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

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**Abstract**

We conduct a randomized controlled trial to study the direct and downstream effects of providing free public transit to individuals with low income. While a subsidy that reduces the price of transit to zero nearly doubles transit use, it does not have economically or statistically 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 suggest that participants use free transit to access a variety of services and amenities such that, contrary to the assumptions of most quantitative models of cities, the benefits of reductions in transit costs primarily accrue from sources other than employment.

**Keywords**: public transportation, transit subsidies, randomized controlled trial
**JEL**: R4, R5, H7

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# 1 Introduction

Governments around the globe have reduced public transit fares in recent years. Worldwide, at least 100 cities now provide public transportation with no fares at all (Barry and Rybus, 2020). Many of these cities are in Europe, but Kansas City and Washington, DC have also recently taken steps to eliminate fares on their transit systems. Efficiency gains from reducing pollution and congestion externalities associated with car travel as well as from taking advantage of returns to scale in transit motivate some of these changes. Just as often, however, equity considerations motivate transit fare reductions (Serebrisky et al., 2009). Reduced transit fares may benefit residents with limited means through improved mobility and direct savings on transportation. As a result, reduced-fare programs in many places, including New York, San Francisco, and Seattle, are means-tested. However, there is limited evidence on the economic impacts of reduced fares for people with low incomes.

This paper studies the effects of free public transit fares on employment, public assistance receipt, financial credit, criminal activity, 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 these outcomes. 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 different 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.

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 fares on employment or take-up of public benefits, we find some suggestive but imprecisely measured evidence of improved finances among a sub-sample of study participants who match to credit report data. Further, while access to free public transit has no detectable effects on the probability of moving residences, there is some indication that it reduces the likelihood of contact with the criminal justice system.

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.

Our study makes several contributions. First, we extend the study of fare-free transit to a range of outcomes beyond travel behavior. 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.

We thus paint a more complete picture of the impacts of free transit on the well-being of individuals with low income.

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. A long-running literature considers the causes and consequences of spatial mismatch (Kain, 1968; Wilson, 1997), or the geographic distribution of employment and residences that leads to differential access to jobs across groups. 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). Meanwhile, many quasi-experimental studies have argued that transportation infrastructure can improve employment outcomes for disadvantaged populations (Holzer, Quigley and Raphael, 2003; Tyndall, 2021; Fiorini and Sanfilippo, 2022; Abu-Qarn and Lichtman-Sadot, 2022; 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 non-work benefits could affect the optimal design of transit systems, which historically have been focused primarily on facilitating commutes to urban cores (Cervero, 2013).[1]

This paper is organized as follows. In the next section, we describe the setting for our RCT. We discuss the design and implementation of our experiment in Section 3. In Section 4, we describe the data and provide descriptive statistics. Section 5 outlines our empirical strategy. In Section 6, we present our main results based on administrative and proprietary data linkages with study participants. We examine the nature and extent of heterogeneity in treatment effects in Section 7. Section 8 further discusses our results and provides supplementary evidence based on participant surveys. Section 9 concludes.

# 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. Figure 1 shows 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.[2]

With a median household income of $106,326, King County skews higher income than the

[1]Our study also relates to the literature on the effects of in-kind transfer programs on individuals' work behavior and well-being. A large body of work considers the impacts of in-kind transfer receipt on labor supply, and often concludes the effects are modest but negative (Moffitt, 2002; Currie, 2003; Hoynes and Schanzenbach, 2015). In contrast, we find that providing free transit as an in-kind benefit to individuals with low income is, at worst, neutral for employment prospects, and could possibly improve recipients' broader financial and health situations.

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

U.S. as a whole at $68,703.[3] These high income levels reflect a historically large technology industry presence and significant employment concentrations downtown and in suburbs east of Seattle, like Bellevue. At the same time, at 9.3%, King County's poverty rate in 2021 was not substantially lower than the national rate of 11.6%. The shading in Figure 1 shows how income levels vary considerably across King County. Many people in our study reside in the southern portion of King County between downtown Seattle and Tacoma, where on average individuals have lower incomes.

# 3 Free Transit Experiment

Our experiment in providing free public transit involved two separate waves of participants, which we refer to as cohorts. Study enrollment for the first cohort occurred March-July 2019, and study enrollment for the second cohort occurred December 2019-March 2020. The two cohorts had similar designs, reached much the same population, and delivered similar treatments. They differed primarily in their 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. 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

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

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.[4] 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.[5]

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

[4]Two-thirds (67%) of individuals who expressed interest in transit benefits enrolled in the study.

[5]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.

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, 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.[6] 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 3-month extension, individuals in the treatment group in the second cohort received between 14 and 27 weeks of free transit.[7]

# 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 individuals' 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

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

[7]Notably, travel by transit was relatively depressed among individuals in both the treatment and control groups in the final quarter of 2020.

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 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 identifiers in 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.[8]

## 4.2 Washington State administrative records

We use several administrative datasets to capture downstream outcomes. 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.[9] These data also allow us to construct measures of job stability, including job starts and exits as well as employment continuity.

Second, individuals are linked with records from the Economic Services Administration of DSHS. Using these records, we can track 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

[8]We also have information on the use of any replacement or supplemental cards for those individuals in the study who received them.

[9]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.

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 records from the Washington State Patrol (WSP). 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, and failure to comply. We observe monthly indicators for each type of arrest.

Fourth, we track individuals' health care 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-funded health care visit by month of healthcare use. We can further break out health care 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.[10]

Washington DSHS's Research and Data Analysis group matched study participants who completed random assignment to state administrative records based on the 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. That is, our study sample includes those who had some record of

[10]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.

employment, public benefit receipt, healthcare, or arrest prior to random assignment. 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.

## 4.3 Proprietary data

In addition to linking individuals in the study to state administrative records, we link individuals to proprietary records to measure financial 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, predicted incomes, debt-to-income ratios, bill delinquency, 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 a non-negligible number of people 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 measure residential mobility using consumer reference address histories. We follow Phillips (2020) in constructing measures of address moves from data compiled by Infutor Data Solutions. These data are derived from consumer reference records (e.g., cell phone bills) and cover the entire United States. They provide exact addresses and move dates by month, which we use to measure if a household moves after random assignment and, if so, where. We match study records to Infutor records using a fuzzy match 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" during the first cohort and via a traditional phone and web survey in the second cohort. Respondents completed questions about travel on the prior day, including information on trip quantity, modes, purposes, and payment methods. Brough, Freedman and Phillips (2022) provide additional details about the survey instruments. In the present paper, 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 well-being questions on a 1 to 5 Likert scale, where 1 is "much worse" and 5 is "much better."[11]

[11] All individuals in the second cohort were eligible to receive the survey containing subjective well-being questions. Among those providing valid phone numbers, 351 individuals were randomly assigned to a more intense outreach effort in which they would be able to respond to the survey by phone (in addition to by web); this intense outreach effort was conducted in early March 2020 and December 2020. All remaining individuals received the survey between March 2020 and December 2020 through web-links sent to emails and by text. We aggregate all responses received in any form (web link or by phone) for this analysis. Additionally, 72 individuals responding to the survey prior to December 2020 were selected to have a second opportunity to respond to the phone survey in December. Survey responses are averaged among any multiple survey responses.

