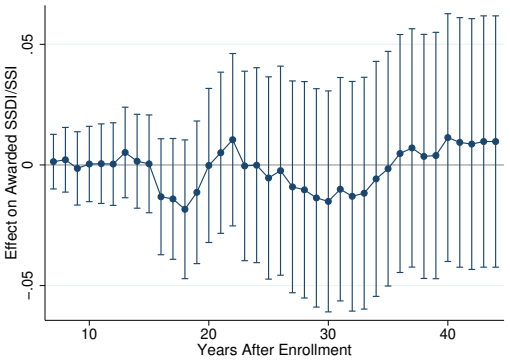
(a) Positive earnings



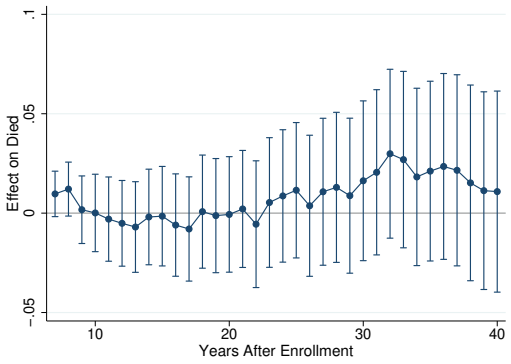
(b) Total earnings



(c) Applied for SSDI/SSI



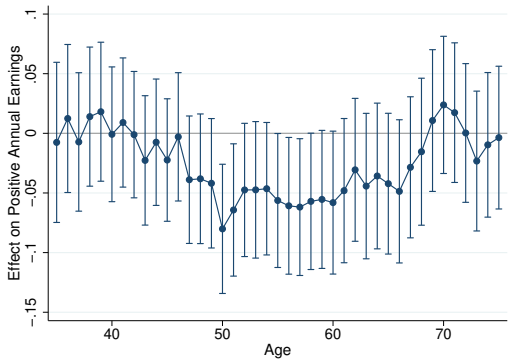
(d) Awarded SSDI/SSI



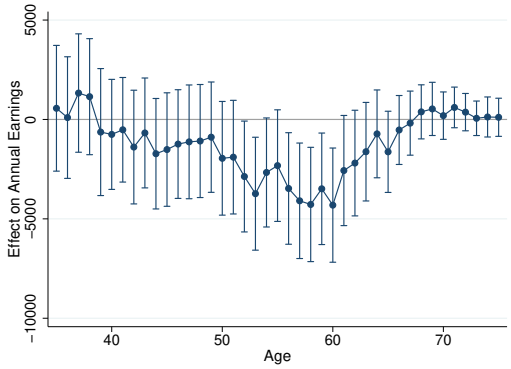
(e) Died

Figure C.3: Parents, effects by years after start of experiment, Denver only

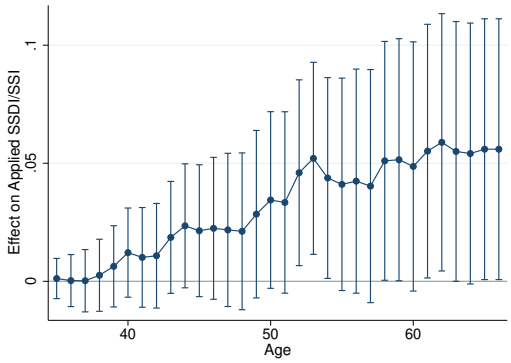
Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from a certain number of years into the experiment. Confidence intervals are based on standard errors that are clustered 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 pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. 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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.



