Eliminating Fares to Expand Opportunities: Experimental Evidence on the Impacts of Free Public Transportation on Economic and Social Disparities

Rebecca Brough, Matthew Freedman, and David C. Phillips*

July 2023

Abstract

We conduct a randomized controlled trial to study the direct and downstream effects of providing free public transportation to individuals with low income. While reducing the price of transit to zero doubles transit use, it has no meaningful effects on paid hours worked or earnings. However, rich administrative data on a wide range of other outcomes indicate that free transit improves individuals' well-being, and in particular health. Complementary survey data reveal that participants use free transit to access a variety of services and amenities, implying that the benefits of lower transit costs primarily accrue from sources other than employment.

Keywords: public transportation, transit subsidies, randomized controlled trial

JEL: H4, H7, I3, R4, R5

*Brough: University of California-Davis (e-mail: rjbrough@ucdavis.edu). Freedman: University of California-Irvine (e-mail: matthew.freedman@uci.edu). Phillips: University of Notre Dame (e-mail: david.phillips.184@nd.edu). This research was supported by King County Metro Transit, the University of Notre Dame's Wilson Sheehan Lab for Economic Opportunities (LEO), the Institute for Research on Poverty, and the National Bureau of Economic Research (NBER) and was carried out with the assistance of King County Metro Transit and the Washington Department of Social and Health Services. The University of Wisconsin Survey Center conducted the phone surveys. This RCT was registered as AEARCTR-0005538 (Brough, Freedman and Phillips, 2020). This study was approved by the University of Notre Dame IRB (18-08-4821) and Washington State IRB (2021-020). Special thanks to Carrie Cihak, Taylor Danielson, Lindsey Greto, Truong Hoang, Maria Jimenez-Zepeda, Silvia Khammixay, Mark Konecny, Rich Lee, and Lori Mimms. Katherine Fugate, Matthew Green, and Charles Hanzel provided excellent research assistance. Thanks to audiences at King County Metro, the University of Washington, UC Berkeley IRLE, NBER, WMATA, and the Lab@DC for helpful comments. The views expressed here are those of the authors and do not necessarily represent the views of King County or the State of Washington.

1 Introduction

Living in a neighborhood lacking access to amenities and employment limits lifelong economic opportunity (Chetty and Hendren, 2018). There is extensive research on housing and place-based interventions aimed at addressing disparities that arise due to differences in opportunity across geography (Ludwig et al., 2012; Busso, Gregory and Kline, 2013; Chetty, Hendren and Katz, 2016; Bartik, 2020). However, governments have increasingly experimented with transportation policy, and in particular their public transit systems, in efforts to expand access to jobs, services, and amenities among disadvantaged populations. In the U.S. alone, cities including New York, Los Angeles, Boston, San Francisco, Washington D.C., Dallas, Denver, Portland, Austin, Salt Lake City, and Seattle have recently adopted or are considering implementing means-tested public transit fare programs. Yet despite the enthusiasm around these initiatives, there is limited evidence on the impact of transit fare reductions on the lives and livelihoods of people with low income.

This paper studies the effects of free public transit fares on employment, public assistance receipt, finances, criminal justice contact, health, and residential mobility among individuals with low income. We conducted a randomized controlled trial (RCT) that enrolled 1,797 participants at public assistance offices in King County, Washington, which is the location of Seattle, in 2019 and early 2020. In the experiment, individuals in the treatment group received transit fare cards that provided up to six months of free public transit, passes that would otherwise cost about $200 to purchase. Individuals in the control group received the status quo means-tested transit fare card that provided reduced fares of $1.50 per bus ride. As detailed in a prior paper (Brough, Freedman and Phillips, 2022), access to free public transportation induced large changes in travel behavior, doubling travel by public transit. To measure the effects of fare-free public transit and the resulting changes in travel on downstream outcomes, we link individuals in the experiment to rich administrative data from payroll tax, public assistance, criminal justice, and healthcare records as well as proprietary data on consumer credit and residential locations. We additionally take advantage of detailed surveys of participants that not only shed light on anticipated and actual trip purposes, but also provide an array of indicators of individuals' well-being.

We first explore the effects of providing free public transit on a range of employment outcomes. We do not detect large effects of the treatment on employment. One quarter after random assignment, individuals in the treatment group work for pay 1.6 more hours per quarter than those in the control group on average. This gap is not statistically distinguishable from zero and is relatively small. The 95% confidence interval excludes increases in paid hours worked greater than 4% of full-time employment. Though the COVID-19 pandemic complicates measuring longer-term effects, we can gain additional precision by pooling treatment effects over multiple quarters (extending into the pandemic period). In a typical quarter, paid hours worked increase in the treatment group by no more than 3% of full-time work. Similarly, the treatment is not associated with large changes in employment rates, total earnings, wage rates, job transitions, or employment stability. There may be margins of adjustment with respect to employment that we cannot detect with administrative data, but our results point to limited impacts of the treatment on the paid work lives of individuals with low incomes.

However, we find evidence that access to free public transit improves well-being on other dimensions. Most notably, individuals in the treatment group appear healthier, using less healthcare as measured by Medicaid-covered visits to healthcare providers. Specifically, those in the treatment group are 5.6 percentage points less likely to visit a doctor or hospital within three months of study enrollment, compared to a control group mean of 34.7%. Less expensive non-emergency outpatient visits drive most of the relative decline in healthcare use, so improved health likely has limited impact on the cost to the state of providing healthcare. Additionally, while we do not observe any effects of free transit access on employment, take-up of public benefits, or residential mobility, we find some imprecisely measured but suggestive evidence of improved finances and reduced contact with the criminal justice system among individuals in the treatment group. Although the presence of many outcomes could lead to false positives, the estimated effects on healthcare use and financial circumstances are largely robust to adjustments for multiple hypothesis testing.

Overall, our results suggest that free fares for public transit improve individuals' well-being through channels other than formal employment, most likely because people with low income use transit for a diffuse set of activities. At baseline, larger fractions of study participants anticipate they will use the subsidy for errands and shopping, visiting family and friends, health-related travel, and accessing public benefits than for paid work. Based on a follow-up survey of a sub-sample of participants, individuals in the treatment group report that 58% of transit trips are for non-work purposes. Consistent with our main results based on administrative records and proprietary data, follow-up surveys of study participants also point to positive treatment effects on multiple indicators of well-being. Study participants' diverse intentions and varied uses of transit better explain the lack of effects on employment than other potential explanations. For example, using machine learning methods developed by Athey and Imbens (2016), we cannot detect meaningful heterogeneity in the treatment's impacts on employment-related outcomes across subgroups, which suggests that our employment results are broadly applicable rather than over-representing particular populations (e.g., those detached from the labor force).

Taken together, our findings indicate that a fairly broad group of low-income individuals benefit from free transit primarily for reasons other than employment. The pattern of results hints that income effects might be important; the subsidy could free up money individuals use to participate in recreation, pay down debt, or engage in other activities that improve well-being, while at the same time having negligible effects on labor supply. However, not only is the cash equivalent of the subsidy relatively small, but we also observe impacts of the treatment on transit use that are at least five times larger than we would expect to see if individuals instead received cash. Additionally, we benchmark our estimates against other recent work studying the impacts of unconditional cash transfers to similar populations and conclude that at most a small fraction of the estimated impact of providing free transit is likely attributable to income effects alone. Rather, in-kind benefits in the form of free public transportation appear to impact several dimensions of individuals' lives in ways that cash equivalents would be unlikely to do.

Our study makes several contributions. First, we extend the study of an increasingly popular policy, free fares on public transportation systems, to a wide variety of outcomes. Earlier work exploiting the same experiment found that providing free public transportation significantly increased public transit use; the effect on overall mobility (including modes other than transit) was potentially large but less clear (Brough, Freedman and Phillips, 2022). Studies on the effects of free transit fares in other contexts, including some RCTs, have also pointed to large effects on transit use as well as important implications for overall mobility (Volinski, 2012; Cools, Fabbro and Bellemans, 2016; Cats, Susilo and Reimal, 2017; Bull, Munoz and Silva, 2021; Busch-Geertsema, Lanzendorf and Klinner, 2021). Other work has examined the effects of free or reduced transit fares on particular domains, such as healthcare use (Rosenblum, 2020) or court appearances (Brough et al., 2022). We build on this literature by studying the effects of free public transit on a wide array of downstream outcomes for people with limited means.

Second, our results show that transit benefits people with low income by providing access to a variety of services and amenities, not just formal employment opportunities. Recent and prominent quantitative models of urban location typically focus on people who commute to work but benefit from amenities only at their residence (Ahlfeldt et al., 2015; Monte, Redding and Rossi-Hansberg, 2018; Barwick et al., 2021; Almagro and Domínguez-Iino, 2022). As a result, studies using such models to quantify the overall benefits and distributional implications of transit systems exclusively measure changes that operate through employment and residential location (Severen, 2021; Tsivanidis, 2022). Similarly, a long-running literature considers the role that differential access to jobs across neighborhoods plays in generating disparate labor market outcomes and persistent concentrations of poverty (Kain, 1968; Wilson, 1997). Many quasi-experimental studies have argued that transportation infrastructure can improve employment outcomes for disadvantaged populations (Holzer, Quigley and Raphael, 2003; Tyndall, 2021; Li and Wyczalkowski, 2023), and a few RCTs indicate that subsidizing transportation for unemployed individuals can increase job search intensity and at least temporarily improve labor market outcomes (Phillips, 2014; Franklin, 2018; Abebe et al., 2021). Relative to this literature, we not only study a deeper subsidy covering several months among a much broader group of disadvantaged individuals, but also measure a wider range of outcomes. Contrary to assumptions in standard urban economics models and the focus of prior empirical work, our results suggest that transit benefits people with low income primarily through access to amenities rather than employment. As a result, echoing the implications of recent work using smartphone-based mobility data (Miyauchi, Nakajima and Redding, 2022), our findings imply that existing methods that focus on the commuting channel likely understate the overall benefits of transit, particularly for people with low income. The prevalence of such non-work benefits could affect the optimal design of transit systems, which historically have been focused on facilitating commutes to urban cores (Cervero, 2013).

Our results have broader implications for policies aimed at improving the lives of populations with low incomes. Prior work on housing programs that incentivize relocation from high poverty neighborhoods show little increase in employment but increases in well-being for adults (Ludwig et al., 2012) as well as benefits for children (Chetty, Hendren and Katz, 2016; Chyn, 2018). We similarly find that transit benefits adults primarily through mechanisms other than employment. Alongside programs that involve direct investment in distressed places or that incentivize relocation to better areas, programs that better connect neighborhoods and integrate metropolitan areas via improvements in transportation access can impact individuals' and families' well-being along a number of dimensions.

2 Context

We conducted the experiment in King County, Washington. King County is home to Seattle, and with 2.3 million residents in 2020, it is the most populous county in Washington State. King County is served by an extensive public bus, streetcar, light rail, water taxi, and ferry network, which is overseen by the King County Metro Transit Department (i.e., King County Metro), the Central Puget Sound Regional Transit Authority (i.e., Sound Transit), and other local transit agencies. The maps in Figure 1 show the extent of the transit network at the time of our study. At that time, rail service largely consisted of one line running from the region's primary airport in south King County to the University of Washington north of downtown Seattle. Both rapid transit buses ("rapid ride") and regular local buses cover the remainder of the study area. In 2019, 15% of all workers in King County, and 10% of those with incomes below 150% of the federal poverty line, commuted by public transportation.[1]

With a median household income of $106,326, King County skews higher income than the U.S. as a whole at $68,703.[2] However, there is considerable heterogeneity in income levels and access to opportunity across neighborhoods in King County. The first map in Figure 1 uses data from Opportunity Insights (Chetty et al., 2018) to illustrate the heterogeneity in economic mobility across census tracts in western King County. Among children with parents earning $27,000 (the 25th percentile), average household income at age 35 for those growing up in the lower income neighborhoods south of downtown Seattle is less than half that of those from the more affluent neighborhoods north and east of downtown. As in other cities, there is also substantial mismatch between the residential and employment locations of individuals with low incomes in the Seattle area. The second map in Figure 1 uses data from the 2018 LEHD Origin-Destination Employment Statistics (LODES) to show the difference between the number of low-wage jobs in a census block group and the number of low-wage residents there. A disproportionate share of low-income residents live south of Seattle, but

[1]Authors' calculations based on the 2019 American Community Survey.

[2]Authors' calculations based on the 2017-2021 American Community Survey.

many jobs held by these residents are in downtown Seattle.

In this context, fare-free transit could increase mobility, leading to greater access to employment, amenities, and, ultimately, greater well-being. Fully subsidized transit should generate more transit use, due to the typical downward sloping demand curve and potentially also because of strong effects of zero prices like those observed in the take-up of health products (Dupas, 2014). Transit price reductions also have income effects, though as described further below, we expect these effects to be small in our setting. To the extent that increased transit use generates new or different travel, rather than just displacing travel by other modes, it could affect labor market outcomes through several channels. In standard urban models (Fujita, 1991; Zenou, 2009; Ahlfeldt et al., 2015) and classic empirical studies (Zax and Kain, 1991), workers reject jobs when commute costs are too high. Travel costs can also limit the scope of individuals' job searches (Phillips, 2014; Franklin, 2018; Abebe et al., 2021). Additionally, commute costs can influence employers' choices of whom to hire (Phillips, 2020*a*; Diaz and Salas, 2020; Carlsson and Eriksson, 2023).

While the literature focuses primarily on these employment effects, free transit could impact other areas of individuals' lives. Prior research points to a strong positive association between public transit use and physical activity, which in turn could affect health (Webb, Netuveli and Millett, 2012; Freeland et al., 2013; Saelens et al., 2014; Kärmeniemi et al., 2018). The better access to family and community groups, parks, exercise areas, and full-service grocery stores that free transit could provide might also impact physical and mental health (Renalds, Smith and Hale, 2010; McCormack and Shiell, 2011). Free transit could directly facilitate medical visits, but also could reduce demand for healthcare through its effects on other behaviors; for example, individuals induced to exercise may use less outpatient care (Buchner et al., 1997). In view of these potential mechanisms, several studies on new transit infrastructure have considered its effects on health outcomes such as obesity, body mass indices, and healthcare costs (Brown and Werner, 2008; Stokes, MacDonald and Ridgeway, 2008; MacDonald et al., 2010). Other studies similarly focused on the implications of transit network expansion for crime (Billings, Leland and Swindell, 2011; Phillips and Sandler, 2015; Ridgeway and MacDonald, 2017) and residential location choices (Mulalic and Rouwendal, 2020; Chernoff and Craig, 2022).

3 Free Transit Experiment

Our experiment in providing free public transit involved two separate waves of participants, which we refer to as cohorts. The two cohorts had similar designs, reached much the same population, and delivered similar treatments. They differed in their timing and scope as well as in follow-up surveying approaches.

3.1 Recruitment and random assignment

For both cohorts, we recruited a subset of individuals visiting Department of Social and Health Services (DSHS) Community Service Offices (CSOs) in King County, Washington. Individuals visit CSOs either to enroll in or to renew public assistance benefits. The first map in Figure 1 displays the locations of these offices, with the size of the circle indicating the proportion of the sample recruited at that office. The first study cohort recruited 526 clients from three offices between March 13 and July 1, 2019. These three CSOs included one office in downtown Seattle (Capitol Hill), one larger office just outside the downtown area (White Center), and one office in an area further from downtown Seattle with more limited transit availability (Auburn). The second cohort recruited 1,271 clients from all ten CSOs in the area from December 13, 2019 to March 13, 2020, when we discontinued enrollment due to COVID-19 and associated disruptions. In King County, as in much of the rest of the U.S., COVID-19 prompted widespread business and school closures.

During the experiment, customer service agents asked individuals at the end of their enrollment process for other assistance programs if they were interested in transit benefits. If they responded positively, they were offered an opportunity to participate in a study in which there was a chance they would receive free public transit fares for a period of time. Those who expressed interest in the study went through a consent process, took a brief intake survey, and then were randomized into treatment and control groups.[3] The probability of treatment was one-third from the beginning of the study until February 17, 2020, or midway through the second cohort, when it was increased to one-half.

3.2 Control and treatment

The control group received the status quo, which was a partial fare subsidy. King County Metro operates the ORCA LIFT program, which provides fare discounts to people with income below 200% of the federal poverty line. At the time of the study, this pass reduced the price of a bus ride to $1.50 from $2.75. Since all recipients of major public assistance programs qualify for ORCA LIFT, DSHS customer service offices were already enrolling interested clients in this partial subsidy program. For the study, anyone assigned to the control group was offered the opportunity to register and immediately receive an ORCA LIFT card with $10 loaded on it.[4]

Individuals in the treatment group received a fully subsidized transit pass that lasted for up to six months. Specifically, those in the treatment group received a transit card pre-loaded with monthly "passport" passes, which in effect gave the user free rides until the passports expired. At expiration, the card reverted to an ORCA LIFT card identical to those provided to the control group.

The exact length of the full subsidy varied across people and study cohorts. In the first study cohort, the full subsidy expired on either July 31 or August 31, 2019, depending on when the passports were loaded onto the cards. As a result, individuals in the treatment group in the first cohort received as few as 4 weeks to as many as 24 weeks of free transit,

[3]Based on records of total LIFT and EBT cards issued at DSHS offices from September 2018 to August 2019, 15% of people who receive an EBT card also receive a LIFT card. Among those receiving a LIFT card during our study period, two-thirds (67%) enrolled in the study.