## 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 represents a relatively disadvantaged group of participants with limited labor force attachment. 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, which is 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 hours worked per quarter for the first cohort decline from over 100 to under 80 hours between the quarter before and the quarter of study entry. Similarly, in panel (c) of Figure 2, medical visits exhibit an increase just prior to study enrollment. These declines in 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, which we discuss in the next section.

# 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:

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

In this regression, which we estimate on cross-sections of individuals, $i$ indexes individuals and $\tau$ indexes time relative to study enrollment; depending on the outcome, $\tau$ refers to either 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 $\tau$ after random assignment. The binary variable $T_i$ indicates random assignment to treatment, and the estimate of $\beta_\tau$ measures the difference in average outcomes between treatment and control at time $\tau$. We include covariates $X_i$ that adjust this raw mean difference for two reasons. First, $X_i$ 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, $X_i$ includes variables that reduce residual variance by predicting $Y_{i\tau}$.[12] Since random assignment was at the individual level, we compute heteroskedasticity robust standard errors.

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.[13] However, we also show event study-type figures in which we present estimates of $\beta_\tau$ 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.

## 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 treat-

[12]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.

[13]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.

ment 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 affects 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 (e.g., two quarters after random assignment) 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.[14]

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 $\tau$. 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 $\tau$ 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 time. For example, for an individual in cohort 2 enrolled on January 31, 2020, $\bar{T}_{i,\tau=0} = 2/3$ when outcomes are measured quarterly. The manner in which we define $\bar{T}_{i\tau}$ allows for a simple interpretation of its coefficient, $\gamma$, which will reflect the average causal effect of having fully subsidized transit

[14]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.

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

In addition to our cross-sectional regressions, event study, and panel regression approaches, 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 related to, for example, labor supply and healthcare utilization. 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 7.

### 5.4 Baseline balance

Random assignment successfully balanced baseline characteristics across control and treatment groups in our RCT. Table 1 shows baseline descriptive statistics for our main analysis sample. 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 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, 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 can measure how often study participants used their cards to board public transportation. 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. These results indicate that 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. As discussed in Brough, Freedman and Phillips

(2022), some of this increase could result from the treatment group shifting from untraceable payment methods, like cash, or from non-payment. That paper uses the sub-sample survey to quantify these changes in payment method and concludes 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 large. If the card induces additional travel of one boarding per day for 16 weeks, that would cost the control group \$168 in fares. The price of purchasing the actual monthly passes provided to the treatment group is similar, at \$200. For a group of people with average earnings of \$2,000 per quarter (see Table 1), these represent large expenditures.

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 outcomes, we also show the full time path in event study figures as well as panel regressions that pool treatment effects over longer time horizons.

## 6.2 Labor market outcomes

We observe relatively small changes in UI-covered employment 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 difference between the two group means, which is 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.

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. The change in paid hours worked in the quarter after study enrollment is not statistically different from zero at conventional levels. The 95% confidence interval for the estimate for on 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 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, with a 95% confidence interval spanning -15.0 to 14.2. 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. 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 probably 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 marginally significant 2.9 percentage point decline in job starts (measured as having no recorded 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 recorded 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.

## 6.3 Public assistance

Transit subsidies might also help connect participants to public benefits. However, we find little evidence that the treatment group is more likely to access cash or food 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. On the other hand, control group rates of receiving TANF cash assistance or other program benefits are low, at 2% and 13%, respectively. Still, the treatment group appears no more likely to access these assistance programs, suggesting that transit access does not help people sign up for or maintain public benefits.[15]

[15] Event studies and panel regressions confirm the absence of any impacts of the treatment on public benefit receipt; see Appendix Table A1 for panel regression results. We also show event study estimates for any public food or cash receipt in panel (a) of Appendix Figure A1.

## 6.4 Finances

Despite no change in access to financial resources from employment or public benefits, we find some suggestive evidence that transit subsidies help improve the financial situation of the treatment group. We match a sub-sample of the study participants to credit records. The second panel of Table 4 shows results using credit-related outcomes in the first full quarter after enrollment.[16] 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. However, they are economically meaningful and similar in magnitude to the effect of being evicted (Collinson et al., 2022) or having a bankruptcy removed from one's record (Gross, Notowidigdo and Wang, 2020). Consistent with the strong immediate effect 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. While we do not detect changes in delinquencies, we do see, for instance, 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, which is statistically significant at the 5% level, suggests that the financial situation of those that receive free transit improves such that they do not need to open new lines of credit.[17]

## 6.5 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

[16] These outcomes are measured at a quarterly frequency, but reflect circumstances at the end of the relevant quarter.

[17] We similarly find a negative effect of treatment on credit inquiries in our panel data model, although the pooled treatment effect estimates are more mixed for total debt balances and credit scores; see Appendix Table A2.

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.[18] 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.[19] These arguably represent the types of crimes for which 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. Taken together, these results suggest that providing free public transportation reduces participants' likelihood of coming into contact with the criminal justice system.

### 6.6 Healthcare use

People receiving transit subsidies are 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 pre-specified healthcare outcome, the cost of Medicaid services, is $77 lower for the treatment group relative to the control group. However, 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 baseline mean. We have greater power for detecting changes in healthcare visits. In the control group, 34.7% of participants

[18] See Appendix Table A1. We also show event study estimates for arrests in panels (c) and (d) of Figure A1.

[19] See Appendix Table A3.

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 difference between the two groups in the probability of a healthcare visit is statistically significant at the 5% level. Panels (c) and (d) of Figure 5 show 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.[20] 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.

## 6.7 Residential location

Any changes in residential location in response to transit subsidies appear to be small. We are able to match a sub-sample of 722 study participants to consumer reference address history data, which we use to measure rates of residential moves. The final panel of Table 4 displays these results. 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 treatment effect is -0.3 percentage points. While the point estimate is not large in magnitude, the 95% confidence interval admits decreases in move rates of up to 1.8 percentage points and increases of up to 1.3 percentage points. This suggests that the vast majority of people do not move in the three months following study enrollment, but we cannot rule out treatment effects that are large relative to baseline move rates. While our pooled treatment effect estimates are more precise and closer to zero, we still cannot rule out sizable impacts of free fares on residential mobility.[21]

The residential address data also help address concerns about sample attrition for our

[20] See Appendix Table A1.