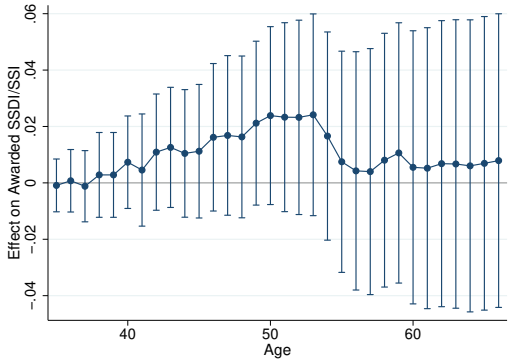
(a) Positive earnings



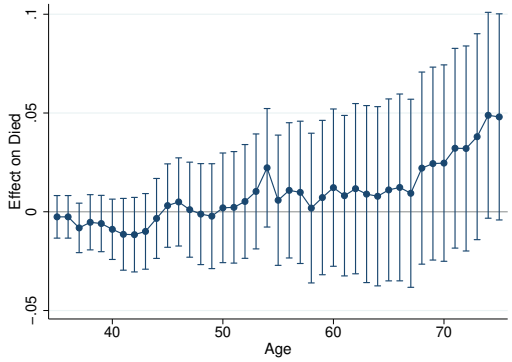
(b) Total earnings



(c) Applied for SSDI/SSI



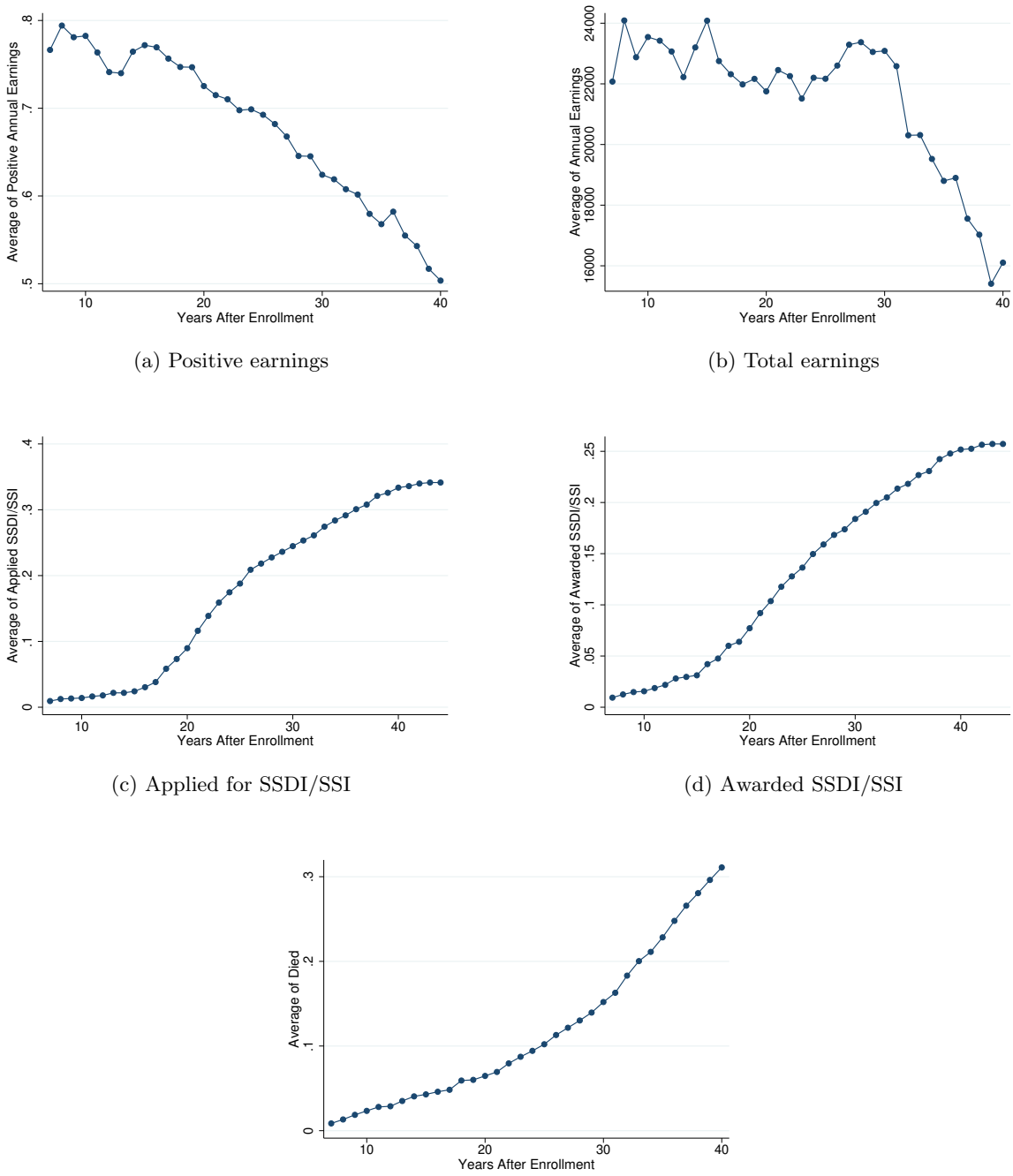
(d) Awarded SSDI/SSI



(e) Died

Figure C.4: Parents, effects by age, Denver only

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



(a) Positive earnings

(b) Total earnings

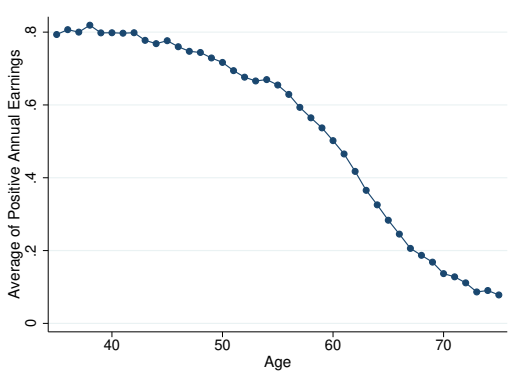
(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