[4]For a brief period at the beginning of December 2019, those in the control group received a card pre-loaded with $15 instead of the status quo $10.

depending on when they visited the DSHS office and were issued their card. On average, the treatment group in the first cohort received 16.7 weeks of free transit. In the second cohort, treatment card passports were set to expire on June 30, 2020. The onset of the pandemic, though, prompted substantial changes to public transit services, including a suspension of fare collection for all riders, which rendered the treatment moot as of March 21, 2020.[5] As a result, participants in the second cohort received between 0 and 14 weeks (mean 6.1 weeks) of full subsidies prior to the onset of COVID-19. Transit fares were reinstated system-wide on October 1, 2020. We were able to extend the treatment group's free transit period through December 31, 2020; we sent notices to study participants in May as well as in October 2020 alerting them of this change. Including this three-month extension, individuals in the treatment group in the second cohort received between 14 and 27 weeks of free transit.

4 Data and descriptive statistics

4.1 Baseline characteristics and transit use

During enrollment in the study, participants took an intake survey that collected information on demographics and baseline travel habits. We use identifiers recorded in the survey to link study participants with King County Metro's LIFT registry, which contains additional demographic characteristics. Combining these two data sets, we have information on study participant age, race, household size, census block group of residence, language, transit use in the 30 days prior to enrollment, and usual method of payment for transit. For participants in the second cohort, we also asked about mode of transportation to the enrollment site, whether cost represents a barrier to using public transit, and their anticipated uses of transit were it free. Using the LIFT registry, we can also track individuals' transit card use, measured as "taps" on any vehicle operated by King County Metro or a partner agency.[6]

[5] Brough, Freedman and Phillips (2021) document the impacts of COVID-19 and related policy responses on travel behavior in the King County area.

[6] We also have information on participants' use of any replacement or supplemental cards.

4.2 Washington State administrative records

We use multiple state administrative datasets to capture our pre-specified primary outcomes related to employment as well as several of our pre-specified secondary outcomes related to public benefit receipt, arrests, and healthcare utilization. First, we link the data to Washington State unemployment insurance (UI) records. These records allow us to track whether an individual was working in UI-covered jobs each quarter, and if they were working, how much they earned and their hours of paid work.[7] These data also allow us to construct measures of job stability, including job starts and separations as well as employment continuity.

Second, we link individuals to DSHS records that report monthly participation in Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), Washington's Aged, Blind or Disabled Cash Assistance Program (ABD), and Washington's Housing and Essential Needs Program (HEN). SNAP provides individuals and families with low incomes monthly benefits that can be used to buy food. TANF offers temporary cash assistance to children and families in need. ABD provides cash assistance to those aged 65 and over, who are blind, or who have a long-term disability and who meet certain income and resource requirements. HEN provides access to essential needs items and rental assistance to individuals with low income and who are at least temporarily unable to work due to a physical or mental incapacity.

Third, we measure criminal justice system contact using Washington State Patrol (WSP) records. WSP compiles data from local jurisdictions to conduct background checks. We can track felony, gross misdemeanor, and misdemeanor arrests, and can further break out arrests by type including assault, theft, sex crime, domestic violence, custody-related crime, alcohol/drug crime, trespass, reckless driving, vehicle license, weapons, probation, murder,

[7] Washington's Employment Security Department (ESD) collects these records for all workers who earn wages in the state and are covered by UI. These data do not include jobs not covered by UI, such as contract work or informal jobs. Washington records more employment details in its UI system than do other states (Lachowska, Mas and Woodbury, 2020; Jardim et al., 2022), so we can measure treatment effects on paid hours worked in addition to employment and earnings. Employers report actual hours worked for those employees who are paid by the hour. For salaried workers, hours are calculated as 40 times the number of weeks worked.

and failure to comply. We observe monthly indicators for each type of arrest.

Fourth, we track individuals' healthcare utilization under Medicaid. Medicaid provides health insurance to individuals and families with low to moderate incomes. The State of Washington maintains its own Medicaid billing records, and approximately 63% of the matched study sample is eligible for Medicaid at baseline. Therefore, relying on Medicaid records is reasonably complete. We can observe any Medicaid-covered healthcare visit by month of healthcare use. We can further break out healthcare visits into emergency in- and outpatient visits as well as non-emergency in- and outpatient visits. Following Finkelstein et al. (2012), we assign expected costs to Medicaid of visits based on the average cost of different inpatient/outpatient and emergency/non-emergency combinations.[8]

Washington DSHS's Research and Data Analysis group matched study participants who completed random assignment to state administrative records based on name and date of birth as recorded in Metro's LIFT registry. Our main sample consists of individuals who completed random assignment and matched to any of these state administrative datasets prior to enrollment. We limit the sample in this way because the internal organization of these records is such that matching to one dataset provides identifiers that facilitate exact matching to others, while failing to match to at least one dataset is not a guarantee that the individual does not appear in those datasets (given the match with our study records is probabilistic). Because we can match on a wide array of information, and because individuals in our study are by definition DSHS clients, we have a high match rate; 89% (1,598/1,797) of people who completed random assignment appear in our analysis sample. Match rates are similar across treatment (90%) and control (88%).

4.3 Proprietary data

In addition to linking individuals in the study to state administrative records, we link individuals to proprietary records to measure pre-specified secondary outcomes related to financial

[8]The average costs for non-ER inpatient care, ER inpatient care, ER outpatient care, and non-ER outpatient care are $7,523, $7,958, $435, and $150, respectively.

health and residential mobility.

We measure financial health using quarterly cross-sections of credit records from Experian. The Experian data allow us to observe individuals' debt balances, credit scores, and credit inquiries. Experian conducts a match to the universe of credit reports using data on name, date of birth, and address; however, Experian requires an address to complete a match. Since our sample includes some individuals experiencing homelessness or with an unstable address, these data have a lower match rate of 44% (796/1,797). The low match rate limits statistical power compared to outcomes derived from state administrative data.

We follow Phillips (2020*b*) in constructing measures of residential mobility based on data compiled by Infutor Data Solutions. These data are derived from consumer reference records (e.g., cell phone bills) and cover the entire U.S. They provide exact addresses by month, which we use to measure whether households move after random assignment and, if so, where. We match study records to Infutor records based on name and date of birth within the set of people who ever show a King County address in Infutor's data. However, since some people do not generate a sufficient number of consumer records to appear in the Infutor data, these data also have a lower match rate of 40% (722/1,797). Again, this limits statistical power compared to outcomes derived from state administrative data.

4.4 Follow-up surveys

To complement our state administrative records and proprietary data, we gathered information on travel behavior as well as subjective well-being using surveys of study participants conducted in months after study enrollment. We ran these surveys via a text message "chatbot" in the first cohort and via a traditional phone and web survey in the second cohort. Respondents completed questions about travel the prior day, including information on trip quantity, modes, purposes, and payment methods. We draw on questions asked of both cohorts about transit use and trip purposes as well as questions asked only of the second cohort about subjective well-being. The latter questions ask, "In the past two months, how much has your X situation changed?," where X is alternately transportation, employment, financial, health, housing, and education. We place responses to these questions on a 1 to 5 Likert scale, where 1 is "much worse" and 5 is "much better."

4.5 Descriptive figures

Figure 2 shows, for each cohort, average outcomes over calendar time for three selected measures: mean paid hours worked, credit scores, and number of medical visits. The figures highlight three important features of our study sample. First, our sample has limited labor force attachment and is relatively disadvantaged. In both cohorts, the average study participant has worked for pay just over 100 hours per quarter, compared to full-time work of 520 hours per quarter. The average participant also has a credit score near 520, well below the prime credit score cutoff (600 for the Experian Vantage Score). Second, many participants enroll in the study soon after experiencing a major shock. For example, in each panel of Figure 2, the enrollment period for the first cohort is shaded in dark gray. Panel (a) shows that mean paid hours worked per quarter for the first cohort decline from over 100 to under 80 between the quarter before and the quarter of study entry. Similarly, in panel (c), medical visits increase just prior to study enrollment. These declines in paid hours worked and increases in healthcare utilization are not surprising for a group of people soon to visit DSHS and enroll in public benefits. Third, the COVID-19 pandemic affected study participants significantly. At the onset of the COVID-19 (vertical red line), both hours worked and medical visits decline considerably. Trends in these outcomes inform our empirical strategy.

5 Empirical strategy

5.1 Cross-sectional treatment effects and event studies

We start with a simple specification that allows us to measure treatment effects flexibly. Since we study an RCT with complete take-up, we measure treatment effects at different time horizons using regression-adjusted differences in mean outcomes:

$$Y_{i\tau} = \alpha_\tau + \beta_\tau T_i + \mathbf{X}_i \delta_\tau + \epsilon_{i\tau} \tag{1}$$

In this regression, which we estimate on cross-sections of individuals, i indexes individuals and τ indexes time relative to study enrollment; depending on the outcome, τ refers to weeks, months, or quarters relative to study enrollment. $Y_{i\tau}$ is an outcome (for example, paid hours of work) for person i in time period τ after random assignment. The binary variable T_i indicates random assignment to treatment, and the estimate of β_τ measures the difference in average outcomes between treatment and control at time τ. We include covariates $\mathbf{X}_i$ that adjust this raw mean difference for two reasons. First, $\mathbf{X}_i$ includes an indicator for randomization strata related to the one-time change in the probability of treatment in the middle of the study. Second, in some specifications, $\mathbf{X}_i$ includes variables that reduce residual variance by predicting $Y_{i\tau}$.[9] Since random assignment was at the individual level, we compute heteroskedasticity robust standard errors. Given the number of outcomes we consider, we also present sharpened false discovery rate (FDR) q-values to adjust for multiple hypothesis testing (Benjamini, Krieger and Yekutieli, 2006; Anderson, 2008).

Given the typical duration of the treatment and observed impacts on travel behavior, we focus on downstream outcomes measured approximately three months after study enrollment.[10] However, we also show event study-type figures in which we present estimates of β_τ estimated for a range of time periods, including both pre- and post-enrollment when possible. For most outcomes, we observe data up to 24 months (8 quarters) before and 24 months (8 quarters) after study enrollment.

[9] These variables include indicators for female, Black, Hispanic, and the month of study enrollment. We also include the outcome from the period prior to random assignment, when available. When measuring outcomes in state administrative records, we do not include some variables listed in our pre-analysis plan (age, days of transit use, mode of travel to the CSO, and office indicators) because we were not permitted by the state to link the de-identified state administrative data back to our full study baseline survey.

[10] Employment and credit outcomes are measured in the first full calendar quarter after study enrollment. Other outcomes are measured in the third month following the month of study enrollment.

5.2 Pooled treatment effects

Leveraging data over multiple time periods may provide a more accurate depiction of the impacts of free fares on outcomes and could also help with precision. However, pooling treatment effects over time proves complicated for two reasons. First, the COVID-19 pandemic impacts different participants at different times relative to study enrollment. As noted above, COVID-19 both directly affected outcomes and temporarily made fare-free transit available to everyone. Since the treatment subsidy ended before 2020 for the first cohort, this shock matters more for the second cohort. However, when pooling across cohorts, the same relative quarter may reflect outcomes for individuals differentially impacted by COVID-19. Second, and more mechanically, participants enter the study continuously but we observe downstream outcomes aggregated by calendar quarter or month.[11]

To address these issues, we estimate treatment effects pooled over time using a panel data model that accounts for both time aggregation and whether a treatment-control contrast existed at a particular moment in time. In particular, we estimate:

$$Y_{i\tau} = \gamma \bar{T}_{i\tau} + \nu_i + \mu_\tau + \xi_t + u_{i\tau} \tag{2}$$

We estimate this model on a panel of individuals, again indexed by i, in relative time τ. We include fixed effects for person, relative time, and calendar time (t). A new treatment variable, $\bar{T}_{i\tau}$, measures the fraction of relative time period τ for which person i received an active treatment from the study. This variable equals 1 for a treated individual in a period during which the treatment was active the entire time, zero for treated (and control) individuals in a period during which the treatment was not active the entire time (including while fares were not collected during the pandemic), and a value between 0 and 1 for a treated individual in a period during which the treatment was active only part of the time. For example, for an individual in cohort 2 enrolled on January 31, 2020, $\bar{T}_{i,\tau=0} = 2/3$ when

[11] For example, the state measures hours worked, employment, and earnings at the quarterly level. For each person, relative quarter zero will in general include a mix of pre- and post-enrollment outcomes.

outcomes are measured quarterly. The manner in which we define $\bar{T}_{i\tau}$ allows for a simple interpretation of its coefficient, γ, which will reflect the average causal effect of having fully subsidized transit for an entire time period. Since we estimate a panel with multiple observations per person, we cluster standard errors by individual with this approach.

5.3 Heterogeneity analyses

We examine heterogeneity in the treatment effects in two ways. First, informed by specific contextual and institutional features of our setting, we explore heterogeneity along several individual economic and demographic dimensions, including prior employment history, prior earnings, gender, race, vehicle ownership, and Medicaid eligibility. We additionally follow the causal forest methodology developed by Athey and Imbens (2016) to estimate potential heterogeneous treatment effects. Their data-driven approach involves repeatedly dividing the sample, using one sub-sample to construct partitions and a separate sub-sample to estimate group-specific treatment effects. This approach is well suited to contexts like ours in which the functional forms of the relationships between treatment effects and individual characteristics are not known, and where many characteristics of individuals are observed; in our case, these characteristics include not just baseline demographics, but also pre-enrollment values of outcome variables. Athey and Imbens' (2016) approach has the advantage of identifying important dimensions of heterogeneity in effects, while also providing unbiased subgroup-specific point estimates and confidence intervals. We further discuss this approach and the results from our heterogeneity analyses in Section 6.4.

5.4 Baseline balance

Random assignment successfully balanced baseline characteristics across control and treatment groups. Table 1 shows baseline descriptive statistics for our main analysis sample.[12]

[12] Appendix Table A1 shows baseline descriptive statistics for all study participants (including those not matched to state administrative records). For the full sample, we can show balance on additional characteristics that, for confidentiality reasons, we were not permitted to match to state records. For example,

Columns (1) and (3) show means for the control and treatment groups, respectively, with sample sizes in columns (2) and (4). Column (5) shows a difference in means between the two groups, adjusting only for the change in randomization regime. The variables in different panels of the table come from different data sources, and sample sizes vary by data source. The first panel shows demographic characteristics from the intake survey and Metro's ORCA LIFT registry. The second panel shows lagged outcomes (measured in $\tau = -1$) from state administrative records, credit reports, and consumer reference address histories.

Consistent with randomization, individuals assigned to treatment and control are very similar. For example, 42.3% of individuals in the control group identify as White, compared to 40.7% of those in the treatment group. The regression-adjusted difference of 1.6 percentage points is identical to the raw difference between the two groups and not statistically significant at the 5% level. About 40% of both the control and treatment groups are women,[13] and the typical study participant has approximately 12 years of education. Less than 20% of participants own their own vehicle. Of particular note, outcomes measured prior to study enrollment show balance across all linked datasets. This suggests that treatment-control comparisons remain useful measures of causal effects even in the credit report and address history data for which match rates are lower.

6 Results

6.1 Travel behavior

In response to a full transit subsidy, individuals in the study ride transit much more frequently. Using data on card "taps" on King County area transit agencies' fleet of vehicles, we measure how often study participants used their cards to board public transportation.

we observe self-reported baseline transit use in the full sample; at the time of study enrollment, 88% of individuals assigned to both the treatment and control groups report using transit in the prior 30 days.

[13] 54% of working-age food assistance recipients in Washington are women (Pavelle et al., 2019). The slightly greater share of males in our sample likely results from differential interest in transit use by gender.

Based on the event study approach described in Section 5, Figure 3 shows treatment effects on total transit boardings per week, as measured by card use. Individuals in the treatment group board transit using a card 6-7 additional times per week on average in the first three months after study enrollment, or about four times as often as individuals in the control group. We arrive at a similar percentage increase when we use a measure of "trips" based on consolidating boardings that happen within one hour of each other. Some of this increase could result from the treatment group shifting from untraceable payment methods, like cash or non-payment, or from travel by people other than the intended recipient. Brough, Freedman and Phillips (2022) use the sub-sample survey of actual recipients to quantify changes in payment method and conclude that overall transit use at least doubles in response to treatment, even after accounting for changes in payment methods.

The results on transit use suggest that the treatment represents a meaningful subsidy. First, the implied elasticity of transit demand is large, indicating that transit trips at least double in response to reducing the fare from $1.50 to $0. Second, the cash value of the treatment is nontrivial. If the card induces additional travel of one boarding per day for 16 weeks, that would cost an individual in the control group $168 in fares. The price of the actual monthly passes provided to the average treated individual is similar, at $200.

While we see large and statistically meaningful effects of the treatment on transit card use up to about five months after study enrollment, the largest treatment effects occur in the first three months. This motivates our initial focus on downstream outcomes measured at approximately three months after individuals joined the study in our cross-sectional regressions. However, for our primary and selected secondary outcomes, we also show the full time path of treatment effects in event study figures as well as present results from panel regressions that pool treatment effects over longer time horizons.