[21] See Appendix Table A4. We show event study estimates for residential moves in panel (b) of Figure A1.

other outcomes. The data on employment, public benefit use, 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.

## 7 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 at least in part from study participants' relatively low overall attachment to the labor force. Indeed, based on UI records, only one-third of participants were employed in the quarter prior to study enrollment. 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.[22]

In the data, we do not detect significant heterogeneity in effects for employment-related outcomes, but we do find some evidence of heterogeneity in impacts on healthcare use. 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

[22]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.

work an average of 118 hours in paid employment in the quarter after random assignment. Those in the treatment group work 12 more hours on average, with a 95% confidence interval spanning -18 to 42 hours, or -3.5% to 8% of full-time work. 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 effect for people not employed at baseline.

The lack of heterogeneity in effects on 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.[23]

On the other hand, we do detect some evidence of heterogeneity in effects on healthcare use. The fifth panel of Table 5 displays effects on having 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 less pronounced, we also find some indication of heterogeneity in effects on arrests, with stronger negative treatment effects among women and non-White participants.

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.[24] On the other hand, their method identifies some heterogeneity in effects for healthcare outcomes, pointing to potentially stronger im-

[23]For the full set of outcomes related to employment, public benefit receipt, and arrests, see Appendix Table A6. We also find limited evidence of any heterogeneity in impacts for financial outcomes from the credit reporting data; see Appendix Table A7.

[24]See Appendix Table A8. We provide more details on the methodology in the notes to the table.

pacts of free transit for those with a recent history of medical visits. However, an omnibus F-test of heterogeneity cannot reject the null of no heterogeneity in the causal forest for healthcare use.

# 8 Diffuse benefits of transit cost reductions

Our results suggest that, while not affecting employment, free transit improves 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 latter explanation unlikely for two 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.[25] Second, 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 areas, 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.

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 that point to improved financial situations of those in the treatment group. However, improved well-being does not necessarily extend to all areas of life. Based on the surveys, subjective

[25] 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.

well-being in the areas of education and housing do not increase; the 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 6th out of 10. 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.[26] 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.

Together with the seeming lack of strong impacts of the treatment on employment outcomes, even for those with stronger labor force attachment, these results suggest that existing models of urban location fail to capture much of the benefits of transit for people with low income. Typical models allow for commuting to work but assume that amenities are attached to a particular location. Individuals only access those non-work amenities by purchasing housing in that location. While these tractable quantitative models have many advantages,

[26] The proportion of trips for work is by 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.

our results suggest that they ignore the primary means by which transit matters for our population of interest. In our study, people with low income mostly use transit to travel to non-work services and amenities. As a result, they see improvements in their health and welfare even without observed changes in employment or residential location.

# 9 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, rejecting increases in paid hours worked among those with access to free transit of more than 4% of full-time employment. However, transit appears to have significant benefits outside the confines of the formal labor market for low-income individuals. People receiving free transit appear to be healthier; they are 16% less likely to visit a doctor or hospital. Data from credit reports also suggest improvements in their financial situation, and criminal justice records indicate a reduction in their likelihood of being arrested. Follow-up surveys of study participants corroborate the results from the administrative data in pointing to wide-ranging impacts of free transit fares on the travel habits as well as the well-being of individuals with low incomes.

It is possible that the results from this study might not generalize to a broader population of low-income individuals, 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 treatment effects for employment and most other outcomes do not differ substantially by prior labor force attachment or across other sub-groups.

Our results suggest that fare-free transit generates important welfare benefits that would be missed by existing economic models. Typical models 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 study participants use free transit for a wide variety of activities, not just paid work. As a result, the additional travel generates 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.

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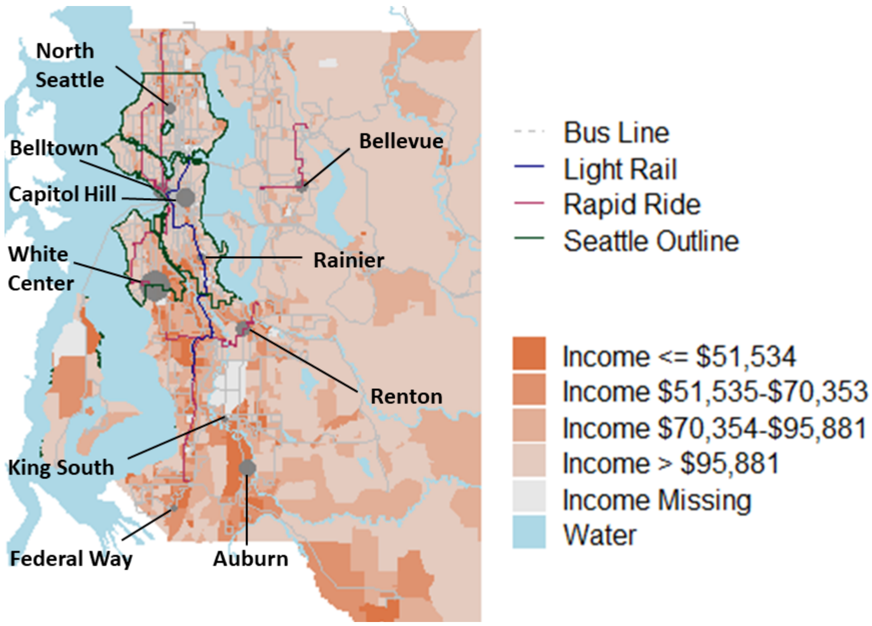
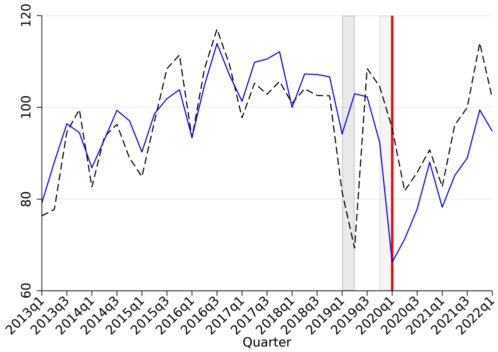
# Figures

Figure 1. Western King County, Washington

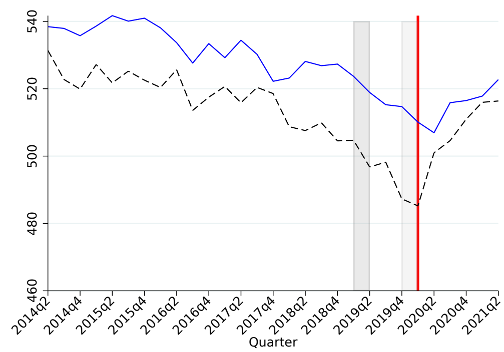



*Notes:* This a map of the western portion of King County, Washington, which is the location of Seattle. Census tracts are shaded by income quartile using data from the 2014-2018 American Community Survey. The extent of the transit network is shown as of 2019. The ten King County DSHS Community Service Offices (CSOs) where enrollment occurred are marked by gray dots. The sizes of the dots correspond to the proportion of the sample who enrolled from each CSO office.