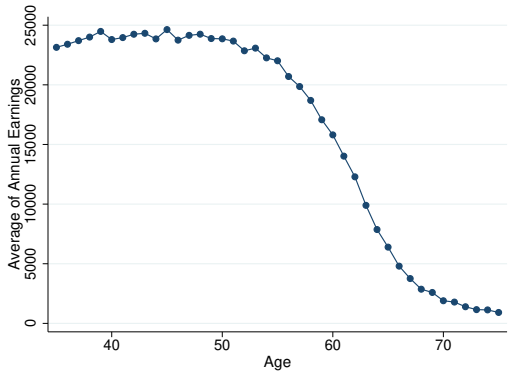
(e) Died

Figure C.5: Parents, average values by years after start of experiment, Denver only

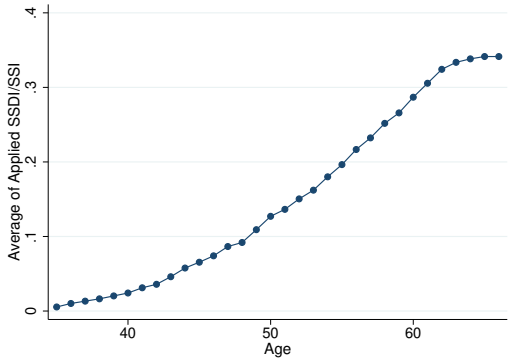
Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from a certain number of years into the experiment.Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. All 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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.