6.2 Labor market outcomes

We observe relatively small changes in our pre-specified primary employment outcomes in response to transit subsidies. Table 2 shows mean employment-related outcomes one quarter after study enrollment for the control and treatment groups in columns (1) and (2), respectively. Column (3) displays the "simple" regression-adjusted differences between the two group means, which are based on estimating equation (1) controlling only for the change in treatment probability over time. The estimates in column (4) are based on regressions that additionally include pre-specified baseline control variables. For each coefficient estimate, we present the standard error in parentheses and the associated p-value in brackets. We also present a sharpened FDR q-value adjusted for multiple hypothesis testing in braces, where the adjustments are based on all nine outcomes reported in the table.

The first row of Table 2 shows results for paid hours worked in the first full quarter after study enrollment ($\tau = +1$); the sample in this case includes those with zero recorded work hours, and therefore the measured effect captures both extensive and intensive margin adjustments. On average, the treatment group works in UI-covered jobs for 81.5 hours in the quarter after random assignment, compared to 76.8 hours in the control group. The gap of 4.7 hours between the two groups widens to 5.6 hours when controlling for the randomization regime but narrows to 1.6 hours when controlling for other baseline characteristics. Whether we rely on conventional p-values or sharpened FDR q-values, we cannot rule out that the change in paid hours worked in the quarter after study enrollment is zero. Based on the heteroskedasticity-robust standard error reported in the table, the 95% confidence interval for the estimate for paid hours worked in column (4) spans -15.0 to 18.2 hours. This range includes values that are large relative to the control group mean, but that are small relative to full-time work hours. For example, the upper bound of the 95% confidence interval for paid hours worked per quarter corresponds to 24% of the control group mean, but only 4% of full-time work hours.

As shown in panel (a) of Figure 4, regressions with full controls estimated in each quarter relative to the time of study enrollment show no statistically significant differences in paid hours worked between treatment and control groups for at least eight quarters after random assignment. As shown in Table 3, the panel data model (equation (2)) that pools post-enrollment quarters (taking into account that the treatment contrast between the two groups disappears during the initial months of the COVID-19 pandemic) produces an average effect on paid hours worked of -0.5 per quarter, with a 95% confidence interval spanning -15.0 to 14.2. Based on these estimates, paid hours worked per quarter increase by no more than 18% of the control group mean and 3% of full-time employment.

We also observe only small, statistically insignificant changes in other employment-related outcomes that we can measure using administrative data. Based on our cross-sectional model with controls (column (4) of Table 2), average earnings increase by only \$8 per quarter (0.5%), with a 95% confidence interval ranging from -\$312 to \$327. The control group means and treatment effects for paid work hours and earnings imply that hourly wage rates for the treatment group in the quarter after enrollment fall slightly from \$19.00 to \$18.70. Meanwhile, the probability of any UI-covered employment in the quarter after study enrollment is slightly lower in the treatment group than in the control group, at 29.5% vs. 32.2%. Job transitions also do not change substantially. The point estimates indicate a statistically insignificant 2.9 percentage point decline in job starts (measured as having no hours worked in $\tau = -1$ and positive hours worked in $\tau = +1$) and a 0.9 percentage point increase in job exits (measured as having positive hours worked in $\tau = -1$ and no hours worked in $\tau = +1$). We also detect no change in continuous employment between pre- and post-enrollment periods (measured as having positive hours worked in both $\tau = -1$ and $\tau = +1$), a measure of job stability; this is true regardless of whether we measure it for any employment or employment in narrowly defined industries. The likelihood of being continuously unemployed between quarters before and after study enrollment (i.e., no hours worked in either $\tau = -1$ or $\tau = +1$) is also similar between control and treatment groups.

Overall, we observe very limited impacts of free public transit on the paid work lives of individuals with low incomes. Although we measure a range of employment-related outcomes and few even approach statistical significance (especially after adjusting for multiple hypothesis testing), it is possible that the treatment affects aspects of individuals' work lives that are not captured in our data. For example, free public transit may allow people to take jobs further from their homes, or jobs with more desirable benefits. It is also possible that the temporary nature of the subsidy limited the extent to which people changed behavior on this margin.

6.3 Secondary outcomes

Following our pre-analysis plan, we consider a number of secondary outcomes. The cross-sectional treatment effects for these outcomes are reported in Table 4. As in Table 2, we present control and treatment group means along with regression-adjusted differences. For each coefficient estimate, we again report a heteroskedasticity-robust standard error and p-value as well as a sharpened FDR q-value that adjusts for multiple hypothesis testing.

6.3.1 Public assistance

We find little evidence that transit subsidies help connect study participants to public benefits. The first panel of Table 4 shows these results. For indicators of receiving any benefits and receiving food benefits three months after study enrollment, we observe null effects of the treatment. However, there is limited scope for the transit subsidy to affect these outcomes; due to the way in which study enrollment was conducted at DSHS offices, over 90% of individuals in the experiment receive SNAP in the first quarter after random assignment. Control group rates of receiving TANF cash assistance or other program benefits are lower, at 2% and 13%, respectively. Still, the treatment group appears no more likely to access these assistance programs, suggesting that free transit does not help people sign up for or maintain public benefits.[14]

[14]Event studies and panel regressions confirm the absence of any impacts of the treatment on public benefit receipt; see Appendix Figure A1 and Appendix Table A2.

6.3.2 Finances

Despite no change in access to financial resources from employment or public benefits, we find some suggestive evidence that transit subsidies improve the financial situation of the treatment group, at least in the short run. We match a sub-sample of study participants to credit records. The second panel of Table 4 shows results using credit-related outcomes in the first full quarter after enrollment.[15] Based on our regressions with full controls (column (6)), total debt balances are $97 (5%) lower for the treatment group and credit scores are 13 points (3%) higher. In this smaller sample, neither of these estimates is statistically significant even prior to adjusting for multiple hypothesis testing. However, the point estimates are large; for example, the credit score effect is over half the size of that associated with economically important events like being evicted (Collinson et al., 2023) or having a bankruptcy removed from one's record (Gross, Notowidigdo and Wang, 2020). Consistent with the strong immediate impact of free fares on transit use, any effects on treated participants' financial situations also appear soon after random assignment, as shown in the event studies in panels (a) and (b) of Figure 5. Other variables observed on credit reports further suggest improved financial situations. For instance, we see members of the treatment group seeking less new credit after random assignment. Measured one quarter after study enrollment, individuals in the treatment group have made 0.08 (24%) fewer new credit inquiries in the past three months. This difference suggests that the financial situation of those who receive free transit improves such that they do not need to open new lines of credit. We similarly find a negative effect of treatment on credit inquiries in our panel data model. However, the pooled treatment effect estimates are more mixed for total debt balances and credit scores, suggesting that improvements in financial circumstances may be short-lived.[16]

[15] These outcomes reflect circumstances at the end of the relevant quarter.

[16] See Appendix Table A3.

6.3.3 Contact with the criminal justice system

We find some indication that the transit subsidy reduces contact with the criminal justice system. As the third panel of Table 4 shows, arrest rates among individuals in the treatment group in the three months after study enrollment are 1.5 percentage points lower than those in the control group, at 11.1% vs. 13.6%. While the cross-sectional estimate is not statistically significant, it amounts to an economically meaningful 11% decline in the likelihood of arrest within three months. In addition, we find a very similar magnitude (-1.4 percentage points) and statistically significant effect of free transit access on arrests when we pool post-enrollment periods with our panel approach.[17] The relative declines in arrests appear to be driven primarily by reductions in gross misdemeanors; when we break out treatment effects by specific crime types, we find that the treatment is associated with relatively large declines in arrests for theft, trespassing, probation violations, and failure to comply with officers.[18] These arguably represent the types of crimes that improved mobility, or the eased financial constraint owing to free transit, might help to avert. In contrast, we see no evidence of impacts of free transit fares on crimes with less of a financial motive or where transportation is less likely to have posed an important obstacle, such as assaults, sex crimes, domestic violence, custody violations, alcohol/drug violations, or weapons violations. Although they are only suggestive, taken together these results indicate that free public transportation may reduce individuals' likelihood of coming into contact with the criminal justice system.

6.3.4 Healthcare use

People receiving transit subsidies appear less likely to use healthcare. The fourth panel of Table 4 shows average healthcare use during the first three months after study enrollment, as measured by Medicaid claims records. Our first pre-specified healthcare outcome, the cost of Medicaid services, is $77 lower for the treatment group relative to the control group. How-

[17]See Appendix Table A2. We show event study estimates for arrests in panel (c) of Figure A1.
[18]See Appendix Table A4.

ever, the estimate for health care costs is imprecise; the lower bound of the 95% confidence interval corresponds to a decline of $404, or 41% of the control group mean. We have greater power for detecting changes in healthcare visits. In the control group, 34.7% of participants have a healthcare visit of some kind within three months of random assignment. This value is 5.6 percentage points lower in the treatment group; the simple regression-adjusted difference in the probability of a healthcare visit is statistically significant at the 5% level based on the unadjusted p-value (and the 10% level based on the sharpened FDR q-value). Panel (c) of Figure 5 shows that the effect on healthcare visits materializes within three months of study enrollment and does not grow in magnitude subsequently. Our pooled treatment effect estimates further confirm that the impacts are concentrated in the months immediately following random assignment.[19] Most of the decline is driven by outpatient visits, and in particular non-emergency outpatient visits. Such visits decline by 5.0 percentage points from a base of 29.8%. That outpatient visits drive the main result and are also less expensive than inpatient visits helps explain why we cannot detect effects on total cost measures.

6.3.5 Residential location

We do not detect large changes in residential mobility in response to transit subsidies. The final panel of Table 4 shows these results for the sub-sample of study participants that match to consumer reference records. Overall rates of moving are relatively low. In the three months after random assignment, only 1.2% of the control group made any residential move. Move rates within three months are somewhat lower in the treatment group at 1.0%; the regression-adjusted difference is -0.3 percentage points. While the point estimate is small in magnitude, the 95% confidence interval admits changes in move rates that are large relative to baseline. Our pooled treatment effect estimates are more precise and closer to zero, but we still cannot rule out sizable impacts of free fares on residential mobility.[20]

The residential address data also help address concerns about sample attrition. The data

[19]See Appendix Table A2.

[20]See Appendix Table A5. We show event study estimates for residential moves in panel (b) of Figure A1.

on employment, public benefits, arrests, and healthcare use all cover the state of Washington; people moving out of state will exit those data. The address history data indicate that any such potentially selective attrition is low. As panel E of Table 4 shows, only 0.5% of the control group and 0.3% of the treatment group move out of state within three months.

6.4 Heterogeneous effects

The average treatment effects we estimate may mask heterogeneity in impacts across subgroups. Understanding any heterogeneity in effects is important from a program targeting perspective. It can also speak to how specific our results are to the particular study sample. For example, the lack of observed effects on paid hours worked and other employment-related outcomes may stem in part from study participants' relatively low overall attachment to the labor force. If few individuals in our study are on the margin of working for pay, then public transit access might have a muted average effect on employment in our sample but a large effect in the full population of people with low income.[21]

In the data, we find limited evidence of heterogeneity in effects for most outcomes, including for our primary employment-related outcomes. We explore heterogeneity first by estimating effects for various subgroups, and then using the causal tree method of Athey and Imbens (2016). Table 5 shows heterogeneous effects estimated for different subgroups. The first panel shows results with paid hours worked as the outcome. The first two columns contrast effects for participants who are unemployed versus employed at baseline, measured as having zero versus positive paid hours worked at $\tau = -1$. Conditional on being employed at baseline, individuals in the control group work for pay an average of 118 hours in the quarter after random assignment. Those in the treatment group work 8 more hours on average. This subgroup treatment effect is somewhat larger than the full-sample estimate, but is small in practical terms and not statistically different from either zero or the subgroup

[21]Notably, our sample is broadly representative of the low-income population in King County. Our study draws participants primarily from the pool of individuals enrolling in SNAP, which is one of the broadest public assistance programs. As discussed in Section 3, our study also had high rates of participation.

effect for people not employed at baseline.

The lack of heterogeneity in effects for paid hours worked is not an artifact of focusing on particular sample splits. The remainder of the first row of Table 5 shows that we cannot detect heterogeneity in effects on paid hours worked for sample splits based on the 75th percentile of baseline earnings, gender, vehicle ownership, race, or Medicaid eligibility. As shown in subsequent panels of Table 5, there is also little indication of heterogeneity in impacts for any employment or for public benefit receipt. The null average effects we observe for these outcomes seems to be broadly representative of the effects for different subpopulations.[22]

We find some limited evidence of heterogeneity in effects on healthcare use. The fifth panel of Table 5 displays results for any healthcare visit. In these subgroup tests, we find evidence of larger declines in healthcare use for participants who are White and who have earnings above the 75th percentile. While even less pronounced, we also find some indication of heterogeneity in effects on arrests, with stronger negative treatment effects among women.

We detect similar patterns of heterogeneity using the causal tree method developed by Athey and Imbens (2016). Their data-driven approach can identify important dimensions of heterogeneity in effects, and at the same time provide unbiased subgroup-specific point estimates and confidence intervals. Using their approach, we find no evidence of heterogeneous effects for any employment-related outcomes.[23] On the other hand, their method identifies some heterogeneity in effects for healthcare outcomes, pointing to potentially stronger impacts of free transit for those with a recent history of medical visits. There also appears to be some heterogeneity in effects for arrests, with impacts varying by educational attainment, gender, and prior public assistance receipt. However, an omnibus F-test of heterogeneity cannot reject the null of no heterogeneity in the causal forest for healthcare use or arrests.

[22]For the full set of outcomes related to employment, public benefit receipt, and arrests, as well as results with and without controls, see Appendix Tables A6–A10. We also find limited evidence of heterogeneity in impacts for financial outcomes from the credit reporting data; see Appendix Tables A11 and A12.

[23]See Appendix Table A13. We provide more details on the methodology in the notes to the table.

7 Discussion and survey-based evidence

Our results suggest that, while not affecting employment, free transit may improve well-being across several areas of recipients' lives. We observe decreased use of healthcare, which could indicate either better health or reduced healthcare access. We find the former explanation more likely for three reasons. First, theory would suggest that free transit access should make it easier rather than harder for participants to visit a doctor, hospital, or clinic.[24] Second, other examples exist where improved health leads to less healthcare use. For instance, one medical trial found that experimentally-induced exercise reduced later use of outpatient care (Buchner et al., 1997). Third, small-sample survey results suggest that self-reported well-being improves. As discussed in Section 4, we surveyed a sub-sample of participants from the second cohort and asked a series of questions about changes in well-being in different areas of life over the prior 2 months. Outcomes in each case are measured on a Likert scale from 1 to 5. The top panel of Table 6 reports these results. Relative to those in the control group, individuals in the treatment group report improvements in well-being in several domains, including not just transportation, but also health. Interpreting these survey outcomes is somewhat difficult; the small sample and survey non-response makes the measures noisy and potentially measured with bias. They are consistent, though, with the idea that reductions in healthcare use reflect improvements in health. More generally, they align with prior research documenting a positive association between transit use and indicators of health (MacDonald et al., 2010; Martin, Goryakin and Suhrcke, 2014).

The survey results also indicate greater financial well-being among individuals who received access to free transit. This echoes the previous findings based on credit reports. However, improved well-being does not necessarily extend to all areas of life. Based on the surveys, subjective well-being in the areas of education and housing do not increase; the

[24] An alternative explanation is that individuals in the treatment group were more likely to transition off Medicaid, in which case we would not observe their healthcare visits. However, given we find no impacts on employment (and hence potential access to employer-provided private health insurance) or on other public benefit receipt, we also view this explanation as unlikely.

latter result is consistent with the limited residential mobility response to the treatment as measured in the consumer reference data.

These diffuse improvements in several areas of life reflect how participants expect to and actually do use transit. At baseline, we asked participants to state if they would use transit more if it were free. Among the 99% who responded positively, we asked if they would use free transit to expand travel for each of ten different activities. Figure 6 shows the results. While 52% of study participants said they would use it to travel to work, this category only ranked sixth out of ten. More participants expected to use to the transit card for shopping (71%), errands (62%), visiting family and friends (61%), using healthcare (60%), and visiting the public benefits office (56%). Measuring trip purposes for actual trips taken is more difficult; we must rely on follow-up surveys for a small and selected sample. The bottom panel of Table 6 shows how people who have at least one transit trip sampled for the survey split their transit trips across different trip purposes. Treatment effects are difficult to measure with precision, but the small sample can provide a sense of how common different trip types are in general.[25] Averaging across treatment and control, respondents with at least one sampled transit trip use 33% of their transit trips for work. The other two-thirds of their transit trips are for non-work purposes, particularly shopping, errands, visiting family and friends, recreation, and using healthcare.