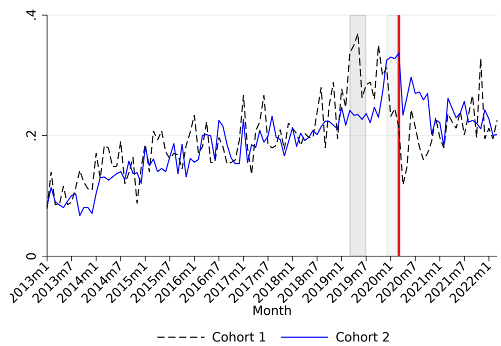
Figure 2. Mean Outcomes, by Calendar Time and Cohort




(a) Paid Hours Worked



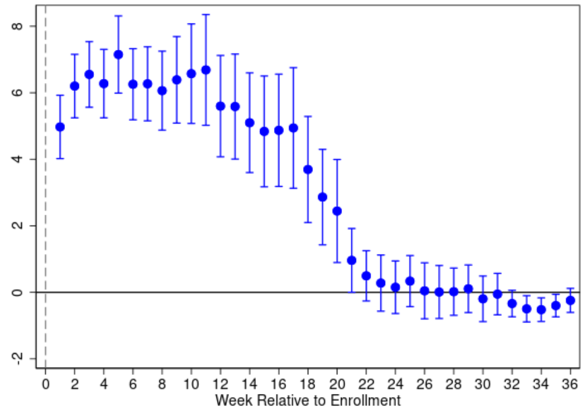

(b) Credit Scores




(c) Medical Visits

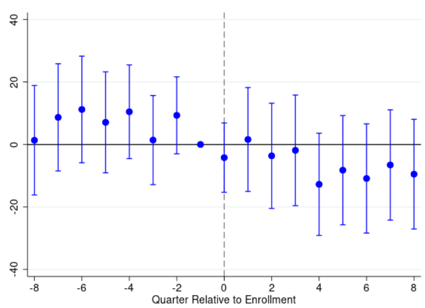
*Notes:* These figures display trends in mean (a) paid hours worked, (b) credit scores, and (c) Medicaid-covered doctor, clinic, or hospital visits by cohort. Paid hours worked and credit scores are measured at a quarterly frequency, while Medicaid visits are measured at a monthly frequency. Means for 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

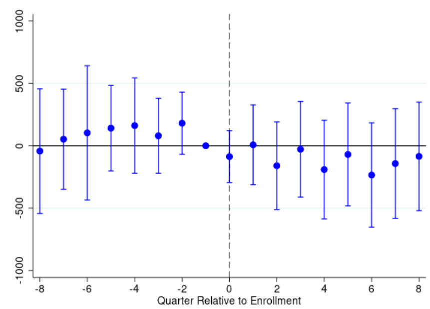



*Notes:* This figure depicts treatment effects on transit card use over time. Each dot measures the treatment effect of receiving free public transit at the relative week indicated on the horizontal axis. Each treatment effect is measured as regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcome is the number of transit boardings paid for with an ORCA card. 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.

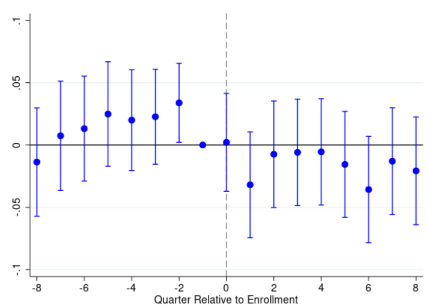
Figure 4. Treatment Effects on Employment Outcomes, by Relative Time




(a) Quarterly Paid Hours Worked



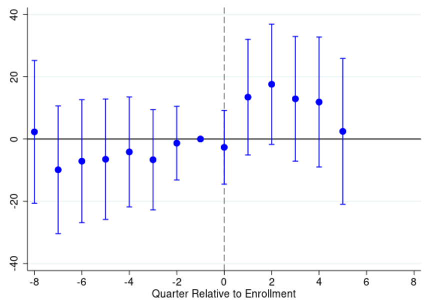

(b) Quarterly Earnings



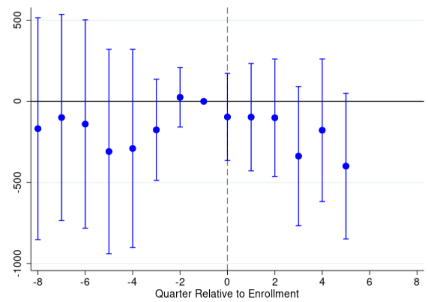

(c) Any Paid Employment

*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 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.

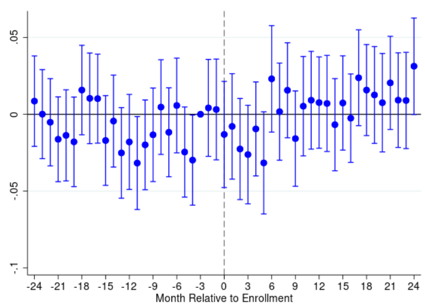
Figure 5. Treatment Effects on Financial and Health Outcomes, by Relative Time