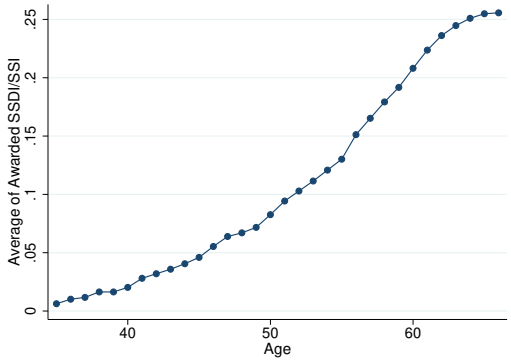
(a) Positive earnings



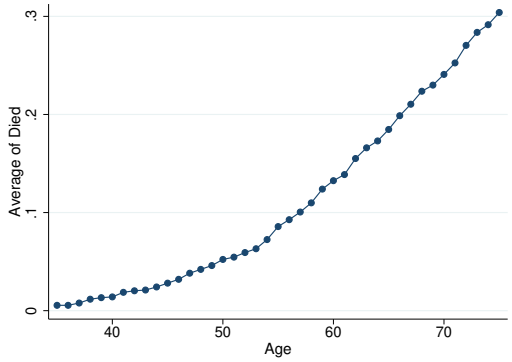
(b) Total earnings



(c) Applied for SSDI/SSI



(d) Awarded SSDI/SSI



(e) Died

Figure C.6: Parents, average values by age, Denver only

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from individuals when they are a certain age.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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dep Var	**Applied SSDI**	**Applied SSI**	**Awarded SSDI**	**Awarded SSI**	**Died**	**Positive Annual Self-Empl Earnings**	**Annual Self-Empl Earnings**	**Earn > 10k**	**Earn > 20k**	**Earn > 50k**	**Ln(Earn +1k), by Year**
Treated	.00133	−.00122	−.000618	−.00838	−.0021	.000737	−15.7	.0108	.00617	−.00192	.0199
	(.0147)	(.0144)	(.0106)	(.008)	(.00975)	(.00408)	(119)	(.0146)	(.0146)	(.0132)	(.0505)
Dep var summary stats											
Mean	.173	.181	.091	.0483	.073	.041	552	.586	.437	.301	9.22
Std. Dev.	.379	.385	.288	.214	.26	.198	6148	.493	.496	.459	1.53
N	3273	3273	3273	3273	3273	92789	92789	92789	92789	92789	92789
People	3273	3273	3273	3273	3273	3273	3273	3273	3273	3273	3273
Clusters	1208	1208	1208	1208	1208	1208	1208	1208	1208	1208	1208

Table C.13: Children, other variables, Denver only

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

	(1)	(2)	(3)	(4)	(5)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
Usual	−.00952	.00588	−428	−.00519	−.00461
	(.0221)	(.0111)	(768)	(.0159)	(.0115)
No Manpower	−.0105	.00527	−442	−.00453	−.00502
Control	(.022)	(.0111)	(768)	(.0159)	(.0115)
No Age/Sex	−.00879	.00694	−360	−.00631	−.00556
Control	(.0224)	(.0112)	(773)	(.0163)	(.0117)
No Earnings	−.00947	.00538	−465	−.00466	−.00465
Control	(.0221)	(.0111)	(771)	(.016)	(.0115)
Control for Earn	−.00919	.00464	−293	−.00579	−.00459
in All Years	(.0227)	(.0112)	(776)	(.0163)	(.0117)
Control for Any	−.00932	.00538	−454	−.00468	−.00357
Pre-exp Earn	(.0221)	(.0111)	(766)	(.0159)	(.0116)
No Post-Exp	−.012	.00343	−441	−.0195	−.00593
Births	(.0208)	(.0132)	(920)	(.0204)	(.015)
No Post-Exp Births	−.0313	.00304	−719	−.0113	−.0128
Or Parent Recs	(.0207)	(.0149)	(1028)	(.022)	(.0164)
75% Conf	−.00322	.00284	−847	.00551	−.00553
Sample	(.0215)	(.0109)	(752)	(.0158)	(.0116)
99% Conf	−.00302	.00541	−517	−.00579	−.00497
Sample	(.0223)	(.0113)	(781)	(.0161)	(.0116)
Cox				−.0703	−.0599
Model				(.0827)	(.115)
Include	−.00619	.00255	−597	−.00862	−.00534
20-Yr Sample	(.0207)	(.0107)	(744)	(.0154)	(.011)