The lack of employment effects coupled with improvements in financial and other indicators of well-being might suggest that income effects associated with the subsidy are important. Rather than the transit benefit itself affecting individuals' lives, the money freed up due to the transfer could be driving the effects. In that case, the cash equivalent of the in-kind transfer might have similar impacts. We believe this is unlikely for three reasons. First, based on either per trip fares or the cost of monthly passes, the cash equivalent of the transfer was at most $200, which represents only about 2.5% of average annual earnings

[25]The proportion of trips for work is 21 percentage points higher in the treatment group as compared to the control group, but the 95% confidence interval ranges from 0 to 43 percentage points. Similarly, the 95% confidence interval for the effect of the treatment on shopping trips ranges from -8 to 41 percentage points.

among individuals in our sample. Second, the transit subsidy sharply increased transit use relative to what would be expected with a cash transfer. According to the 2019 Consumer Expenditure Survey, the budget share for transportation as a whole (not just for public transit) among households in the bottom quintile of the income distribution is 16%; even if individuals would have allocated that entire fraction of a $200 transfer to transit alone, it would translate into 21 additional trips, or less than one-fifth the additional trips we observed as a result of the treatment. Finally, other recent work on one-time small-scale cash transfers to similar populations point to little impact on measures of hardship or subjective well-being. Small cash transfers can have large effects for people who are very poor (Haushofer and Shapiro, 2016) or have experienced large negative shocks (Phillips and Sullivan, 2023). But for a broad set of people living in poverty in the U.S., small unconditional cash transfers during the COVID-19 pandemic had smaller effects than what we observe (Jacob et al., 2022; Pilkauskas et al., 2023; Jaroszewicz et al., 2022). For example, Jaroszewicz et al. (2022) use an RCT to examine the impacts of unconditional cash assistance and find that cash transfers ranging from $500 to $2000 create very short-term increases in spending but no lasting positive effects on bank balances or on self-reported financial well-being and health. The income effects alone of the transit card we study, whose cash equivalent is only 10-40% the size of these cash transfers, are therefore unlikely to explain more than a small fraction of the impacts we observe.

8 Conclusion

This paper reports the results of a randomized controlled trial that provided several months of fare-free public transportation to individuals with low income. Among a group of people enrolling in public benefits in the Seattle area during 2019 and 2020, we compare how recipients of free transit differ from people who pay $1.50 per bus ride on a rich set of outcomes derived from administrative and proprietary data. We do not detect large effects of free transit access on employment outcomes. However, transit appears to have benefits outside the confines of the formal labor market for low-income individuals. We find suggestive evidence that free transit improves study participants' health. We also find that the treatment is associated positively with participants' financial situations and negatively with the likelihood of contact with the criminal justice system.

Follow-up surveys of study participants corroborate these results, pointing to impacts of free transit fares on the travel habits as well as the well-being of individuals with low incomes. Our results are also consistent with other experiments focused on in-kind transfers to families with low incomes, including the Moving to Opportunity (MTO) experiment, that find limited effects on objective measures of economic self-sufficiency, but significant improvements in health and subjective well-being (Ludwig et al., 2012).

The results from this study might not generalize to a broader population of low-income individuals, and in particular one with stronger labor force attachment. However, checks for heterogeneity in treatment effects, including tests using recently developed causal tree methods, indicate that the impacts of free transit access on employment and most other outcomes do not differ substantially by prior labor force attachment or across other subgroups. It is also possible that, while sufficient to affect some aspects of individuals' lives, the subsidy did not last long enough to influence decisions about employment or residential location. Future work leveraging the introduction of permanent, at-scale free-fare programs may be able to speak to this issue, as well as shed more light on the potential general equilibrium implications of subsidized fare policies.

Our results suggest that fare-free transit generates important welfare benefits that would be missed by prominent and influential economic models of urban location. Such models typically quantify benefits of transit access based on changes in the costs associated with traveling from home to work (Severen, 2021; Tsivanidis, 2022). In principle, however, spatial frictions matter for any activity requiring travel: working for pay, accessing public benefits, utilizing healthcare, shopping, visiting family, and so on. Our results indicate that travel behavior of low-income individuals responds elastically to the price of transit, and that those individuals use free transit for a wide variety of activities, not just paid work. We find that this additional travel may generate health and financial benefits, despite little change in labor market outcomes or neighborhood choice. Thus, even in a context where public transportation has limited effects on formal employment and residential location, it can have important welfare benefits for people with low income.

References

Abebe, Girum, A. Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn. 2021. “Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City.” *Review of Economic Studies*, 88(3): 1279–1310.

Ahlfeldt, Gabriel, Stephen Redding, Daniel Sturm, and Nikolaus Wolf. 2015. “The Economics of Density: Evidence from the Berlin Wall.” *Econometrica*, 83(6): 2127–2189.

Almagro, Milena, and Tomás Domínguez-Iino. 2022. “Location Sorting and Endogenous Amenities: Evidence from Amsterdam.” University of Chicago, Booth School of Business Working Paper.

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

Athey, Susan, and Guido Imbens. 2016. “Recursive Partitioning for Heterogeneous Causal Effects.” *Proceedings of the National Academy of Sciences*, 113(27): 7353–7360.

Bartik, Timothy. 2020. “Using Place-Based Jobs Policies to Help Distressed Communities.” *Journal of Economic Perspectives*, 34(3): 99–127.

Barwick, Panle Jia, Shanjun Li, Andrew Waxman, Jing Wu, and Tianli Xia. 2021. “Efficiency and Equity Impacts of Urban Transportation Policies with Equilibrium Sorting.” National Bureau of Economic Research Working Paper No. 29012.

Benjamini, Yoav, Abba Krieger, and Daniel Yekutieli. 2006. “Adaptive Linear Step-Up Procedures That Control the False Discovery Rate.” *Biometrika*, 93(3): 491–507.

Billings, Stephen, Suzanne Leland, and David Swindell. 2011. “The Effects of the Announcement and Opening of Light Rail Transit Stations on Neighborhood Crime.” *Journal of Urban Affairs*, 33(5): 549–566.

Brough, Rebecca, Matthew Freedman, and David C. Phillips. 2020. “A (Free) Ticket to Ride: Experimental Evidence on the Effects of Means-Tested Public Transportation Subsidies.” AEA RCT Registry, RCT ID AEARCTR-0005538.

Brough, Rebecca, Matthew Freedman, and David C. Phillips. 2021. “Understanding Socioeconomic Disparities in Travel Behavior during the COVID-19 Pandemic.” *Journal of Regional Science*, 61(4): 753–774.

Brough, Rebecca, Matthew Freedman, and David C. Phillips. 2022. “Experimental Evidence on the Effects of Means-Tested Public Transportation Subsidies on Travel Behavior.” *Regional Science and Urban Economics*, 96: 103803.

Brough, Rebecca, Matthew Freedman, Daniel E. Ho, and David C. Phillips. 2022. “Can Transportation Subsidies Reduce Failures to Appear in Criminal Court? Evidence from a Pilot Randomized Controlled Trial.” *Economics Letters*, 216: 110540.

Brown, Barbara, and Carol Werner. 2008. "Before and After a New Light Rail Stop: Resident Attitudes, Travel Behavior, and Obesity." *Journal of the American Planning Association*, 75(1): 5–12.

Buchner, David, M. Elaine Cress, Barbara De Lateur, Peter Esselman, Anthony Margherita, Robert Price, and Edward Wagner. 1997. "The Effect of Strength and Endurance Training on Gait, Balance, Fall Risk, and Health Services Use in Community-Living Older Adults." *Journals of Gerontology Series A: Biological Sciences and Medical Sciences*, 52(4): M218–M224.

Bull, Owen, Juan Carlos Munoz, and Hugo Silva. 2021. "The Impact of Fare-Free Public Transport on Travel Behavior: Evidence from a Randomized Controlled Trial." *Regional Science and Urban Economics*, 86: 103616.

Busch-Geertsema, Annika, Martin Lanzendorf, and Nora Klinner. 2021. "Making Public Transport Irresistible? The Introduction of a Free Public Transport Ticket for State Employees and its Effects on Mode Use." *Transport Policy*, 106: 249–261.

Busso, Matias, Jesse Gregory, and Patrick Kline. 2013. "Assessing the Incidence and Efficiency of a Prominent Place Based Policy." *American Economic Review*, 103(2): 897–947.

Carlsson, Magnus, and Stefan Eriksson. 2023. "Do Employers Avoid Hiring Workers from Poor Neighborhoods? Experimental Evidence from the Real Labor Market." *Scandinavian Journal of Economics*, 125(2): 376–402.

Cats, Oded, Yusak Susilo, and Triin Reimal. 2017. "The Prospects of Fare-Free Public Transport: Evidence from Tallinn." *Transportation*, 44(5): 1083–1104.

Cervero, Robert. 2013. *Suburban Gridlock.* Transaction Publishers.

Chernoff, Alex, and Andrea Craig. 2022. "Distributional and Housing Price Effects from Public Transit Investment: Evidence from Vancouver." *International Economic Review*, 63(1): 475–509.

Chetty, Raj, and Nathaniel Hendren. 2018. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *Quarterly Journal of Economics*, 133(3): 1107–1162.

Chetty, Raj, John Friedman, Nathaniel Hendren, Maggie Jones, and Sonya Porter. 2018. "The Opportunity Atlas: Mapping the Childhood Roots of Social Mobility." National Bureau of Economic Research Working Paper No. 25147.

Chetty, Raj, Nathaniel Hendren, and Lawrence Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review*, 106(4): 855–902.

Chyn, Eric. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children." *American Economic Review*, 108(10): 3028–3056.

Collinson, Robert, John Humphries, Nicholas Mader, Davin Reed, Daniel Tannenbaum, and Winnie Van Dijk. 2023. "Eviction and Poverty in American Cities." *Quarterly Journal of Economics*, forthcoming.

Cools, Mario, Yannick Fabbro, and Tom Bellemans. 2016. "Free Public Transport: A Socio-Cognitive Analysis." *Transportation Research Part A: Policy and Practice*, 86: 96–107.

Diaz, Ana Maria, and Luz Magdalena Salas. 2020. "Do Firms Redline Workers?" *Regional Science and Urban Economics*, 83: 103541.

Dupas, Pascaline. 2014. "Getting Essential Health Products to Their End Users: Subsidize, But How Much?" *Science*, 345(6202): 1279–1281.

Finkelstein, Amy, Sarah Taubman, Bill Wright, Mira Bernstein, Jonathan Gruber, Joseph Newhouse, Heidi Allen, Katherine Baicker, and Oregon Health Study Group. 2012. "The Oregon Health Insurance Experiment: Evidence from the First Year." *Quarterly Journal of Economics*, 127(3): 1057–1106.

Franklin, Simon. 2018. "Location, Search Costs and Youth Unemployment: Experimental Evidence from Transport Subsidies." *Economic Journal*, 128: 2353–2379.

Freeland, Amy, Shailendra Banerjee, Andrew Dannenberg, and Arthur Wendel. 2013. "Walking Associated with Public Transit: Moving Toward Increased Physical Activity in the United States." *American Journal of Public Health*, 103(3): 536–542.

Fujita, M. 1991. *Urban Economic Theory.* Cambridge University Press.

Gross, Tal, Matthew Notowidigdo, and Jialan Wang. 2020. "The Marginal Propensity to Consume over the Business Cycle." *American Economic Journal: Macroeconomics*, 12(2): 351–84.

Haushofer, Johannes, and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." *Quarterly Journal of Economics*, 131(4): 1973–2042.

Holzer, Harry, John Quigley, and Steven Raphael. 2003. "Public Transit and the Spatial Distribution of Minority Employment: Evidence from a Natural Experiment." *Journal of Policy Analysis and Management*, 22(3): 415–441.

Jacob, Brian, Natasha Pilkauskas, Elizabeth Rhodes, Katherine Richard, and H. Luke Shaefer. 2022. "The COVID-19 Cash Transfer Study II: The Hardship and Mental Health Impacts of an Unconditional Cash Transfer to Low-Income Individuals." *National Tax Journal*, 75(3): 597–625.

Jardim, Ekaterina, Mark Long, Robert Plotnick, Emma van Inwegen, Jacob Vigdor, and Hilary Wething. 2022. "Minimum-Wage Increases and Low-Wage Employment: Evidence from Seattle." *American Economic Journal: Economic Policy*, 14(2): 263–314.

Jaroszewicz, Ania, Jon Jachimowicz, Oliver Hauser, and Julian Jamison. 2022. "How Effective Is (More) Money? Randomizing Unconditional Cash Transfer Amounts in the U.S." SSRN Working Paper No. 4154000.

Kain, John F. 1968. "Housing Segregation, Negro Employment, and Metropolitan Decentralization." *Quarterly Journal of Economics*, 82(2): 175–197.

Kärmeniemi, Mikko, Tiina Lankila, Tiina Ikäheimo, Heli Koivumaa-Honkanen, and Raija Korpelainen. 2018. "The Built Environment as a Determinant of Physical Activity: A Systematic Review of Longitudinal Studies and Natural Experiments." *Annals of Behavioral Medicine*, 52(3): 239–251.

Lachowska, Marta, Alexandre Mas, and Stephen Woodbury. 2020. "Sources of Displaced Workers' Long-Term Earnings Losses." *American Economic Review*, 110(10): 3231–66.

Li, Fei, and Christopher Wyczalkowski. 2023. "How Buses Alleviate Unemployment and Poverty: Lessons from a Natural Experiment in Clayton, GA." *Urban Studies*, forthcoming.

Ludwig, Jens, Greg Duncan, Lisa Gennetian, Lawrence Katz, Ronald Kessler, Jeffrey Kling, and Lisa Sanbonmatsu. 2012. "Neighborhood Effects on the Long-Term Well-Being of Low-Income Adults." *Science*, 337(6101): 1505–1510.

MacDonald, John, Robert Stokes, Deborah Cohen, Aaron Kofner, and Greg Ridgeway. 2010. "The Effect of Light Rail Transit on Body Mass Index and Physical Activity." *American Journal of Preventive Medicine*, 39(2): 105–112.

Martin, Adam, Yevgeniy Goryakin, and Marc Suhrcke. 2014. "Does Active Commuting Improve Psychological Wellbeing? Longitudinal Evidence from Eighteen Waves of the British Household Panel Survey." *Preventive Medicine*, 69: 296–303.

McCormack, Gavin, and Alan Shiell. 2011. "In Search of Causality: A Systematic Review of the Relationship Between the Built Environment and Physical Activity Among Adults." *International Journal of Behavioral Nutrition and Physical Activity*, 8: 1–11.

Miyauchi, Yuhei, Kentaro Nakajima, and Stephen Redding. 2022. "The Economics of Spatial Mobility: Theory and Evidence Using Smartphone Data." National Bureau of Economic Research Working Paper No. 28497.

Monte, Ferdinando, Stephen Redding, and Esteban Rossi-Hansberg. 2018. "Commuting, Migration, and Local Employment Elasticities." *American Economic Review*, 108(12): 3855–90.

Mulalic, Ismir, and Jan Rouwendal. 2020. "Does Improving Public Transport Decrease Car Ownership? Evidence from a Residential Sorting Model for the Copenhagen Metropolitan Area." *Regional Science and Urban Economics*, 83: 103543.

Pavelle, Bridget, Taylor Danielson, Barbara Lucenko, and Barbara Felver. 2019. "Basic Food Client Characteristics: Working-Age Adults Receiving Food Assistance in Washington State." Washington State Department of Social and Health Services.

Phillips, David C. 2014. "Getting to Work: Experimental Evidence on Job Search and Transportation Costs." *Labour Economics*, 29: 72–82.

Phillips, David C. 2020*a*. "Do Low-Wage Employers Discriminate Against Applicants with Long Commutes?: Evidence from a Correspondence Experiment." *Journal of Human Resources*, 55(3): 864–901.

Phillips, David C. 2020*b*. "Measuring Housing Stability with Consumer Reference Data." *Demography*, 57(4): 1323–1344.

Phillips, David C., and Danielle Sandler. 2015. "Does Public Transit Spread Crime? Evidence from Temporary Rail Station Closures." *Regional Science and Urban Economics*, 52: 13–26.

Phillips, David C., and James Sullivan. 2023. "Do Homelessness Prevention Programs Prevent Homelessness? Evidence from a Randomized Controlled Trial." *Review of Economics and Statistics*, forthcoming.

Pilkauskas, Natasha, Brian Jacob, Elizabeth Rhodes, Katherine Richard, and H. Luke Shaefer. 2023. "The COVID Cash Transfer Study: The Impacts of a One-Time Unconditional Cash Transfer on the Well-Being of Families Receiving SNAP in Twelve States." *Journal of Policy Analysis and Management*, 42(3): 771–795.

Renalds, Arlene, Tracey Smith, and Patty Hale. 2010. "A Systematic Review of Built Environment and Health." *Family and Community Health*, 31(1): 68–78.

Ridgeway, Greg, and John MacDonald. 2017. "Effect of Rail Transit on Crime: A Study of Los Angeles from 1988 to 2014." *Journal of Quantitative Criminology*, 33: 277–291.

Rosenblum, Jeffrey. 2020. "Expanding Access to the City: How Public Transit Fare Policy Shapes Travel Decision Making and Behavior of Low-Income Riders." PhD diss. Massachusetts Institute of Technology, Department of Urban Studies and Planning.

Saelens, Brian, Anne Vernez Moudon, Bumjoon Kang, Philip Hurvitz, and Chuan Zhou. 2014. "Relation Between Higher Physical Activity and Public Transit Use." *American Journal of Public Health*, 104(5): 854–859.

Severen, Christopher. 2021. "Commuting, Labor, and Housing Market Effects of Mass Transportation: Welfare and Identification." *Review of Economics and Statistics*, forthcoming.

Stokes, Robert, John MacDonald, and Greg Ridgeway. 2008. "Estimating the Effects of Light Rail Transit on Health Care Costs." *Health & Place*, 14(1): 45–58.

Tsivanidis, Nick. 2022. "Evaluating the Impact of Urban Transit Infrastructure: Evidence from Bogota's TransMilenio." UC Berkeley Working Paper.

Tyndall, Justin. 2021. "The Local Labour Market Effects of Light Rail Transit." *Journal of Urban Economics*, 124: 103350.