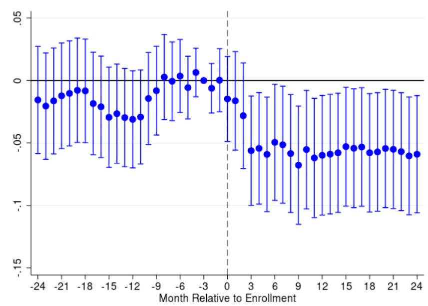

(a) Credit Score




(b) Balance in Collections



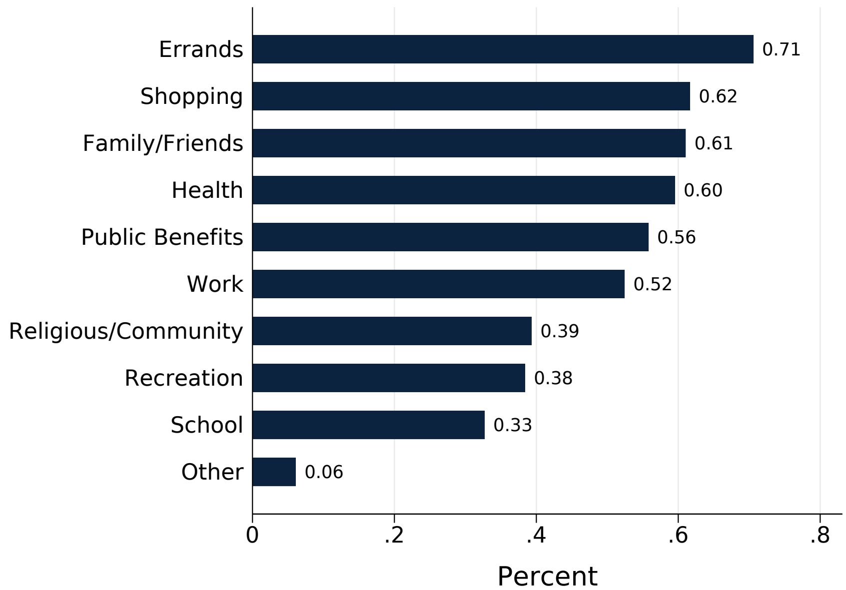

(c) Medical Visits, Flow




(d) Medical Visits, Cumulative

*Notes:* This figure depicts treatment effects on (a) credit scores, (b) balance in collections, (c) medical visits measured each month, 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) and (d)). 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 while (c) and (d) come 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 | 0.423 | 977 | 0.407 | 621 | -0.016 |
| | | | | | (0.025) |
| Hispanic | 0.088 | 977 | 0.076 | 621 | -0.013 |
| | | | | | (0.014) |
| Black | 0.279 | 977 | 0.288 | 621 | 0.010 |
| | | | | | (0.023) |
| Female | 0.407 | 977 | 0.391 | 621 | -0.017 |
| | | | | | (0.025) |
| Years of education | 11.945 | 849 | 12.103 | 552 | 0.168 |
| | | | | | (0.111) |
| Owns Vehicle | 0.195 | 977 | 0.169 | 621 | -0.024 |
| | | | | | (0.020) |
| *Outcomes Measured at $\tau = -1$* | | | | | |
| State Administrative Records | | | | | |
| Paid hours worked | 99.404 | 977 | 108.615 | 621 | 8.916 |
| | | | | | (9.069) |
| Total earnings | 1,955.365 | 977 | 2,110.329 | 621 | 45.528 |
| | | | | | (190.243) |
| Any formal employment | 0.331 | 977 | 0.361 | 621 | 0.014 |
| | | | | | (0.024) |
| Any food or cash benefits | 0.599 | 977 | 0.588 | 621 | -0.008 |
| | | | | | (0.025) |
| Any arrest, cumulative | 0.124 | 977 | 0.098 | 621 | -0.023 |
| | | | | | (0.016) |
| Any misdemeanor, cumulative | 0.015 | 977 | 0.011 | 621 | -0.003 |
| | | | | | (0.006) |
| Any gross misdemeanor, cumulative | 0.044 | 977 | 0.034 | 621 | -0.010 |
| | | | | | (0.010) |
| Any felony, cumulative | 0.041 | 977 | 0.034 | 621 | -0.006 |
| | | | | | (0.010) |
| Cost to Medicaid, cumulative | 612.962 | 977 | 806.055 | 621 | 162.471 |
| | | | | | (132.024) |
| Any Medicaid visit, cumulative | 0.243 | 977 | 0.245 | 621 | -0.001 |
| | | | | | (0.022) |
| State Administrative Records | | | | | |
| Credit score | 515.691 | 473 | 508.610 | 323 | -8.389 |
| | | | | | (13.150) |
| Balance in collection | 1,929.994 | 473 | 1,557.749 | 323 | -310.858 |
| | | | | | (331.588) |
| Infutor Data | | | | | |
| Any move | 0.012 | 432 | 0.014 | 290 | 0.000 |
| | | | | | (0.009) |

*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 emrollment; educational attainment data is incomplete for individuals matching to state administrative records, and so is only reported for 1,401 individuals. Paid hours worked, earnings, and any formal 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 in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 2. Employment Outcomes, One Quarter After Study Enrollment

| | (1) Control | (2) Treatment | (3) Simple Reg. Adj. Diff | (4) Reg. Adj. Diff. |
|---|---|---|---|---|
| Paid hours worked in $\tau = +1$ | 76.827 | 81.520 | 5.649 | 1.608 |
| | | | (8.949) | (8.489) |
| Earnings in $\tau = +1$ | 1,459.380 | 1,476.942 | 48.370 | 7.527 |
| | | | (170.232) | (163.043) |
| Any paid employment in $\tau = +1$ | 0.322 | 0.295 | -0.023 | -0.032 |
| | | | (0.024) | (0.022) |
| Job gain | 0.132 | 0.106 | -0.027 | -0.029* |
| (unemployed in $\tau = -1$, employed in $\tau = +1$) | | | (0.017) | (0.017) |
| Job loss | 0.138 | 0.151 | 0.010 | 0.009 |
| (employed in $\tau = -1$, unemployed in $\tau = +1$) | | | (0.018) | (0.018) |
| Continuous employment | 0.190 | 0.188 | 0.004 | 0.001 |
| (employed in $\tau = -1$, employed in $\tau = +1$) | | | (0.020) | (0.020) |
| –Continuous sector employment | 0.133 | 0.134 | 0.003 | 0.004 |
| | | | (0.018) | (0.018) |
| –Continuous industry employment | 0.107 | 0.108 | 0.004 | 0.006 |
| | | | (0.016) | (0.016) |
| Continuous unemployment | 0.539 | 0.554 | 0.013 | 0.019 |
| (unemployed in $\tau = -1$, unemployed in $\tau = +1$) | | | (0.026) | (0.026) |
| 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 unemployment insurance 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 in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 3. Employment Outcomes, Panel Regressions

| | (1) Paid Hours Worked | (2) Earnings | (3) Any Paid Employment |
|---|---|---|---|
| Treated | -0.461 | -48.119 | 0.001 |
| | (7.468) | (147.724) | (0.022) |
| Person Fixed Effects | ✓ | ✓ | ✓ |
| Calendar Quarter Fixed Effects | ✓ | ✓ | ✓ |
| Relative Quarter Fixed Effects | ✓ | ✓ | ✓ |
| Control Mean | 96.298 | 1,822.148 | 0.314 |
| 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 an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable is defined as the fraction of the quarter for which the individual is after study enrollment, multiplied by the fraction of the quarter for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 8 quarters prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 4. Secondary Outcomes, One Quarter After Enrollment