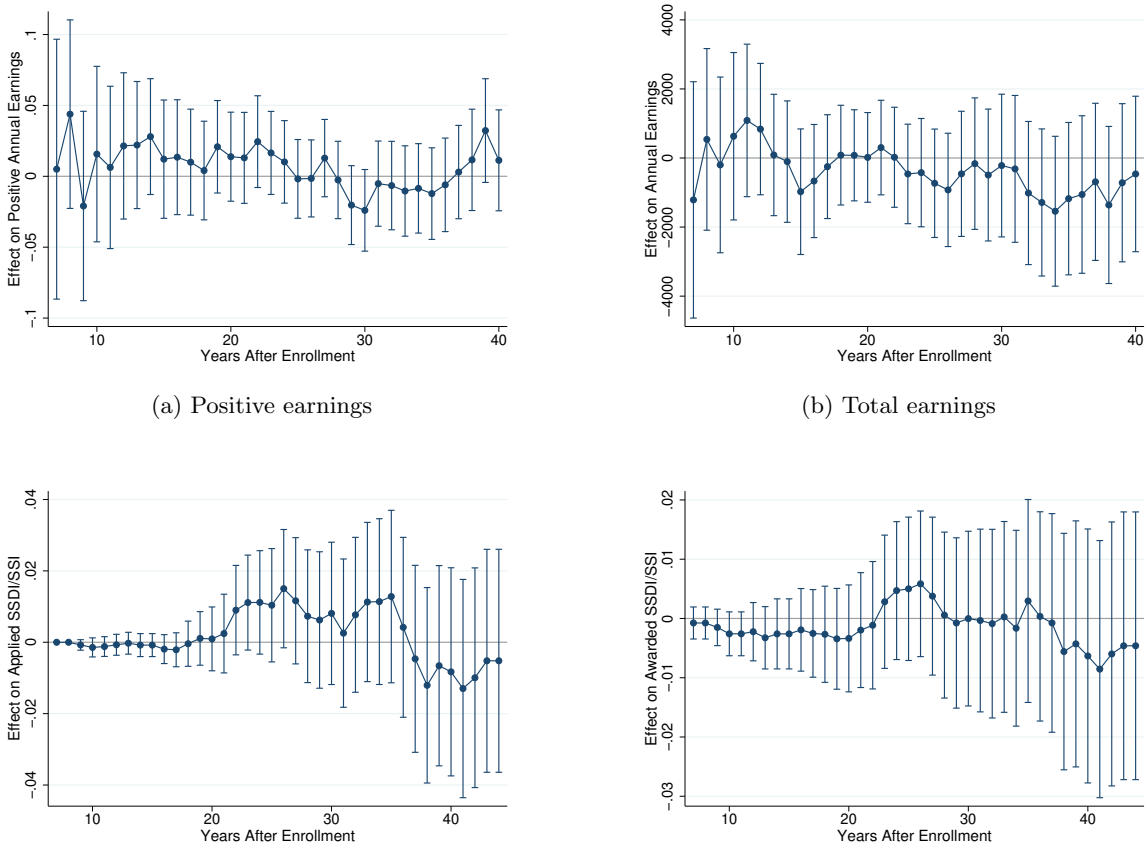
Table C.14: Children, robustness checks, Denver only

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, with the methodology given by the row. Regressions listed as including "No ___ Control" do not control for the given variable. "Control for Earn in All Years" includes controls for four years of pre-experimental income, where that data is available. "Control for Any Pre-exp Earn" controls for the level of pre-experimental income and a dummy for having any such income. "No Post-Exp Births" does not use children born after the experiment began in matching; "No Post-Exp Births Or Parent Records" additionally does not use adult SSA records from after the experiment began for matching. "75% Conf Sample" and "99% Conf Sample" include individuals matched to SSNs with the given confidence level, rather than the standard 95%. "Cox Model" uses a Cox proportional hazard model rather than OLS for the first time that an event occurred. "Include 20-Yr Sample" does not drop families who were told they would receive financial treatment for 20 years. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.

	(1)	(2)	(3)	(4)	(5)
Dep Var	**In Sample**	**Positive Annual Earnings**	**Annual Earnings**	**Applied SSDI/SSI**	**Awarded SSDI/SSI**
All	−.00952	.00588	−428	−.00519	−.00461
	(.0221)	(.0111)	(768)	(.0159)	(.0115)
Fam Inc	−.0468	.0167	69	−.0516	−.0232
< $14k	(.0475)	(.0247)	(1319)	(.0434)	(.0276)
Fam Inc	.00202	.000864	−812	.0256	.0188
$14k - 32k	(.0327)	(.0166)	(1138)	(.0228)	(.0164)
Fam Inc	.00309	.00499	−287	−.0253	−.0255
$32k +	(.0389)	(.0189)	(1464)	(.0254)	(.0202)
Female	−.027	.0193	−306	−.0161	−.0199
	(.0255)	(.0142)	(909)	(.0224)	(.0157)
Male	.00848	−.00698	−360	.0068	.014
	(.0265)	(.0148)	(1066)	(.0228)	(.0171)
Black	−.00157	−.0291	−1974	.0484	.0269
	(.0381)	(.0206)	(1382)	(.0305)	(.0212)
White	−.014	.00182	−2811*	−.0045	.00384
	(.041)	(.0185)	(1481)	(.0257)	(.0189)
Chicano	−.00773	.0345*	2631**	−.0461*	−.0392**
	(.0356)	(.0178)	(1143)	(.0261)	(.019)
Single	−.0424	−.0094	−1370	−.0205	−.0078
Parents	(.0375)	(.0184)	(1252)	(.0309)	(.0197)
Married	.00923	.0116	−112	.0019	−.00187
Parents	(.0273)	(.0137)	(945)	(.0184)	(.0142)
2 Child	−.0488	−.00402	494	.03	.0023
Family	(.0377)	(.0192)	(1359)	(.0291)	(.0214)
3 Child	−.0125	−.028	−2471*	.0106	.000831
Family	(.0371)	(.0184)	(1338)	(.0267)	(.0193)
4+ Child	.0153	.0374**	1155	−.0398	−.0165
Family	(.037)	(.0183)	(1158)	(.0268)	(.0203)
Age	.0225	−.0252	−1713	.0178	.00104
≤ 0	(.0406)	(.0227)	(1719)	(.0334)	(.0246)
Age	.00252	.021	366	−.0353	−.0436**
1 - 5	(.031)	(.0163)	(1118)	(.0248)	(.0188)
Age	−.0145	.0267	103	.0146	.0091
6 - 10	(.0349)	(.0204)	(1335)	(.0333)	(.023)
Age	−.016	−.0172	−959	−.013	.0116
11+	(.0355)	(.021)	(1451)	(.0339)	(.0271)