Volinski, Joel. 2012. *Implementation and Outcomes of Fare-Free Transit Systems.* Transit Cooperative Research Program, Transportation Research Board.

Webb, Elizabeth, Gopalakrishnan Netuveli, and Christopher Millett. 2012. "Free Bus Passes, Use of Public Transport and Obesity Among Older People in England." *Journal of Epidemiology & Community Health*, 66(2): 176–180.

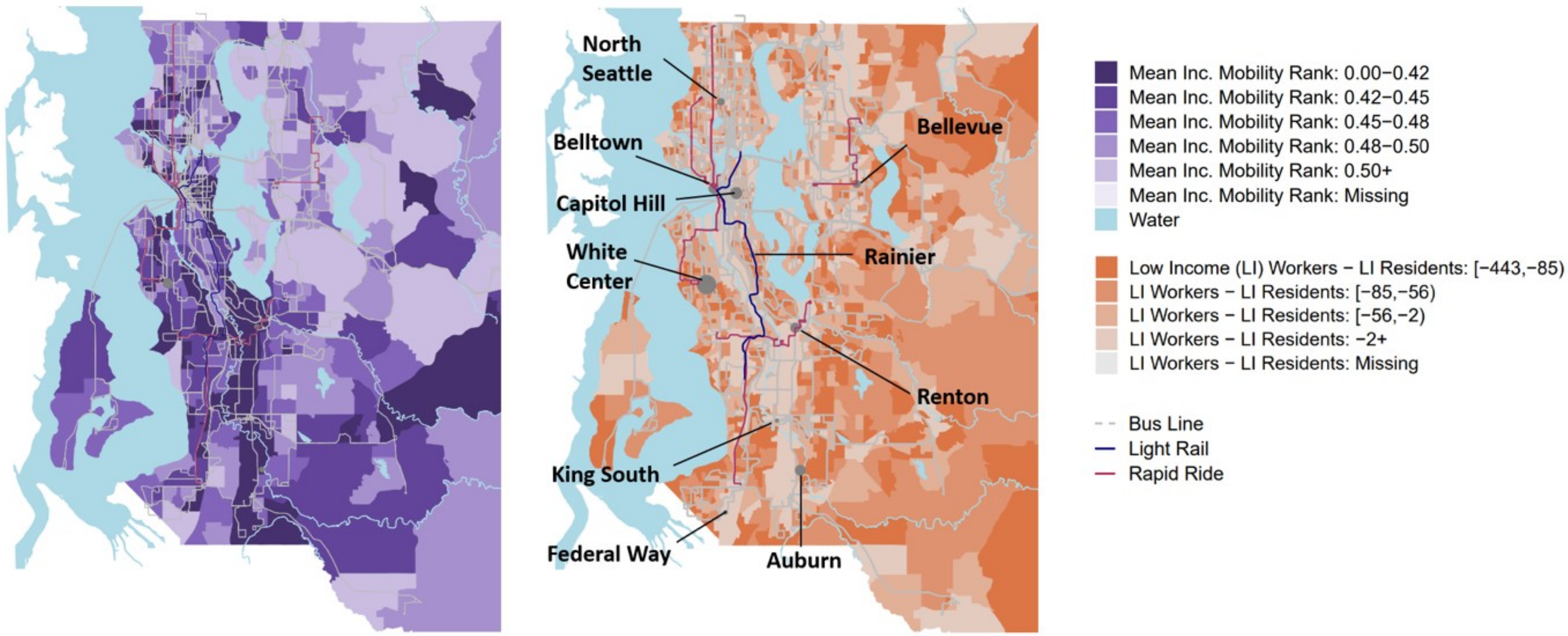
Wilson, William Julius. 1997. *When Work Disappears: The World of the New Urban Poor.* Vintage Books.

Zax, Jeffrey, and John F. Kain. 1991. "Commutes, Quits, and Moves." *Journal of Urban Economics*, 29(2): 153–165.

Zenou, Yves. 2009. *Urban Labor Economics.* Cambridge University Press.

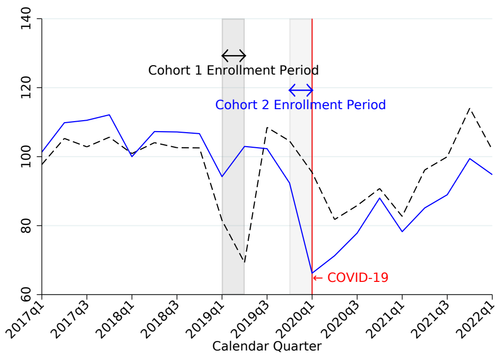
Figures

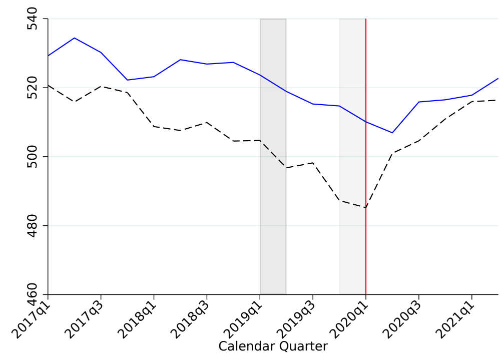
Figure 1. Economic Mobility and Spatial Mismatch in King County, Washington



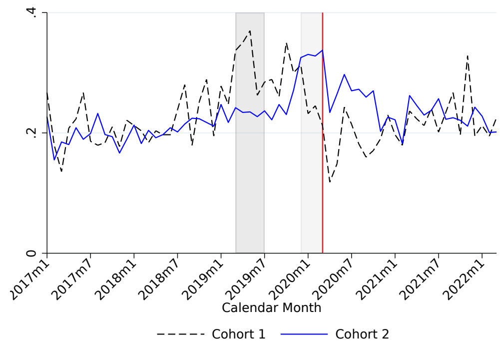
Notes: These are maps of the western portion of King County, Washington, which is the location of Seattle. In the first map, census tracts are shaded based on economic mobility measures provided by Chetty et al. (2018). Specifically, we plot the pooled (by race and gender) "kid family rank" measure for children growing up in a household in the 25th income percentile. This mobility metric reflects the average income rank of a child growing up in a given tract in a family with income in the 25th percentile by the time they are 31-37 years old. Shading brackets are based on data quintiles. In the second map, census block groups are shaded based on the difference betweeen the number of low-income workers (defined as earning less than or equal to $1,250 per month) and low-income residents (defined as earning less than or equal to $1,250 per month) using 2018 LODES data. Shading brackets are based on data quartiles. The extent of the transit network in both maps is shown as of 2019. The ten King County DSHS Community Service Offices (CSOs) where enrollment occurred are marked by gray dots in the second map. The sizes of the dots correspond to the proportion of the sample who enrolled at each CSO.

Figure 2. Mean Outcomes, by Calendar Time and Cohort





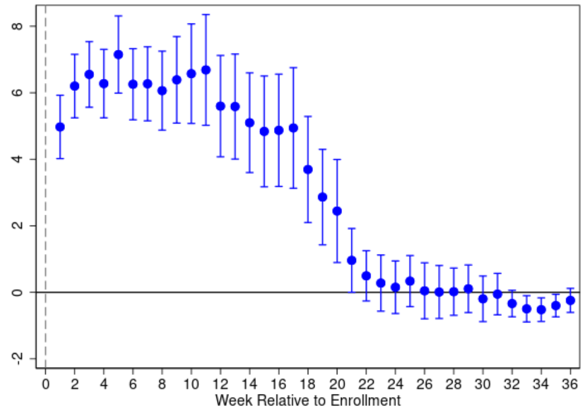
(a) Paid Hours Worked (b) Credit Scores



(c) Medical Visits

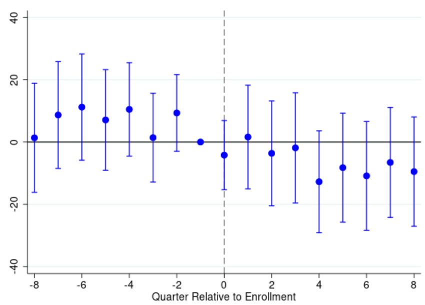
Notes: These figures display trends in mean (a) paid hours worked, (b) credit scores, and (c) Medicaid-covered doctor, clinic, or hospital visits by cohort. Paid hours worked (Washington State UI records) and credit scores (Experian) are measured at a quarterly frequency, while Medicaid visits are measured at a monthly frequency. Means for cohort 1 are shown as black dashed lines. Means for cohort 2 are shown as solid blue lines. The dark gray shading corresponds to the time frame during which cohort 1 enrolled the study (March-July 2019). The light gray shading corresponds to the time frame during which cohort 2 enrolled the study (December 2019-March 2020). The red vertical line denotes March 2020, when COVID-19 cases begin to rise in King County and when King County Metro stop charging fares for services.

Figure 3. Treatment Effects on Transit Boardings, by Relative Time

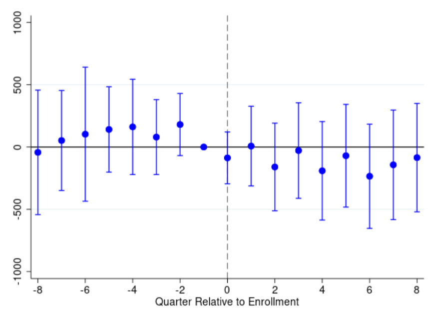


Notes: This figure depicts treatment effects on transit card use over time. Each dot measures the treatment effect of receiving free public transit at the relative week indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcome is the number of transit boardings for which an ORCA card was used. Control variables include indicators for randomization regime, female, Black, Hispanic, non-White, and the month of study enrollment as well as age and age squared. The vertical lines represent 95% confidence intervals, computed using heteroskedasticity-robust standard errors.

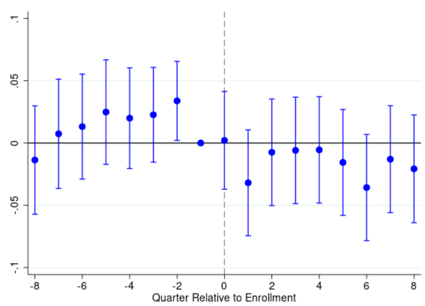
Figure 4. Treatment Effects on Employment Outcomes, by Relative Time



(a) Quarterly Paid Hours Worked



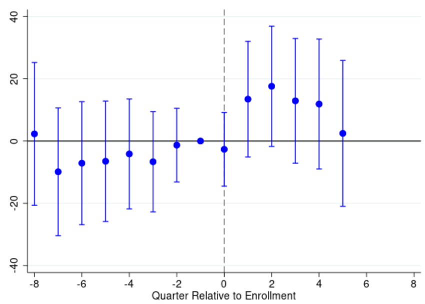
(b) Quarterly Earnings



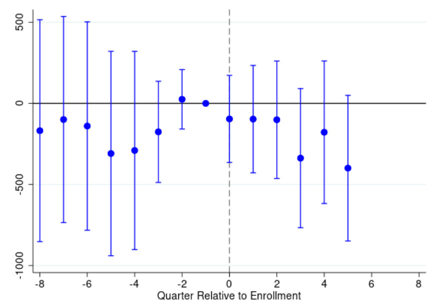
(c) Any Paid Employment

Notes: This figure depicts treatment effects on (a) paid hours worked, (b) earnings, and (c) any paid employment over time. Each dot measures the treatment effect of receiving free public transit at the relative quarter indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). Outcomes are measured using Washington State UI records. Control variables are the outcome in the period prior to random assignment as well as indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment; participant age is not available in the state administrative records. The vertical lines represent 95% confidence intervals, computed using heteroskedasticity-robust standard errors.

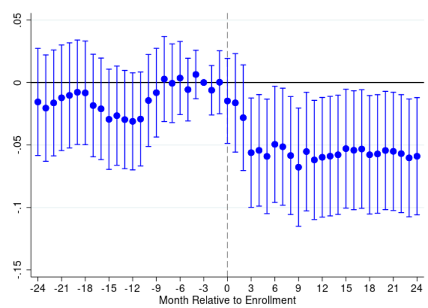
Figure 5. Treatment Effects on Financial and Health Outcomes, by Relative Time



(a) Credit Score



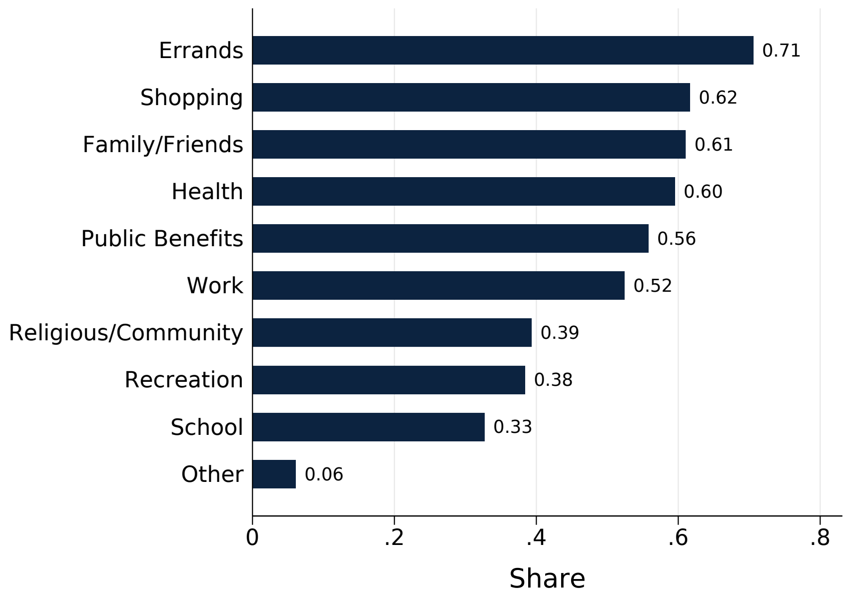
(b) Balance in Collections



(c) Medical Visits, Cumulative

Notes: This figure depicts treatment effects on (a) credit scores, (b) balance in collections, and (d) medical visits measured cumulatively over time. Each dot measures the treatment effect of receiving free public transit at the relative time indicated on the horizontal axis (quarter in (a) and (b), month in (c)). Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcomes in (a) and (b) come from quarterly cross sections of Experian credit reports (only available up to 5 quarters after enrollment) while the outcome in (c) comes from monthly summaries of Medicaid records. Control variables are the outcome 3 months (or 1 quarter) prior to random assignment and indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment. Figures (a) and (b) additionally control for age and age squared. The vertical lines represent 95% confidence intervals, computed using heteroskedasticity-robust standard errors.

Figure 6. Anticipated Uses of Public Transit Services if Free, Measured at Baseline



Notes: This figure shows the fraction of cohort 2 study participants indicating in the baseline survey that they would use transit more for each option, conditional on reporting that they would use transit more if it were free. Of the 1,312 people in cohort 2 responding to the baseline survey, 1,298 indicated they would use transit more if it were free. The figure shows responses to a follow-up question for those 1,298 individuals that asked, "If you used public transit more, where would you go?" Fractions add up to more than one because respondents could respond in the positive to all options that apply.

Tables

Table 1. Mean Baseline Characteristics by Treatment Assignment

	(1)	(2)	(3)	(4)	(5)
	Control		Treatment		Simple Reg.
	Mean	N	Mean	N	Adj. Diff.
Demographic Characteristics Measured at Baseline					
White	0.42	977	0.41	621	-0.02 (0.03)
Hispanic	0.09	977	0.08	621	-0.01 (0.01)
Black	0.28	977	0.29	621	0.01 (0.02)
Female	0.41	977	0.39	621	-0.02 (0.03)
Years of education	11.94	849	12.10	552	0.17 (0.11)
Owns vehicle	0.20	977	0.17	621	-0.02 (0.02)
Outcomes Measured at $\tau = -1$					
State Administrative Records					
Paid hours worked	99	977	109	621	9 (9)
Total earnings	1,955	977	2,110	621	46 (190)
Any paid employment	0.33	977	0.36	621	0.01 (0.02)
Any food or cash benefits	0.60	977	0.59	621	-0.01 (0.03)
Any arrest, cumulative	0.12	977	0.10	621	-0.02 (0.02)
Any misdemeanor, cumulative	0.02	977	0.01	621	-0.003 (0.01)
Any gross misdemeanor, cumulative	0.04	977	0.03	621	-0.01 (0.01)
Any felony, cumulative	0.04	977	0.03	621	-0.01 (0.01)
Eligible for Medicaid	0.60	977	0.58	621	-0.01 (0.03)
Cost to Medicaid, cumulative	613	977	806	621	162 (132)
Any Medicaid visit, cumulative	0.24	977	0.24	621	-0.001 (0.02)
Experion Data					
Credit score	516	473	509	323	-8 (13)
Balance in collection	1,930	473	1,558	323	-311 (332)
Infutor Data					
Any move	0.01	432	0.01	290	0.00003 (0.01)

Notes: This table presents means and regression-adjusted differences in means for baseline characteristics. The demographic characteristics shown in the top panel are derived from the study's intake survey and Metro's ORCA LIFT registry. The pre-study enrollment ($\tau = -1$) outcome data shown in the bottom panel are derived from state administrative records, Experian credit records, and Infutor consumer reference data. Different match rates across these datasets result in different sample sizes. Demographics are measured at the time of study enrollment; educational attainment data are incomplete for individuals matching to state administrative records, and so are only reported for 1,401 individuals. Paid hours worked, earnings, and any paid employment are measured one quarter prior to enrollment. Public benefit receipt is measured three months prior to enrollment. Arrests and health visits and costs are measured cumulatively over the three months prior to enrollment. Credit scores and debt balances are measured one quarter before enrollment, and residential moves are measured cumulatively over the three months prior to enrollment. Column (5) presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses.