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Control | | Treatment | | Simp Reg | Reg. |
| | Mean | N | Mean | N | Adj. Diff | Adj. Diff |
| *A. Public Assistance Receipt, measured three months post enrollment* | | | | | | |
| Any food or cash benefits | 0.932 | 977 | 0.915 | 621 | -0.018 | -0.016 |
| | | | | | (0.014) | (0.013) |
| –SNAP | 0.912 | 977 | 0.889 | 621 | -0.024 | -0.022 |
| | | | | | (0.016) | (0.015) |
| –TANF | 0.024 | 977 | 0.032 | 621 | 0.007 | 0.003 |
| | | | | | (0.009) | (0.008) |
| –Other | 0.132 | 977 | 0.111 | 621 | -0.020 | -0.014 |
| | | | | | (0.017) | (0.015) |
| *B. Financial Health, measured in the third month of the quarter post enrollment* | | | | | | |
| Balance in Collection | 1,621.746 | 492 | 1,363.698 | 334 | -220.409 | -96.984 |
| | | | | | (219.839) | (168.849) |
| Credit Score | 500.813 | 492 | 513.991 | 334 | 9.214 | 13.448 |
| | | | | | (13.658) | (9.471) |
| Total Inquiries in Past 3 Months | 0.343 | 492 | 0.257 | 334 | -0.098** | -0.082** |
| | | | | | (0.039) | (0.041) |
| *C. Criminal Justice, measured three months post enrollment* | | | | | | |
| Any arrest, cumulative | 0.136 | 977 | 0.111 | 621 | -0.022 | -0.015 |
| | | | | | (0.017) | (0.016) |
| Any misdemeanor, cumulative | 0.015 | 977 | 0.013 | 621 | -0.002 | -0.001 |
| | | | | | (0.006) | (0.006) |
| Any gross misdemeanor, cumulative | 0.050 | 977 | 0.043 | 621 | -0.007 | -0.006 |
| | | | | | (0.011) | (0.011) |
| Any felony, cumulative | 0.056 | 977 | 0.050 | 621 | -0.003 | -0.002 |
| | | | | | (0.011) | (0.011) |
| *D. Healthcare, measured three months post enrollment* | | | | | | |
| Cost to Medicaid, cumulative | 975.211 | 977 | 912.660 | 621 | -43.029 | -77.361 |
| | | | | | (176.374) | (167.001) |
| Any Medicaid Visit, cumulative | 0.347 | 977 | 0.282 | 621 | -0.062*** | -0.056** |
| | | | | | (0.024) | (0.022) |
| –Emergency outpatient | 0.246 | 977 | 0.208 | 621 | -0.034 | -0.032 |
| | | | | | (0.021) | (0.021) |
| –Emergency inpatient | 0.044 | 977 | 0.035 | 621 | -0.008 | -0.008 |
| | | | | | (0.010) | (0.010) |
| –Non-emergency outpatient | 0.298 | 977 | 0.237 | 621 | -0.059*** | -0.050** |
| | | | | | (0.023) | (0.022) |
| –Non-emergency inpatient | 0.024 | 977 | 0.021 | 621 | -0.001 | 0.000 |
| | | | | | (0.007) | (0.007) |
| *E. Residential Mobility, measured three months post enrollment* | | | | | | |
| Any Move | 0.012 | 432 | 0.010 | 290 | -0.003 | -0.003 |
| | | | | | (0.008) | (0.008) |
| Any Move in State | 0.007 | 432 | 0.010 | 290 | 0.003 | 0.002 |
| | | | | | (0.006) | (0.007) |
| Any Move out of State | 0.005 | 432 | 0.003 | 290 | -0.003 | -0.002 |
| | | | | | (0.005) | (0.005) |
| Any Move in County | 0.005 | 432 | 0.010 | 290 | 0.005 | 0.004 |
| | | | | | (0.006) | (0.007) |
| Any Move out of County | 0.007 | 432 | 0.003 | 290 | -0.005 | -0.004 |
| | | | | | (0.005) | (0.005) |

*Notes:* This table presents means and regression-adjusted differences in means for outcomes measured in the quarter after enrollment. Healthcare use, cash and food benefits, and arrests come from Washington state administrative records from Medicaid claims, the state Economic Services Administration, and the Washington State Patrol, respectively, with the sample limited to people who match to any of these records prior to random assignment. Healthcare and arrests are measured cumulatively between random assignment and 3 months later; cash/food benefits at 3 months. Cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Financial measures cover the sample that matches to a repeated cross-section of quarterly Experian credit reports with outcomes measured 1 quarter after random assignment. Well-being measures come from a survey offered to a sub-sample of study participants; see text for details. 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 mean between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Column 6 additionally adjusts for indicators for race and month of study enrollment; outcomes from state records also include controls for gender; panels 2,4, and 6 control for age and age squared; all outcomes except well-being measures also control for the relevant outcome 1 quarter prior to study enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

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) |
| *Hours Worked* | | | | | | | | | | | | |
| Control Mean | 42.51 | 118.19 | 53.70 | 152.36 | 80.94 | 70.85 | 70.55 | 102.68 | 51.18 | 95.61 | 124.26 | 57.49 |
| Reg Adj. Diff. | -4.47 | 11.67 | -1.26 | 9.16 | -2.86 | 18.02 | 5.56 | 10.71 | 2.70 | 6.58 | 21.83 | -0.52 |
| SE | (9.08) | (15.29) | (8.35) | (23.05) | (11.25) | (14.68) | (9.18) | (27.30) | (9.85) | (13.38) | (22.54) | (8.26) |
| P-Value of Diff. | [0.364] | | [0.670] | | [0.259] | | [0.858] | | [0.816] | | [0.351] | |
| *Employed* | | | | | | | | | | | | |
| 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. | [0.828] | | [0.812] | | [0.471] | | [0.369] | | [0.484] | | [0.372] | |
| *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. | [0.972] | | [0.501] | | [0.997] | | [0.872] | | [0.089] | | [0.491] | |
| *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. | [0.622] | | [0.695] | | [0.682] | | [0.246] | | [0.206] | | [0.283] | |
| *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. | [0.686] | | [0.032] | | [0.500] | | [0.702] | | [0.004] | | [0.458] | |
| 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 heterogeneous treatment effects on employment. Each outcome is measured 1 quarter post enrollment. Eligible for medicaid is defined as ever being eligible in the 4 quarters prior to enrollment; 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. The coefficient reported in row "Red Adj. Diff" regresses the outcome of interest on a treatment indicator and randomization regime. The robust standard error of this regression is reported in the row below. The p-value of the difference between columns 1 and 2; 3 and 4; 5 and 6; 7 and 8; and 9 and 10 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 row "p-value of diff". Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively..