Table C.15: Children, effects within subgroups, Denver only

Notes: Significance level: *=10%; **=5%; ***=1%. Standard errors, shown in parentheses, are clustered at the level of the original family. Outcomes based on SSA data. Regressions include dummy variables for each assignment group (unique combinations of site, race, number of household heads, and pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. Regressions on earnings variables include year fixed effects. All dollar values are based on 2013 dollars, adjusted for inflation using the PCE. Non-earnings outcome variables are indicators for whether the event ever occurred in our data. Independent variable "treated" indicates whether the individual was in a treated family. Each cell reports the results of one regression with the dependent variable given by the column, for the subgroup given by the row. "Fam Inc" levels are based on pre-experimental normal income categories. Marital status is based on pre-experimental data. Number of children in the family is based on all children whom it would be possible to match with our methodology. Age is counted from the start of the experiment in each site. Only data from Denver families is included. Comparable results for all families is shown in Table **??**.



(a) Positive earnings

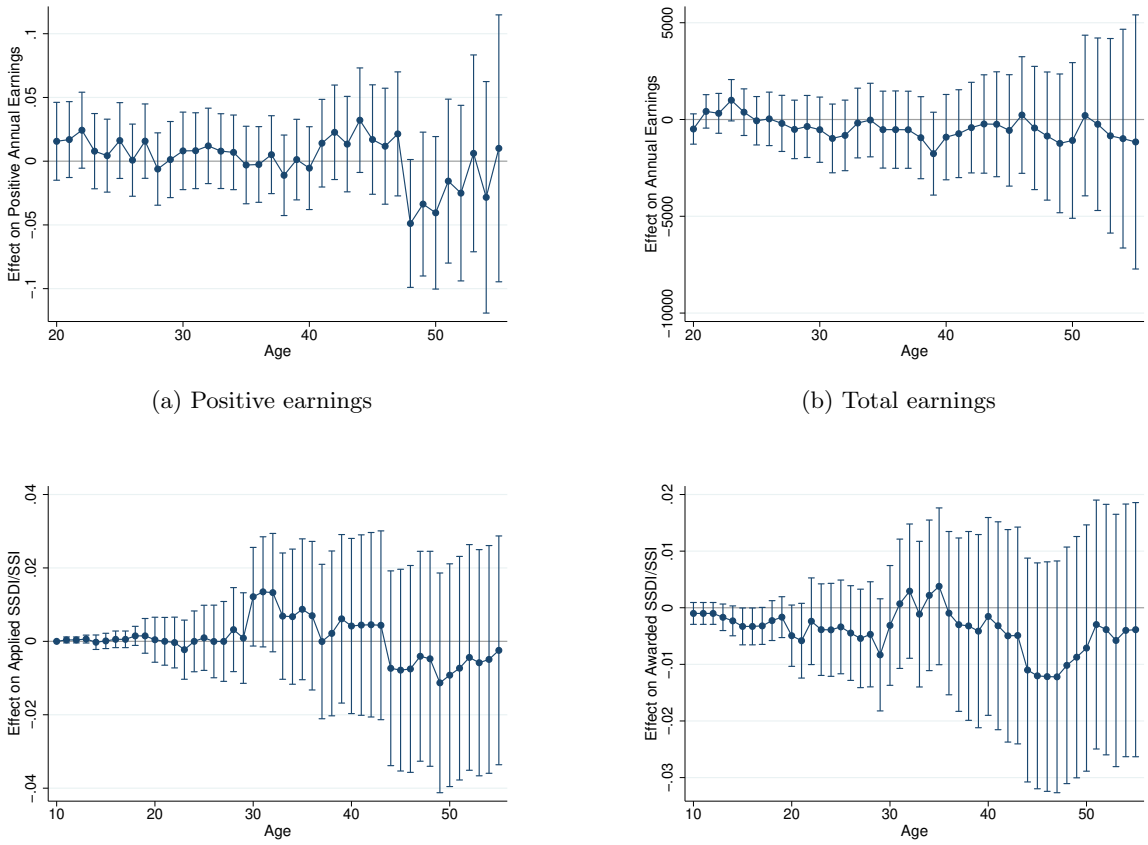
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure C.7: Children, effects by years after start of experiment, Denver only