Table 2. Employment Outcomes, One Quarter After Study Enrollment

	(1) Control	(2) Treatmet	(3) Simple Reg. Adj. Diff.	(4) Reg. Adj. Diff.
Paid hours worked	76.8	81.5	5.6 (8.9)	1.6 (8.5)
			[0.53] {1.00}	[0.85] {1.00}
Earnings	1,459	1,477	48 (170)	8 (163)
			[0.78] {1.00}	[0.96] {1.00}
Any paid employment	0.32	0.30	-0.02 (0.02)	-0.03 (0.02)
			[0.33] {1.00}	[0.14] {1.00}
Job gain	0.13	0.11	-0.03 (0.02)	-0.03 (0.02)
			[0.10] {1.00}	[0.08] {1.00}
Job loss	0.14	0.15	0.01 (0.02)	0.01 (0.02)
			[0.58] {1.00}	[0.61] {1.00}
Continuous employment	0.19	0.19	0.004 (0.02)	0.001 (0.02)
			[0.84]{1.00}	[0.96]{1.00}
–Continuous sector employment	0.13	0.13	0.003 (0.02)	0.004 (0.02)
			[0.85] {1.00}	[0.83] {1.00}
–Continuous industry employment	0.11	0.11	0.004 (0.02)	0.006 (0.02)
			[0.80] {1.00}	[0.69] {1.00}
Continuous unemployment	0.54	0.55	0.01 (0.03)	0.02 (0.03)
			[0.61] {1.00}	[0.47] {1.00}
N	977	621		

Notes: This table presents means and regression-adjusted differences in means for employment outcomes measured in the quarter after enrollment ($\tau = +1$) using Washington State UI records. Continuous employment, job gains, and job losses are measured comparing the quarter before and the quarter after enrollment. Sectors and industries are defined by 2-digit and 6-digit NAICS codes, respectively. Column (3) presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Column (4) additionally adjusts for race, gender, month of study enrollment, and the relevant outcome one quarter prior to study enrollment (for paid hours worked, earnings, and any paid employment outcomes only). The sample is limited to individuals who go through random assignment and match to any Washington State administrative record prior to study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses and the associated p-values are reported in brackets. Sharpened FDR q-values that adjust for multiple hypothesis testing are reported in braces.

Table 3. Employment Outcomes, Panel Regressions

	(1) Paid Hours Worked	(2) Earnings	(3) Any Paid Employment
Treated	-0.5	-48	0.001
	(7.5)	(148)	(0.02)
Person Fixed Effects	✓	✓	✓
Calendar Quarter Fixed Effects	✓	✓	✓
Relative Quarter Fixed Effects	✓	✓	✓
Control Mean	96.3	1,822	0.31
Observations	27,166	27,166	27,166
Individuals	1,598	1,598	1,598

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome on an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable equals zero for individuals in the control group and equals the fraction of a quarter in which the treatment is active for those in the treatment group. The panel consists of 8 quarters prior to study enrollment and 8 quarters following study enrollment for all sample individuals. The sample is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are reported in parentheses.

Table 4. Secondary Outcomes, One Quarter After Enrollment

	(1) Control Mean	(2) N	(3) Treatment Mean	(4) N	(5) Simple Reg. Adj. Diff.	(6) Reg. Adj. Diff.
A. Public Assistance Receipt, measured three months post enrollment						
Any food or cash benefits	0.93	977	0.91	621	-0.02 (0.01)	-0.02 (0.01)
					[0.18] {1.00}	[0.24] {1.00}
–SNAP	0.91	977	0.89	621	-0.02 (0.02)	-0.02 (0.02)
					[0.12] {0.90}	[0.16] {0.99}
–TANF	0.02	977	0.03	621	0.01 (0.01)	0.00 (0.01)
					[0.41] {1.00}	[0.71] {1.00}
–Other	0.13	977	0.11	621	-0.02 (0.02)	-0.01 (0.01)
					[0.23] {1.00}	[0.33] {1.00}
B. Financial Health, measured in the third month of the quarter post enrollment						
Balance in Collection	1,622	492	1,364	334	-220 (220)	-97 (169)
					[0.32] {1.00}	[0.57] {1.00}
Credit Score	501	492	514	334	9 (14)	13 (9)
					[0.50] {1.00}	[0.16] {0.99}
Total Inquiries in Past 3 Months	0.34	492	0.26	334	-0.10 (0.04)	-0.08 (0.04)
					[0.01] {0.09}	[0.05] {0.44}
C. Criminal Justice, measured three months post enrollment						
Any arrest, cumulative	0.14	977	0.11	621	-0.02 (0.02)	-0.02 (0.02)
					[0.20] {1.00}	[0.35] {1.00}
Any misdemeanor, cumulative	0.02	977	0.01	621	-0.00 (0.01)	-0.00 (0.01)
					[0.79] {1.00}	[0.90] {1.00}
Any gross misdemeanor, cumulative	0.05	977	0.04	621	-0.01 (0.01)	-0.01 (0.01)
					[0.51] {1.00}	[0.59] {1.00}
Any felony, cumulative	0.06	977	0.05	621	-0.00 (0.01)	-0.00 (0.01)
					[0.77] {1.00}	[0.85] {1.00}
D. Healthcare, measured three months post enrollment						
Cost to Medicaid, cumulative	975	977	913	621	-43 (176)	-77 (167)
					[0.81] {1.00}	[0.64] {1.00}
Any Medicaid visit, cumulative	0.35	977	0.28	621	-0.06 (0.02)	-0.06 (0.02)
					[0.01] {0.09}	[0.012] {0.29}
–Emergency outpatient	0.25	977	0.21	621	-0.03 (0.02)	-0.03 (0.02)
					[0.12] {0.90}	[0.12] {0.99}
–Emergency inpatient	0.04	977	0.04	621	-0.01 (0.01)	-0.01 (0.01)
					[0.43] {1.00}	[0.39] {1.00}
–Non-emergency outpatient	0.30	977	0.24	621	-0.06 (0.02)	-0.05 (0.02)
					[0.01] {0.09}	[0.021] {0.29}
–Non-emergency inpatient	0.02	977	0.02	621	-0.00 (0.01)	0.00 (0.01)
					[0.91] {1.00}	[1.00] {1.00}
E. Residential Mobility, measured three months post enrollment						
Any move	0.012	432	0.010	290	-0.003 (0.008)	-0.003 (0.008)
					[0.73] {1.00}	[0.71] {1.00}
Any move in state	0.007	432	0.010	290	0.003 (0.006)	0.002 (0.007)
					[0.66] {1.00}	[0.75] {1.00}
Any move out of state	0.005	432	0.003	290	-0.003 (0.005)	-0.002 (0.005)
					[0.55] {1.00}	[0.67] {1.00}
Any move in county	0.005	432	0.010	290	0.005 (0.006)	0.004 (0.007)
					[0.42] {1.00}	[0.51] {1.00}
Any move out of county	0.007	432	0.003	290	-0.005 (0.005)	-0.004 (0.005)
					[0.34] {1.00}	[0.43] {1.00}

Notes: This table presents means and regression-adjusted differences in means for outcomes measured in the quarter after enrollment. Public assistance receipt comes from Washington State Economic Services Administration records and is measured 3 months after random assignment. Financial measures cover the sample that matches to a repeated cross-section of quarterly Experian credit reports and reflect outcomes measured 1 quarter after random assignment. Criminal justice contact measures come from Washington State Patrol records and are measured cumulatively between random assignment and three months later. Healthcare information come from Washington State administrative records on Medicaid claims and is also measured cumulatively between random assignment and 3 months later; cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Residential moves cover a sample that matches to any address from Infutor consumer reference data prior to random assignment; moves are measured cumulatively between random assignment and 3 months later. Column (5) presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Column (6) additionally adjusts for indicators for race, month of study enrollment, and the relevant outcome 1 quarter prior to study enrollment; results in Panels A, C, and D also include controls for gender; results in Panels B and E control for age and age squared. Heteroskedasticity-robust standard errors are reported in parentheses and the associated p-values are reported in brackets. Sharpened FDR q-values that adjust for multiple hypothesis testing are reported in braces.

Table 5. Heterogeneity Tests for Selected Outcomes, One Quarter After Enrollment

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Paid hours worked												
Control Mean	43	118	54	152	81	71	71	103	51	96	124	57
Reg. Adj. Diff.	-5	8	-3	4	-5	9	1	1	4	2	17	-3
SE	(9)	(15)	(8)	(22)	(11)	(13)	(9)	(24)	(9)	(13)	(22)	(8)
P-Value of Diff.	[0.47]		[0.76]		[0.39]		[0.98]		[0.75]		[0.38]	
Employed for pay												
Control Mean	0.17	0.51	0.24	0.59	0.32	0.32	0.30	0.42	0.24	0.38	0.47	0.26
Reg. Adj. Diff.	-0.04	-0.04	-0.04	-0.05	-0.04	-0.02	-0.02	-0.08	-0.00	-0.05	-0.07	-0.01
SE	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.03)	(0.02)	(0.06)	(0.03)	(0.03)	(0.05)	(0.02)
P-Value of Diff.	[1.00]		[0.73]		[0.65]		[0.33]		[0.20]		[0.24]	
Any public benefits												
Control Mean	0.94	0.92	0.93	0.93	0.93	0.93	0.93	0.94	0.96	0.91	0.91	0.94
Reg. Adj. Diff.	-0.01	0.01	0.00	-0.01	-0.00	0.01	-0.01	0.03	0.00	-0.00	0.02	-0.01
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
P-Value of Diff.	[0.28]		[0.61]		[0.61]		[0.19]		[0.91]		[0.19]	
Any arrest, cumulative												
Control Mean	0.17	0.09	0.16	0.06	0.18	0.07	0.16	0.06	0.14	0.13	0.14	0.13
Reg. Adj. Diff.	-0.02	-0.01	-0.01	-0.01	-0.00	-0.03	-0.01	-0.02	-0.02	-0.01	-0.03	-0.01
SE	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.47]		[0.80]		[0.30]		[0.84]		[0.78]		[0.58]	
Any Medicaid visit, cumulative												
Control Mean	0.37	0.32	0.36	0.31	0.33	0.37	0.35	0.35	0.43	0.29	0.19	0.41
Reg. Adj. Diff.	-0.01	-0.02	0.01	-0.07	-0.02	-0.02	-0.02	0.01	-0.08	0.03	-0.01	-0.02
SE	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.70]		[0.08]		[0.82]		[0.58]		[0.01]		[0.98]	
N - Control Mean	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects. Each outcome is measured one quarter after enrollment. Employed at baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff." is based on a regression of the outcome of interest on a treatment indicator, randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Similarly, we do not control for employment outcomes in the quarter prior to enrollment in columns 1-4. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The p-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.".

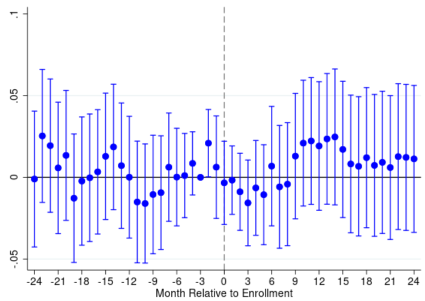
Table 6. Follow-Up Survey Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		Treatment		Simple Reg.	Reg.
	Mean	N	Mean	N	Adj. Diff.	Adj. Diff.
Well-Being Measures						
Transportation well-being	3.02	125	3.21	124	0.21 (0.14)	0.25 (0.14)
					[0.13] {1.00}	[0.07] {0.66}
Employment well-being	2.52	126	2.71	124	0.20 (0.16)	0.24 (0.16)
					[0.20] {1.00}	[0.14] {0.66}
Financial well-being	2.44	126	2.68	124	0.25 (0.16)	0.26 (0.17)
					[0.13] {1.00}	[0.13] {0.66}
Health well-being	2.98	125	3.05	123	0.07 (0.13)	0.13 (0.12)
					[0.59] {1.00}	[0.31] {0.78}
Housing well-being	2.97	125	2.99	125	0.02 (0.14)	-0.01 (0.14)
					[0.89] {1.00}	[0.97] {1.00}
Education well-being	3.36	122	3.32	124	-0.03 (0.12)	-0.04 (0.12)
					[0.78] {1.00}	[0.73] {1.00}
Share of Public transit trips, by Purpose						
Share of transit trips for work	0.23	44	0.42	53	0.20 (0.11)	0.21 (0.11)
					[0.07] {1.00}	[0.05] {0.66}
Share of transit trips for health	0.08	44	0.10	53	0.02 (0.06)	0.02 (0.07)
					[0.77] {1.00}	[0.76] {1.00}
Share of transit trips for public benefits	0.08	44	0.05	53	-0.03 (0.06)	-0.04 (0.07)
					[0.65] {1.00}	[0.52] {0.92}
Share of transit trips for shopping	0.31	44	0.46	53	0.15 (0.13)	0.16 (0.13)
					[0.26] {1.00}	[0.20] {0.74}
Share of transit trips for errands	0.36	44	0.15	53	-0.21 (0.12)	-0.26 (0.13)
					[0.08] {1.00}	[0.04] {0.66}
Share of transit trips for family/friends	0.21	44	0.12	53	-0.09 (0.09)	-0.07 (0.11)
					[0.33] {1.00}	[0.52] {0.92}
Share of transit trips for recreation	0.17	44	0.15	53	-0.01 (0.09)	0.01 (0.08)
					[0.93] {1.00}	[0.87] {1.00}
Share of transit trips for religious/community	0.00	44	0.02	53	0.02 (0.02)	0.02 (0.02)
					[0.32] {1.00}	[0.34] {0.78}
Share of transit trips for school	0.05	44	0.01	53	-0.03 (0.04)	-0.05 (0.06)
					[0.45] {1.00}	[0.38] {0.78}
Share of transit trips for other purpose	0.08	44	0.03	53	-0.05 (.04)	-0.04 (0.04)
					[0.21] {1.00}	[0.32] {0.78}

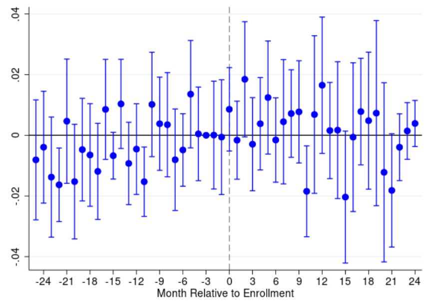
Notes: This table shows outcomes from self-reported surveys conducted by phone and by web in the year following study enrollment. The survey began in March 2020 and continued through December 2020; however, this table only reports results from surveys during which the treatment is effective (prior to March 18, 2020 and after October 1, 2020). Details of the survey are described in Section 4. The upper panel reports well-being measures where participants are asked to describe how their well-being in certain areas has changed in the past 2 months, with responses placed on a 1-5 Likert scale (1 being "much worse" and 5 being "much better"). The upper panel reports responses from 250 respondents. The sample size for some fields is smaller (e.g. 246 respondents for education) due to individuals responding that they do not know or that the field is not applicable. The lower panel shows the share of public transit trips for each trip purpose conditional on taking any public transit trip; of the 250 respondents, 97 report taking at least one public transit trip. Column (5) reports the regression-adjusted difference in means between columns (1) and (3), controlling for the randomization regime. Column (6) additionally controls for month of enrollment and location of study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses and the associated p-values are reported in brackets. Sharpened FDR q-values that adjust for multiple hypothesis testing are reported in braces.

Appendix Figures and Tables

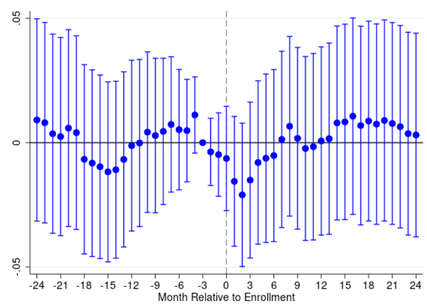
Figure A1. Treatment Effects on Secondary Outcomes, by Relative Time



(a) Any public benefits



(b) Any move



(c) Any arrest, cumulative

Notes: This figure depicts treatment effects on (a) food or cash benefit receipt from DSHS, (b) residential moves, and (c) arrests measured cumulatively over time. Each dot measures the treatment effect of receiving free public transit at the relative month indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcomes in each figure come from monthly data provided by RDA, except for (b) which originates from Infutor. Control variables are the outcome 3 months prior to random assignment and indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment. The vertical lines represent 95% confidence intervals, computed using heteroskedasticity-robust standard errors.