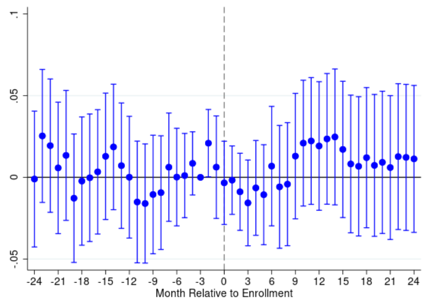
Table 6. Follow-Up Survey Results

| | (1) Control Mean | (2) Control N | (3) Treatment Mean | (4) Treatment N | (5) Simp Reg Adj. Diff | (6) Reg. Adj. Diff |
|---|---|---|---|---|---|---|
| *Well-Being Measures* | | | | | | |
| Transportation well-being | 3.016 | 125 | 3.214 | 124 | 0.207* | 0.249** |
| | | | | | (0.137) | (0.139) |
| Employment well-being | 2.516 | 126 | 2.710 | 124 | 0.203* | 0.238* |
| | | | | | (0.157) | (0.160) |
| Financial well-being | 2.437 | 126 | 2.681 | 124 | 0.246* | 0.258* |
| | | | | | (0.160) | (0.168) |
| Health well-being | 2.980 | 125 | 3.053 | 123 | 0.068 | 0.125 |
| | | | | | (0.126) | (0.123) |
| Housing well-being | 2.972 | 125 | 2.988 | 125 | 0.019 | -0.005 |
| | | | | | (0.136) | (0.138) |
| Education well-being | 3.357 | 122 | 3.319 | 124 | -0.034 | -0.041 |
| | | | | | (0.122) | (0.119) |
| *Share of Public Transit Trips, by Purpose* | | | | | | |
| Share of Transit Trips for Work | 0.225 | 44 | 0.420 | 53 | 0.203** | 0.215** |
| | | | | | (0.112) | (0.109) |
| Share of Transit Trips for Health | 0.081 | 44 | 0.104 | 53 | 0.017 | 0.021 |
| | | | | | (0.058) | (0.066) |
| Share of Transit Trips for Public Benefits | 0.080 | 44 | 0.050 | 53 | -0.029 | -0.043 |
| | | | | | (0.064) | (0.066) |
| Share of Transit Trips for Shopping | 0.311 | 44 | 0.462 | 53 | 0.147 | 0.164* |
| | | | | | (0.130) | (0.125) |
| Share of Transit Trips for Errands | 0.356 | 44 | 0.154 | 53 | -0.212** | -0.264** |
| | | | | | (0.118) | (0.127) |
| Share of Transit Trips for Family/Friends | 0.208 | 44 | 0.119 | 53 | -0.092 | -0.070 |
| | | | | | (0.093) | (0.107) |
| Share of Transit Trips for Recreation | 0.167 | 44 | 0.149 | 53 | -0.008 | 0.013 |
| | | | | | (0.085) | (0.083) |
| Share of Transit Trips for Religious/Community | 0.000 | 44 | 0.019 | 53 | 0.020 | 0.020 |
| | | | | | (0.020) | (0.021) |
| Share of Transit Trips for School | 0.045 | 44 | 0.009 | 53 | -0.034 | -0.050 |
| | | | | | (0.045) | (0.057) |
| Share of Transit Trips for Other Purpose | 0.078 | 44 | 0.025 | 53 | -0.052 | -0.045 |
| | | | | | (0.041) | (0.044) |

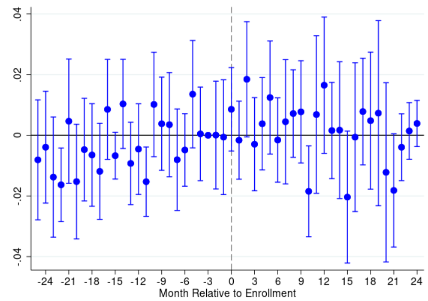
*Notes:* This table describes outcomes from self-reported surveys conducted by phone and by web in the year post study enrollment. The survey began in March 2020 and continued through December 2020; however, this table only reports results from surveys during which the treatment is effective (Prior to March 18, 2020 and after October 1, 2020). Details of the survey are described in Section 4. Panel A reports well-being measures where participants are asked to describe how their well-being in certain areas has changed in the past 2 months, which responses placed on a on 1-5 Likert scale (1 being "much worse" and 5 being "much better"). Panel B is share of public transit trips for each trip purpose. 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

# Appendix Figures and Tables

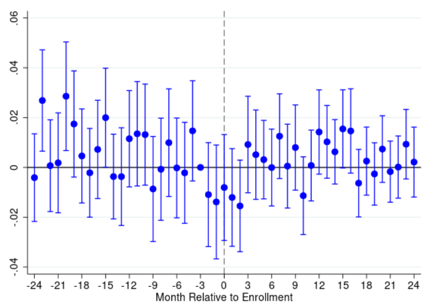
Figure A1. Treatment Effects on Secondary Outcomes, by Relative Time



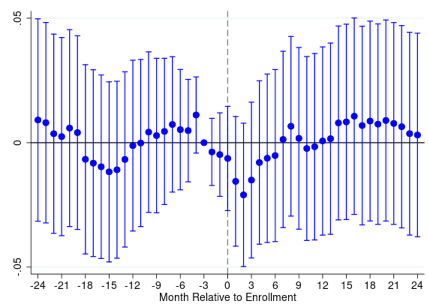

(a) Any food or cash benefits




(b) Any move




(c) Any Arrest, Flow




(d) Any Arrest, Cumulative

*Notes:* This figure depicts treatment effects on (a) credit scores, (b) balance in collections, (c) medical visits measured each month, 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) and (d)). 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 while (c) and (d) come 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.

Table A1. 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 |
| Treat | -18.469 | -0.014 | -0.003 | -0.001 | -0.000 | -0.012 | -0.006 | 0.001 | -0.001 | -0.004 | -0.014** |
| | (41.314) | ( 0.010) | ( 0.008) | ( 0.003) | ( 0.002) | ( 0.010) | ( 0.018) | ( 0.018) | ( 0.006) | ( 0.011) | ( 0.006) |
| Person FE | × | × | × | × | × | × | × | × | × | × | × |
| Calendar Month FE | × | × | × | × | × | × | × | × | × | × | × |
| Relative Month FE | × | × | × | × | × | × | × | × | × | × | × |
| Control Mean | 141.742 | 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 an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable is defined as the fraction of the month for which the individual is after study enrollment, multiplied by the fraction of the month for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 24 months prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