Notes: Each data point represents the estimate and 95% confidence interval of the coefficient on a dummy for financial treatment status in one regression, limiting the sample to data from 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 pre-experimental income category). Unless otherwise noted, the regressions also include assignment to manpower treatment category, pre-experimental earned income, sex, and a cubic polynomial of date of birth. Earnings variables are based on one observation per year for all years between 1978 and 2013 in which the person was aged between 20 and 60. 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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.



(a) Positive earnings

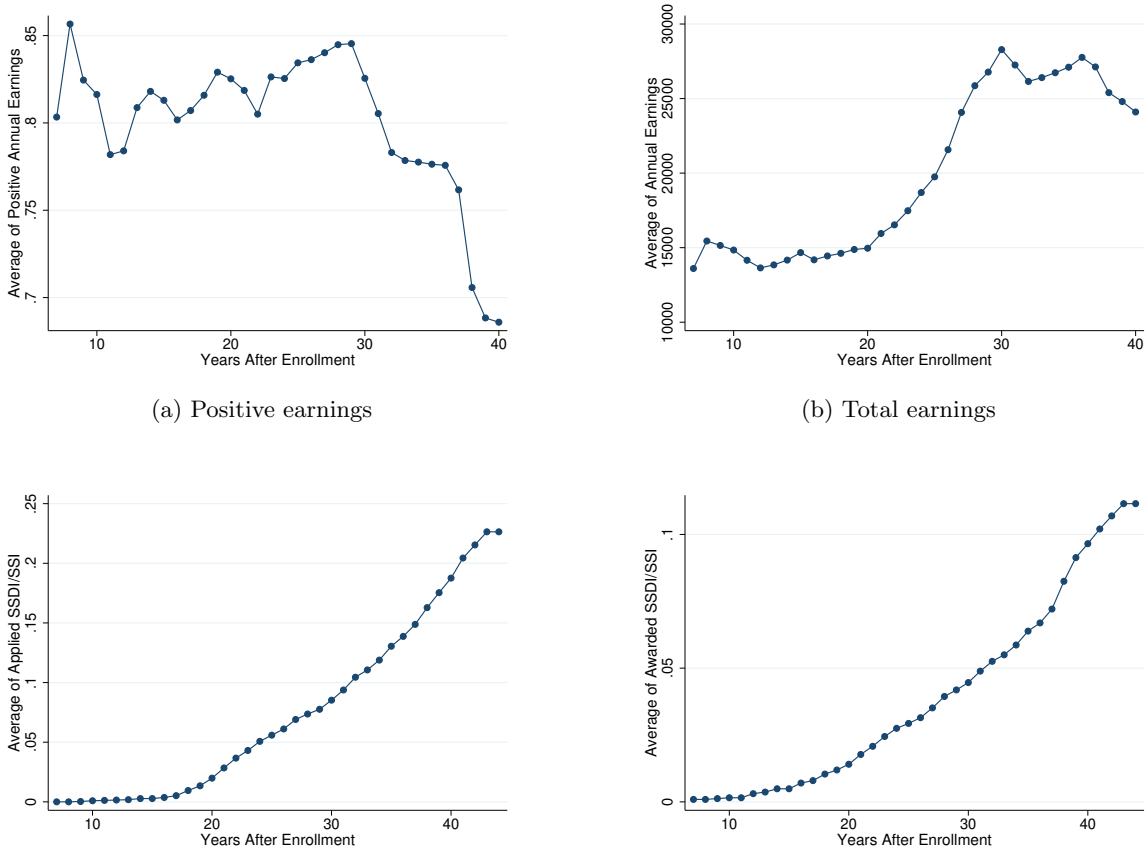
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure C.8: Children, effects by age, Denver only