Table A1. Mean Baseline Characteristics by Treatment Assignment

	(1)	(2)	(3)	(4)	(5)
	Control		Treatment		Simple Reg.
	Mean	N	Mean	N	Adj. Diff.
Demographics at Baseline					
Age at enrollment	39.66	1105	40.88	692	1.05 (0.63)
White	0.41	1105	0.39	692	-0.03 (0.02)
Black	0.29	1105	0.29	692	0.00 (0.02)
Hispanic	0.09	1105	0.08	692	-0.01 (0.01)
Asian	0.03	1105	0.05	692	0.02 (0.01)
American Indian	0.01	1105	0.01	692	0.00 (0.01)
Pacific Islander	0.02	1105	0.03	692	0.01 (0.01)
Multi-racial	0.05	1105	0.05	692	0.01 (0.01)
Missing race	0.04	1105	0.03	692	-0.01 (0.01)
Transit Use at Baseline					
Used transit at all in past 30 days	0.88	1105	0.88	692	0.01 (0.02)
No. days used transit in 30 days prior to enrollment	15.10	1105	15.94	692	1.00 (0.53)
Enrollment Location					
Auburn CSO	0.11	1105	0.08	692	-0.02 (0.01)
Belltown CSO	0.08	1105	0.11	692	0.02 (0.01)
Capitol Hill CSO	0.14	1105	0.12	692	-0.02 (0.02)
Federal Way CSO	0.01	1105	0.02	692	0.01 (0.01)
King East CSO	0.04	1105	0.04	692	-0.01 (0.01)
King South CSO	0.01	1105	0.01	692	-0.00 (0.00)
North Seattle CSO	0.04	1105	0.03	692	-0.01 (0.01)
Rainier CSO	0.02	1105	0.01	692	-0.01 (0.01)
Renton CSO	0.08	1105	0.09	692	0.01 (0.01)
White Center CSO	0.47	1105	0.50	692	0.03 (0.02)

Notes: This table presents means and regression-adjusted differences in means for baseline characteristics for all study participants, including the 1598 participants ultimately matched to administrative records. The demographic characteristics shown in the top panel are derived from the study's intake survey and Metro's ORCA LIFT registry. The second panel corresponds to the location where the participant enrolled in the study. All 10 Community Service Offices (CSO) in King County were enrollment sites, however only Auburn, Capitol Hill, and White Center were enrollment sites prior to December 2019. Office of enrollment is missing for 2 study participants. Column (5) presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses.

Table A2. State Administrative Outcomes, Panel Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Medical						Benefits				Criminal Justice
	Cost to Medicaid Monthly	Any Medicaid visit Monthly	Emergency outpatient	Emergency inpatient	Non-emergency inpatient	Non-emergency outpatient	Any food or cash benefits	SNAP	TANF	Other	Any Arrest
Treated	-18	-0.014	-0.003	-0.001	-0.000	-0.012	-0.006	0.001	-0.001	-0.004	-0.014
	(41)	(0.010)	(0.008)	(0.003)	(0.002)	(0.010)	(0.018)	(0.018)	(0.006)	(0.011)	(0.006)
Person Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Calendar Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Relative Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	142	0.089	0.052	0.008	0.003	0.072	0.620	0.506	0.025	0.055	0.030
Observations	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302
Individuals	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome on an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable equals zero for individuals in the control group and equals the fraction of a quarter in which the treatment is active for those in the treatment group. The panel consists of 24 months prior to study enrollment and 24 months following study enrollment for all sample individuals. The sample is limited to individuals matching to any Washington State administrative record prior to study enrollment. Standard errors clustered by individual are reported in parentheses.

Table A3. Financial Health Outcomes, Panel Regressions

	(1)	(2)	(3)
	Balance in Collections	Credit Score	Credit Inquiries in Past 3 Months
Treated	166	-1	-0.02
	(187)	(6)	(0.03)
Person Fixed Effects	✓	✓	✓
Calendar Quarter Fixed Effects	✓	✓	✓
Relative Quarter Fixed Effects	✓	✓	✓
Control Mean	1,839	516	0.33
Observations	11,061	11,061	11,061
Individuals	872	872	872

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome on an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable equals zero for individuals in the control group and equals the fraction of a quarter in which the treatment is active for those in the treatment group. The panel consists of 8 quarters prior to study enrollment and 5 quarters following study enrollment for all sample individuals. The sample is limited to individuals matching to any credit report prior to study enrollment. Standard errors clustered by individual are reported in parentheses.

Table A4. Criminal Justice Outcomes, One Quarter After Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		Treatment		Simple Reg.	Reg.
	Mean	N	Mean	N	Adj. Diff.	Adj. Diff.
Any arrest	0.136	977	0.111	621	-0.022 (0.017)	-0.015 (0.016)
Crime Category						
–Felony	0.056	977	0.050	621	-0.003 (0.011)	-0.002 (0.011)
–Misdemeanor	0.015	977	0.013	621	-0.002 (0.006)	-0.001 (0.006)
–Gross misdemeanor	0.050	977	0.043	621	-0.007 (0.011)	-0.006 (0.011)
–Unknown	0.078	977	0.066	621	-0.010 (0.013)	-0.004 (0.013)
Crime Type						
–Assault	0.024	977	0.027	621	0.002 (0.008)	0.003 (0.008)
–Theft	0.049	977	0.043	621	-0.005 (0.011)	-0.004 (0.011)
–Sex	0.002	977	0.005	621	0.003 (0.003)	0.003 (0.003)
–Domestic violence	0.011	977	0.011	621	-0.000 (0.006)	0.001 (0.005)
–Custody	0.025	977	0.021	621	-0.001 (0.007)	0.001 (0.007)
–Alcohol/drug	0.018	977	0.021	621	0.003 (0.007)	0.006 (0.007)
–Trespass	0.024	977	0.011	621	-0.011 (0.006)	-0.009 (0.006)
–Reckless driving	0.001	977	0.000	621	-0.001 (0.001)	-0.001 (0.001)
–Vehicle license	0.004	977	0.003	621	-0.000 (0.003)	-0.000 (0.003)
–Weapons	0.004	977	0.005	621	0.001 (0.004)	-0.001 (0.003)
–Probation	0.017	977	0.010	621	-0.008 (0.006)	-0.007 (0.006)
–Murder	0.000	977	0.000	621	0.000 (0.000)	0.000 (0.000)
–Fail to comply	0.046	977	0.035	621	-0.009 (0.010)	-0.007 (0.010)
–Other	0.001	977	0.000	621	-0.001 (0.001)	-0.001 (0.001)

Notes: This table presents means and regression-adjusted differences in means for criminal outcomes measured in the three months after study enrollment. Arrests are measured cumulatively between random assignment and three months later. Column (5) presents the regression-adjusted difference in mean between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Column (6) additionally adjusts for race, gender, month of study enrollment, and the relevant outcome one quarter prior to study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses.

Table A5. Residential Mobility Outcomes, Panel Regressions

	(1)	(2)	(3)	(4)	(5)
	Any Move	Any Move In WA	Any Move Outside WA	Any Move In King County	Any Move Outside King County
Treated	0.006	0.000	0.006	0.001	0.006
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)
Person Fixed Effects	✓	✓	✓	✓	✓
Calendar Month Fixed Effects	✓	✓	✓	✓	✓
Relative Month Fixed Effects	✓	✓	✓	✓	✓
Control Mean	0.014	0.011	0.003	0.009	0.006
Observations	34,790	34,790	34,790	34,790	34,790
Individuals	710	710	710	710	710

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome on an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable equals zero for individuals in the control group and equals the fraction of a quarter in which the treatment is active for those in the treatment group. The panel consists of 24 months prior to study enrollment and 24 months following study enrollment for all sample individuals. The sample is limited to individuals matching to Infutor consumer reference data prior to random assignment. Standard errors clustered by individual are in parentheses.

Table A6. Heterogeneity Tests for Selected Outcomes, One Quarter After Enrollment, No Controls

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-White	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Paid hours worked												
Control Mean	43	118	54	152	81	71	71	103	51	96	124	57
Reg. Adj. Diff.	-4	12	-1	9	-3	18	6	11	3	7	22	-1
SE	(9)	(15)	(8)	(23)	(11)	(15)	(9)	(27)	(10)	(13)	(23)	(8)
P-Value of Diff.	[0.36]		[0.67]		[0.26]		[0.86]		[0.82]		[0.35]	
Employed for pay												
Control Mean	0.17	0.51	0.24	0.59	0.32	0.32	0.30	0.42	0.24	0.38	0.47	0.26
Reg. Adj. Diff.	-0.04	-0.03	-0.03	-0.05	-0.04	-0.00	-0.01	-0.07	-0.01	-0.04	-0.06	-0.01
SE	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)	(0.03)	(0.06)	(0.03)	(0.03)	(0.05)	(0.03)
P-Value of Diff.	[0.83]		[0.81]		[0.47]		[0.37]		[0.48]		[0.37]	
Any public benefits												
Control Mean	0.94	0.92	0.93	0.93	0.93	0.93	0.93	0.94	0.96	0.91	0.91	0.94
Reg. Adj. Diff.	-0.02	-0.02	-0.01	-0.03	-0.02	-0.02	-0.02	-0.01	-0.05	0.00	-0.00	-0.02
SE	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.97]		[0.50]		[1.0]		[0.87]		[0.09]		[0.49]	
Any arrest, cumulative												
Control Mean	0.17	0.09	0.16	0.06	0.18	0.07	0.16	0.06	0.14	0.13	0.14	0.13
Reg. Adj. Diff.	-0.03	-0.01	-0.01	-0.03	-0.02	-0.03	-0.03	0.01	0.00	-0.04	-0.05	-0.01
SE	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.62]		[0.70]		[0.68]		[0.25]		[0.21]		[0.28]	
Any Medicaid visit, cumulative												
Control Mean	0.37	0.32	0.36	0.31	0.33	0.37	0.35	0.35	0.43	0.29	0.19	0.41
Reg. Adj. Diff.	-0.05	-0.07	-0.03	-0.14	-0.05	-0.08	-0.06	-0.08	-0.14	-0.00	-0.04	-0.07
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.06)	(0.04)	(0.03)	(0.04)	(0.03)
P-Value of Diff.	[0.69]		[0.03]		[0.50]		[0.70]		[0.00]		[0.46]	
N - Control	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects. Each outcome is measured one quarter after enrollment. Employed at baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. All other variables are defined as before. The coefficient reported in row "Reg. Adj. Diff." is based on a regression of the outcome of interest on a treatment indicator and randomization regime for the listed sub-group. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in row "P-Value of Diff.".

Table A7. Employment Outcomes, Heterogeneity, With Controls

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Paid hours worked												
Control Mean	43	118	54	152	81	71	71	103	51.18	96	124	57
Reg. Adj. Diff.	-5	8	-3	4	-5	9	1	1	4	2	17	-3
SE	(9)	(15)	(8)	(22)	(11)	(13)	(9)	(24)	(9)	(13)	(22)	(8)
P-Value of Diff.	[0.47]		[0.76]		[0.39]		[0.98]		[0.75]		[0.38]	
Earnings												
Control Mean	765	2296	946	3137	1565	1306	1294	2141	972	1816	2522	1026
Reg. Adj. Diff.	-97	31	-81	-97	-211	282	-1	-10	-34	2	278	-69
SE	(156)	(292)	(140)	(451)	(196)	(280)	(158)	(522)	(169)	(235)	(444)	(138)
P-Value of Diff.	[0.70]		[0.97]		[0.15]		[0.99]		[0.94]		[0.45]	
Employed for pay												
Control Mean	0.17	0.51	0.24	0.59	0.32	0.32	0.30	0.42	0.24	0.38	0.47	0.26
Reg. Adj. Diff.	-0.04	-0.04	-0.04	-0.05	-0.04	-0.02	-0.02	-0.08	-0.00	-0.05	-0.07	-0.01
SE	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.03)	(0.02)	(0.06)	(0.03)	(0.03)	(0.05)	(0.02)
P-Value of Diff.	[1.00]		[0.73]		[0.65]		[0.33]		[0.20]		[0.24]	
Cont. employment between relative quarter -1 and 1												
Control Mean	0.00	0.42	0.08	0.54	0.17	0.21	0.16	0.30	0.15	0.22	0.23	0.18
Reg. Adj. Diff.	0.00	-0.03	0.00	-0.05	-0.03	0.04	0.02	-0.05	-0.02	0.02	0.01	-0.00
SE	(0.00)	(0.04)	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)	(0.03)	(0.04)	(0.02)
P-Value of Diff.	[0.47]		[0.30]		[0.11]		[0.24]		[0.37]		[0.86]	
–Cont. sector employment between relative qtr -1 and 1												
Control Mean	0.01	0.29	0.05	0.39	0.12	0.16	0.12	0.19	0.10	0.16	0.17	0.12
Reg. Adj. Diff.	0.00	-0.01	0.01	-0.06	-0.02	0.04	0.02	-0.04	0.02	-0.00	0.01	-0.00
SE	(0.01)	(0.03)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.02)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.58]		[0.13]		[0.11]		[0.25]		[0.44]		[0.76]	
–Cont. industry employment between relative qtr -1 and 1												
Control Mean	0.00	0.24	0.04	0.34	0.09	0.13	0.09	0.19	0.08	0.12	0.12	0.10
Reg. Adj. Diff.	0.00	-0.00	0.02	-0.06	-0.01	0.03	0.02	-0.05	0.00	0.01	0.02	-0.00
SE	(0.00)	(0.03)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.96]		[0.13]		[0.25]		[0.10]		[1.00]		[0.58]	
Job gain between relative qtr -1 and 1												
Control Mean	0.17	0.09	0.16	0.05	0.15	0.11	0.13	0.13	0.09	0.16	0.24	0.09
Reg. Adj. Diff.	-0.04	-0.01	-0.04	-0.00	-0.02	-0.04	-0.03	-0.02	0.01	-0.06	-0.08	-0.01
SE	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.42]		[0.26]		[0.51]		[0.87]		[0.04]		[0.10]	
Job loss between relative qtr -1 and 1												
Control Mean	0.00	0.30	0.08	0.33	0.14	0.14	0.13	0.18	0.15	0.13	0.17	0.13
Reg. Adj. Diff.	0.00	0.01	-0.02	0.06	0.01	0.00	-0.00	0.06	-0.00	0.02	0.01	0.01
SE	(0.00)	(0.03)	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.82]		[0.13]		[0.83]		[0.26]		[0.55]		[0.99]	
Cont. unemployment between relative quarter -1 and 1												
Control Mean	0.83	0.19	0.68	0.08	0.54	0.54	0.57	0.40	0.60	0.49	0.36	0.61
Reg. Adj. Diff.	0.04	0.03	0.05	-0.00	0.04	-0.00	0.01	0.01	0.01	0.02	0.06	0.00
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.06)	(0.04)	(0.03)	(0.05)	(0.03)
P-Value of Diff.	[0.84]		[0.14]		[0.48]		[0.98]		[0.82]		[0.29]	
N - Control Mean	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects on various employment measures from Washington State UI records. Employed pre-baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling for randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Similarly, we do not control for employment outcomes in the quarter prior to enrollment in columns 1-4. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The p-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.".

Table A8. Employment Outcomes, Heterogeneity, No Controls

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Paid hours worked												
Control Mean	43	118	54	152	81	70.85	71	103	51	96	124	57
Reg. Adj. Diff.	-4	12	-1	9	-3	18	6	11	3	7	22	-1
SE	(9)	(15)	(8)	(23)	(11)	(15)	(9)	(27)	(10)	(13)	(23)	(8)
P-Value of Diff.	[0.36]		[0.67]		[0.26]		[0.86]		[0.82]		[0.35]	
Earnings												
Control Mean	765	2296	947	3137	1565	1306	1294	2141	972	1816	2522	1026
Reg. Adj. Diff.	-101	112	-64	13	-160	353	69	71	-54	97	310	-48
SE	(160)	(299)	(142)	(474)	(205)	(295)	(166)	(579)	(180)	(257)	(451)	(146)
P-Value of Diff.	[0.53]		[0.88]		[0.15]		[1.0]		[0.63]		[0.45]	
Employed for pay												
Control Mean	0.17	0.51	0.24	0.59	0.32	0.32	0.30	0.42	0.24	0.38	0.47	0.26
Reg. Adj. Diff.	-0.04	-0.03	-0.03	-0.05	-0.04	-0.00	-0.01	-0.07	-0.01	-0.04	-0.06	-0.01
SE	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)	(0.03)	(0.06)	(0.03)	(0.03)	(0.05)	(0.03)
P-Value of Diff.	[0.83]		[0.812]		[0.47]		[0.37]		[0.48]		[0.37]	
Cont. employment between relative qtr -1 and 1												
Control Mean	0.00	0.42	0.08	0.54	0.17	0.21	0.16	0.30	0.15	0.22	0.23	0.18
Reg. Adj. Diff.	0.00	-0.02	-0.00	-0.05	-0.02	0.04	0.02	-0.04	-0.02	0.02	0.02	-0.00
SE	(0.00)	(0.04)	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)	(0.03)	(0.04)	(0.02)
P-Value of Diff.	[0.61]		[0.35]		[0.18]		[0.35]		[0.36]		[0.59]	
–Cont. sector employment between relative qtr -1 and 1												
Control Mean	0.01	0.29	0.05	0.39	0.12	0.16	0.12	0.19	0.10	0.16	0.17	0.12
Reg. Adj. Diff.	0.00	-0.02	0.01	-0.07	-0.02	0.04	0.02	-0.05	0.02	-0.01	0.02	-0.00
SE	(0.01)	(0.03)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.02)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.59]		[0.12]		[0.12]		[0.22]		[0.44]		[0.63]	
–Cont. industry employment between relative qtr -1 and 1												
Control Mean	0.00	0.24	0.04	0.34	0.09	0.13	0.09	0.19	0.08	0.12	0.12	0.10
Reg. Adj. Diff.	0.00	-0.01	0.01	-0.06	-0.01	0.03	0.02	-0.06	0.00	0.00	0.02	-0.00
SE	(0.00)	(0.03)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.83]		[0.11]		[0.29]		[0.10]		[1.0]		[0.48]	
Job gain between relative qtr -1 and 1												
Control Mean	0.17	0.09	0.16	0.05	0.15	0.11	0.13	0.13	0.09	0.16	0.24	0.09
Reg. Adj. Diff.	-0.04	-0.01	-0.03	0.00	-0.02	-0.04	-0.03	-0.03	0.01	-0.06	-0.08	-0.01
SE	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.38]		[0.24]		[0.52]		[0.93]		[0.038]		[0.08]	
Job loss between relative qtr -1 and 1												
Control Mean	0.00	0.30	0.08	0.33	0.14	0.14	0.13	0.18	0.15	0.13	0.17	0.13
Reg. Adj. Diff.	0.00	0.00	-0.02	0.05	0.01	0.01	0.00	0.06	-0.00	0.02	0.01	0.01
SE	(0.00)	(0.03)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)	(0.02)	(0.04)	(0.02)
P-Value of Diff.	[0.99]		[0.17]		[0.98]		[0.28]		[0.57]		[0.90]	
Cont. unemployment between relative qtr -1 and 1												
Control Mean	0.83	0.19	0.68	0.08	0.54	0.54	0.57	0.40	0.60	0.49	0.36	0.61
Reg. Adj. Diff.	0.04	0.03	0.05	-0.00	0.03	-0.01	0.01	0.01	0.01	0.02	0.04	-0.00
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.06)	(0.04)	(0.03)	(0.05)	(0.03)
P-Value of Diff.	[0.80]		[0.13]		[0.49]		[0.99]		[0.83]		[0.44]	
N - Control	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects on various employment measures from Washington State UI records. Employed at baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns are calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in the row "P-Value of Diff.".

Table A9. Benefits, Health, Criminal Justice Outcomes, Heterogeneity, With Controls

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any public benefits												
Control Mean	0.94	0.92	0.93	0.93	0.93	0.93	0.93	0.94	0.96	0.91	0.91	0.94
Reg. Adj. Diff.	-0.01	0.01	0.00	-0.01	-0.00	0.01	-0.01	0.03	0.00	-0.00	0.02	-0.01
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.02)	(0.01)
P-Value of Diff.	[0.28]		[0.61]		[0.61]		[0.19]		[0.91]		[0.19]	
SNAP												
Control Mean	0.92	0.90	0.91	0.91	0.92	0.90	0.91	0.91	0.94	0.89	0.88	0.93
Reg. Adj. Diff.	-0.01	0.01	0.00	-0.02	0.00	0.00	-0.01	0.04	0.01	-0.01	0.01	-0.00
SE	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.42]		[0.55]		[0.96]		[0.12]		[0.73]		[0.59]	
TANF												
Control Mean	0.01	0.03	0.02	0.03	0.01	0.05	0.03	0.01	0.01	0.03	0.02	0.03
Reg. Adj. Diff.	0.00	-0.01	-0.00	-0.00	-0.01	0.01	-0.00	-0.00	-0.01	0.00	-0.02	0.01
SE	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.49]		[0.95]		[0.44]		[0.89]		[0.46]		[0.11]	
Other benefits												
Control Mean	0.16	0.09	0.16	0.05	0.15	0.11	0.14	0.10	0.16	0.11	0.11	0.14
Reg. Adj. Diff.	-0.00	-0.03	-0.02	-0.01	-0.02	0.00	-0.02	-0.01	-0.04	-0.00	0.00	-0.02
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.37]		[0.88]		[0.33]		[0.84]		[0.33]		[0.46]	
Cost to Medicaid, cumulative												
Control Mean	982	967	995	912	917	1060	974	982	1216	799	460	1185
Reg. Adj. Diff.	293	-325	175	-443	68	-112	19	-97	28	-13	-83	43
SE	(173)	(136)	(135)	(198)	(153)	(146)	(120)	(291)	(204)	(123)	(64)	(152)
P-Value of Diff.	[0.01]		[0.01]		[0.39]		[0.71]		[1.00]		[0.45]	
Any Medicaid visit, cumulative												
Control Mean	0.37	0.32	0.36	0.31	0.33	0.37	0.35	0.35	0.43	0.29	0.19	0.41
Reg. Adj. Diff.	-0.01	-0.02	0.01	-0.07	-0.02	-0.02	-0.02	0.01	-0.08	0.03	-0.01	-0.02
SE	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.70]		[0.08]		[0.82]		[0.58]		[0.01]		[0.98]	
–Emergency outpatient												
Control Mean	0.26	0.23	0.26	0.20	0.25	0.24	0.26	0.20	0.29	0.21	0.14	0.29
Reg. Adj. Diff.	0.01	-0.01	0.01	-0.03	0.01	-0.02	-0.00	0.02	-0.05	0.03	0.00	-0.00
SE	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.55]		[0.28]		[0.35]		[0.68]		[0.03]		[0.95]	
–Emergency inpatient												
Control Mean	0.04	0.05	0.04	0.05	0.05	0.04	0.04	0.05	0.06	0.03	0.02	0.05
Reg. Adj. Diff.	0.02	-0.02	0.01	-0.03	-0.00	-0.00	-0.00	0.00	-0.01	0.00	-0.01	0.00
SE	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.02]		[0.04]		[0.81]		[0.73]		[0.23]		[0.39]	
–Non-emergency inpatient												
Control Mean	0.02	0.02	0.03	0.02	0.01	0.04	0.02	0.03	0.03	0.02	0.01	0.03
Reg. Adj. Diff.	0.01	-0.01	0.01	-0.01	0.01	0.00	0.01	0.00	0.01	-0.00	0.00	0.01
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
P-Value of Diff.	[0.04]		[0.04]		[0.81]		[0.82]		[0.22]		[0.35]	
–Non-emergency outpatient												
Control Mean	0.31	0.29	0.30	0.28	0.28	0.33	0.30	0.28	0.37	0.24	0.17	0.35
Reg. Adj. Diff.	-0.02	-0.02	-0.00	-0.06	-0.00	-0.05	-0.02	0.01	-0.07	0.02	-0.01	-0.02
SE	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.05)	(0.03)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.94]		[0.18]		[0.27]		[0.48]		[0.05]		[0.88]	
Any arrest, cumulative												
Control Mean	0.17	0.09	0.16	0.06	0.18	0.07	0.16	0.06	0.14	0.13	0.14	0.13
Reg. Adj. Diff.	-0.02	-0.01	-0.01	-0.01	-0.00	-0.03	-0.01	-0.02	-0.02	-0.01	-0.03	-0.01
SE	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.47]		[0.80]		[0.30]		[0.84]		[0.78]		[0.58]	
N - Control Mean	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Employed at baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling for randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The *p*-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.".

Table A10. Benefits, Health, Criminal Justice Outcomes, Heterogeneity, No Controls

	Employed at Baseline		Above 75p Earnings		Sex		Owns Vehicle		Race		Eligible for Medicaid	
	No	Yes	No	Yes	Male	Female	No	Yes	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any public benefits												
Control Mean	0.94	0.92	0.93	0.93	0.93	0.93	0.93	0.94	0.96	0.91	0.91	0.94
Reg. Adj. Diff.	-0.02	-0.02	-0.01	-0.03	-0.02	-0.02	-0.02	-0.01	-0.05	0.00	-0.00	-0.02
SE	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.97]		[0.50]		[1.0]		[0.87]		[0.09]		[0.49]	
SNAP												
Control Mean	0.92	0.90	0.91	0.91	0.92	0.90	0.91	0.91	0.94	0.89	0.88	0.93
Reg. Adj. Diff.	-0.02	-0.03	-0.02	-0.04	-0.03	-0.01	-0.03	-0.01	-0.05	-0.01	-0.00	-0.03
SE	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.84]		[0.49]		[0.52]		[0.59]		[0.15]		[0.45]	
TANF												
Control Mean	0.01	0.03	0.02	0.03	0.01	0.05	0.03	0.01	0.01	0.03	0.02	0.03
Reg. Adj. Diff.	0.01	-0.00	0.01	-0.00	0.00	0.02	0.00	0.02	-0.01	0.02	-0.00	0.01
SE	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.31]		[0.44]		[0.40]		[0.31]		[0.12]		[0.52]	
Other benefits												
Control Mean	0.16	0.09	0.16	0.05	0.15	0.11	0.14	0.10	0.16	0.11	0.11	0.14
Reg. Adj. Diff.	-0.03	-0.01	-0.03	0.02	-0.01	-0.03	-0.02	-0.03	-0.04	-0.00	-0.01	-0.02
SE	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.47]		[0.16]		[0.59]		[0.81]		[0.21]		[0.68]	
Cost to Medicaid, cumulative												
Control Mean	982	967	995	912	917	1060	974	982	1216	799	460	1185
Reg. Adj. Diff.	266	-373	76.82	-352	147	-332	-65	68	-84	-3	-194	13
SE	(257)	(239)	(200)	(365)	(256)	(214)	(178)	(569.24)	(346.31)	(178)	(142)	(239)
P-Value of Diff.	[0.07]		[0.30]		[0.15]		[0.82]		[0.84]		[0.46]	
Any Medicaid visit, cumulative												
Control Mean	0.37	0.32	0.36	0.31	0.33	0.37	0.35	0.35	0.43	0.29	0.19	0.41
Reg. Adj. Diff.	-0.05	-0.07	-0.03	-0.14	-0.05	-0.08	-0.06	-0.08	-0.14	-0.00	-0.04	-0.07
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.06)	(0.04)	(0.03)	(0.04)	(0.03)
P-Value of Diff.	[0.69]		[0.03]		[0.50]		[0.70]		[0.00]		[0.46]	
–Emergency outpatient												
Control Mean	0.26	0.23	0.26	0.20	0.25	0.24	0.26	0.20	0.29	0.21	0.14	0.29
Reg. Adj. Diff.	-0.01	-0.06	-0.01	-0.09	-0.02	-0.06	-0.03	-0.05	-0.08	0.00	-0.01	-0.04
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.24]		[0.05]		[0.27]		[0.67]		[0.07]		[0.50]	
–Emergency inpatient												
Control Mean	0.04	0.05	0.04	0.05	0.05	0.04	0.04	0.05	0.06	0.03	0.02	0.05
Reg. Adj. Diff.	0.01	-0.03	0.00	-0.04	-0.01	-0.01	-0.01	-0.01	-0.01	-0.00	-0.02	-0.00
SE	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.02]		[0.07]		[0.93]		[1.0]		[0.60]		[0.17]	
–Non-emergency inpatient												
Control Mean	0.02	0.02	0.03	0.02	0.01	0.04	0.02	0.03	0.03	0.02	0.01	0.03
Reg. Adj. Diff.	0.00	-0.00	0.00	-0.00	0.01	-0.01	-0.00	-0.00	-0.00	0.00	-0.01	0.00
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.00)	(0.01)
P-Value of Diff.	[0.64]		[0.71]		[0.28]		[0.91]		[0.80]		[0.50]	
–Non-emergency outpatient												
Control Mean	0.31	0.29	0.30	0.28	0.28	0.33	0.30	0.28	0.37	0.24	0.17	0.35
Reg. Adj. Diff.	-0.05	-0.07	-0.03	-0.13	-0.04	-0.08	-0.06	-0.05	-0.14	-0.00	-0.03	-0.07
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.61]		[0.03]		[0.46]		[0.83]		[0.00]		[0.28]	
Any arrest, cumulative												
Control Mean	0.17	0.09	0.16	0.06	0.18	0.07	0.16	0.06	0.14	0.13	0.14	0.13
Reg. Adj. Diff.	-0.03	-0.01	-0.01	-0.03	-0.02	-0.03	-0.03	0.01	0.00	-0.04	-0.05	-0.01
SE	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
P-Value of Diff.	[0.62]		[0.70]		[0.68]		[0.25]		[0.21]		[0.28]	
N - Control	534	443	748	229	579	398	786	191	413	564	283	694
N - Treatment	322	299	451	170	378	243	516	105	253	368	178	443

Notes: This table reports tests for heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Employed at baseline is defined as ever being employed in the 4 quarters pre-enrollment; above 75p earnings is defined as having cumulative earnings greater than $10,209 in the 4 quarters prior to enrollment; eligible for Medicaid is defined as ever being eligible in the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns are calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in the row "P-Value of Diff.".

Table A11. Financial Health, Heterogeneity, With Controls

	Above Median Credit Score		Below Median Debt		Below Median Inquiries	
	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Balance in collection						
Control Mean	2068	1275	2687	261	1801	1494
Reg. Adj. Diff.	-289	-239	-326	70	-606	-84
SE	(399)	(230)	(382)	(79)	(332)	(297)
P-Value of Diff.	[0.88]		[0.30]		[0.24]	
Credit score						
Control Mean	434	553	492	512	486	511
Reg. Adj. Diff.	11	14	15	-9	12	6
SE	(23)	(16)	(17)	(24)	(21)	(19)
P-Value of Diff.	[0.95]		[0.47]		[0.77]	
Total inquiries in past 3 months						
Control Mean	0.37	0.32	0.38	0.29	0.45	0.26
Reg. Adj. Diff.	-0.06	-0.10	-0.12	-0.06	-0.15	-0.05
SE	(0.06)	(0.05)	(0.06)	(0.05)	(0.07)	(0.05)
P-Value of Diff.	[0.68]		[0.35]		[0.21]	
N - Control Mean	215	277	276	216	205	287
N - Treatment	159	175	176	158	126	208

Notes: This table reports tests for heterogeneous treatment effects on financial health. Each financial health outcome is measured 1 quarter (approximately 3 months) post enrollment. Above median credit score, below median debt balance, and below median inquiries measures are calculated among the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff." is the estimated treatment effect from equation (1), controlling for randomization regime, age, age squared, enrollment month, and office of enrollment. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The p-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.".

Table A12. Financial Health, Heterogeneity, No Controls

	Above Median Credit Score		Below Median Debt		Below Median Inquiries	
	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Balance in collection						
Control Mean	2068	1275	2687	261	1801	1494
Reg. Adj. Diff.	-139	-344	-288	23	-567	6
SE	(382)	(239)	(364)	(87)	(328)	(295)
P-Value of Diff.	[0.65]		[0.41]		[0.19]	
Credit score						
Control Mean	434	553	492	512	486	511
Reg. Adj. Diff.	12	16	16	-2	12	5
SE	(21)	(16)	(15)	(24)	(21)	(18)
P-Value of Diff.	[0.86]		[0.52]		[0.80]	
Total inquiries in past 3 months						
Control Mean	0.37	0.32	0.38	0.29	0.45	0.26
Reg. Adj. Diff.	-0.09	-0.11	-0.12	-0.06	-0.16	-0.05
SE	(0.06)	(0.05)	(0.05)	(0.05)	(0.07)	(0.04)
P-Value of Diff.	[0.75]		[0.49]		[0.19]	
N - Control	215	277	276	216	205	287
N - Treatment	159	175	176	158	126	208

Notes: This table reports tests for heterogeneous treatment effects on financial health. Each financial health outcome is measured 1 quarter (approximately 3 months) post enrollment. Above median credit score, below median debt balance, and below median inquiries measures are calculated among the 4 quarters prior to enrollment. The coefficient reported in the row "Reg. Adj. Diff." is the estimated treatment effect from equation (1), controlling only for randomization regime. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns are calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in the row "P-Value of Diff.".

Table A13. Athey and Imbens (2016) Heterogeneity Tests

Outcome	Num. of Leaves	Leaf Categories (Y/N)	F-Stat	F-Stat P-Value
Paid hours worked				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Earnings				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Employed for pay				
– 1 Qtr Post Enrollment	2	Qtrly earnings > $10,000 4 months pre-enrollment	0.848	0.3575
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Any arrest				
– 1 Qtr Post Enrollment	6	HS diploma; sex; received benefits prior to enrollment (x2); eligible for Medicaid prior to enrollment	1.5485	0.1727
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Any Medicaid visit				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	2	Any outpatient visit 4 months pre-enrollment	0.0417	0.8384
– 3 Qtrs Post Enrollment	2	1+ outpatient ER visits 4 months pre-enrollment	0.5077	0.4764
Credit score				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Balance in collections				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Credit inquiries				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA

Notes: This table reports heterogeneity test results obtained by implementing Athey and Imbens' (2016) causal tree package. This package uses a data-driven approach to identify subgroups with shared covariates that have different-sized treatment effects. Subgroups are identified by subsetting the study sample into training and estimation subgroups. All covariates available prior to study enrollment were used as potential covariates for this subsetting. For employment and health outcomes, the set of covariates included race, sex, vehicle ownership, month of enrollment, all outcomes in the 10 quarters before enrollment, and measures of employment "shocks" observed in the year before enrollment, including job gain and job loss. For financial health outcomes, the set of covariates included month of enrollment and all outcomes in the 8 quarters before enrollment. When a meaningful subgroup is identified, it is represented as a different "leaf." If there is no meaningful heterogeneity found, then there exists only 1 leaf (the full sample). When there is more than one leaf, the third column reports the variable that was identified as having different treatment effects. The fourth and fifth columns report the F-statistic and p-value associated with the tests of whether the leaves are statistically different from each other.