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



(a) Positive earnings

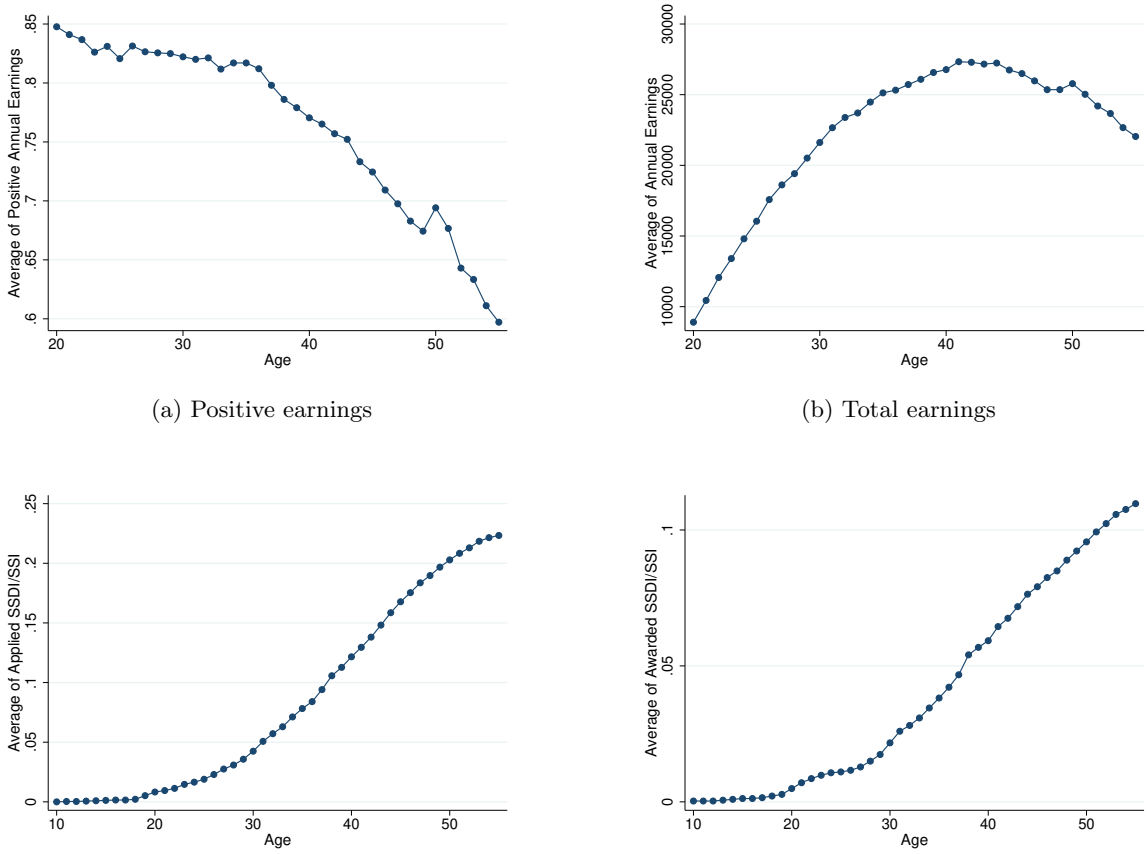
(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure C.9: Children, average values by years after start of experiment, Denver only

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from a certain number of years into the experiment.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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.



(a) Positive earnings

(b) Total earnings

(c) Applied for SSDI/SSI

(d) Awarded SSDI/SSI

Figure C.10: Children, average values by age, Denver only

Notes: Each data point represents the average value of the outcome variable, limiting the sample to data from individuals when they are a certain age.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. Only data from Denver families is included. Comparable results for all families is shown in Figure **??**.