# Table A2. Financial Health Outcomes, Panel Regressions

| | (1)<br>Balance in Collections | (2)<br>Credit Score | (3)<br>Credit Inquiries in Past 3 Months |
|---|---|---|---|
| Treat | 165.978 | -0.974 | -0.022 |
| | (186.794) | ( 6.365) | ( 0.034) |
| Person FE | × | × | × |
| Calendar Qtr FE | × | × | × |
| Relative Qtr FE | × | × | × |
| Control Mean | 1,838.708 | 515.945 | 0.326 |
| Observations | 11061 | 11061 | 11061 |
| 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 an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable is defined as the fraction of the quarter for which the individual is after study enrollment, multiplied by the fraction of the quarter for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 8 quarters prior to and 5 quarters post study enrollment for all sample individuals. The sample here is limited to individuals matching to any credit report prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A3. Secondary Outcomes, Criminal Justice Involvement

| | (1) Control Mean | (2) Control N | (3) Treatment Mean | (4) Treatment N | (5) Simple Reg. Adj. Diff | (6) Reg. Adj. Diff |
|---|---|---|---|---|---|---|
| Any arrest | 0.136 | 977 | 0.111 | 621 | -0.022 | -0.015 |
| | | | | | ( 0.017) | ( 0.016) |
| *Crime Category* | | | | | | |
| –Felony | 0.056 | 977 | 0.050 | 621 | -0.003 | -0.002 |
| | | | | | ( 0.011) | ( 0.011) |
| –Misdemeanor | 0.015 | 977 | 0.013 | 621 | -0.002 | -0.001 |
| | | | | | ( 0.006) | ( 0.006) |
| –Gross misdemeanor | 0.050 | 977 | 0.043 | 621 | -0.007 | -0.006 |
| | | | | | ( 0.011) | ( 0.011) |
| –Unknown | 0.078 | 977 | 0.066 | 621 | -0.010 | -0.004 |
| | | | | | ( 0.013) | ( 0.013) |
| *Crime Type* | | | | | | |
| –Assault, cumulative | 0.024 | 977 | 0.027 | 621 | 0.002 | 0.003 |
| | | | | | ( 0.008) | ( 0.008) |
| –Theft, cumulative | 0.049 | 977 | 0.043 | 621 | -0.005 | -0.004 |
| | | | | | ( 0.011) | ( 0.011) |
| –Sex, cumulative | 0.002 | 977 | 0.005 | 621 | 0.003 | 0.003 |
| | | | | | ( 0.003) | ( 0.003) |
| –Domestic violence, cumulative | 0.011 | 977 | 0.011 | 621 | -0.000 | 0.001 |
| | | | | | ( 0.006) | ( 0.005) |
| –Custody, cumulative | 0.025 | 977 | 0.021 | 621 | -0.001 | 0.001 |
| | | | | | ( 0.007) | ( 0.007) |
| –Alcohol/drug, cumulative | 0.018 | 977 | 0.021 | 621 | 0.003 | 0.006 |
| | | | | | ( 0.007) | ( 0.007) |
| –Trespass, cumulative | 0.024 | 977 | 0.011 | 621 | -0.011* | -0.009 |
| | | | | | ( 0.006) | ( 0.006) |
| –Reckless driving, cumulative | 0.001 | 977 | 0.000 | 621 | -0.001 | -0.001 |
| | | | | | ( 0.001) | ( 0.001) |
| –Vehicle license, cumulative | 0.004 | 977 | 0.003 | 621 | -0.000 | -0.000 |
| | | | | | ( 0.003) | ( 0.003) |
| –Weapons, cumulative | 0.004 | 977 | 0.005 | 621 | 0.001 | -0.001 |
| | | | | | ( 0.004) | ( 0.003) |
| –Probation, cumulative | 0.017 | 977 | 0.010 | 621 | -0.008 | -0.007 |
| | | | | | ( 0.006) | ( 0.006) |
| –Murder, cumulative | 0.000 | 977 | 0.000 | 621 | 0.000*** | 0.000*** |
| | | | | | ( 0.000) | ( 0.000) |
| –Fail to comply, cumulative | 0.046 | 977 | 0.035 | 621 | -0.009 | -0.007 |
| | | | | | ( 0.010) | ( 0.010) |
| –Other, cumulative | 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 quarter after enrollment. Arrests are measured cumulatively between random assignment and 3 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 in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

## Table A4. Residential Mobility Outcomes, Panel Regressions

| | (1) Any Move | (2) Any Move In WA | (3) Any Move Outside WA | (4) Any Move In King County | (5) Any Move Outside King County |
|---|---|---|---|---|---|
| Treat | 0.006 | 0.000 | 0.006* | 0.001 | 0.006 |
| | ( 0.005) | ( 0.004) | ( 0.004) | ( 0.003) | ( 0.004) |
| Person FE | × | × | × | × | × |
| Calendar Qtr FE | × | × | × | × | × |
| Relative Qtr FE | × | × | × | × | × |
| Control Mean | 0.014 | 0.011 | 0.003 | 0.009 | 0.006 |
| Observations | 34790 | 34790 | 34790 | 34790 | 34790 |
| 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 an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable is defined as the fraction of the month for which the individual is after study enrollment, multiplied by the fraction of the month for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 24 months prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to Infutor consumer reference data prior to random assignment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

# Table A5. Employment Outcomes, Heterogeneity

| | Employed Pre 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) |
| *Hours worked in relative qtr 1* | | | | | | | | | | | | |
| Control Mean | 42.51 | 118.19 | 53.70 | 152.36 | 80.94 | 70.85 | 70.55 | 102.68 | 51.18 | 95.61 | 124.26 | 57.49 |
| Reg Adj. Diff | -4.47 | 11.67 | -1.26 | 9.16 | -2.86 | 18.02 | 5.56 | 10.71 | 2.70 | 6.58 | 21.83 | -0.52 |
| SE | (9.08) | (15.29) | (8.35) | (23.05) | (11.25) | (14.68) | (9.18) | (27.30) | (9.85) | (13.38) | (22.54) | (8.26) |
| P-Value of Diff. | [0.364] | | [0.670] | | [0.259] | | [0.858] | | [0.816] | | [0.351] | |
| *Earnings in relative qtr 1* | | | | | | | | | | | | |
| Control Mean | 765.22 | 2296.13 | 945.89 | 3136.62 | 1564.68 | 1306.19 | 1293.82 | 2140.69 | 971.76 | 1816.45 | 2522.12 | 1026.01 |
| Reg Adj. Diff | -101.48 | 112.45 | -63.61 | 13.49 | -159.56 | 353.21 | 68.58 | 70.88 | -54.17 | 96.94 | 309.79 | -47.82 |
| SE | (160.25) | (298.96) | (142.26) | (474.13) | (204.65) | (294.99) | (165.54) | (579.41) | (180.27) | (256.77) | (450.69) | (145.51) |
| P-Value of Diff. | [0.528] | | [0.876] | | [0.153] | | [0.997] | | [0.630] | | [0.450] | |
| *Employed in relative qtr 1* | | | | | | | | | | | | |
| 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. | [0.828] | | [0.812] | | [0.471] | | [0.369] | | [0.484] | | [0.372] | |
| *Cont. Employment between relative quater -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. | [0.611] | | [0.348] | | [0.183] | | [0.353] | | [0.355] | | [0.591] | |
| *–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. | [0.592] | | [0.120] | | [0.123] | | [0.220] | | [0.444] | | [0.634] | |
| *–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. | [0.827] | | [0.107] | | [0.293] | | [0.096] | | [0.999] | | [0.484] | |
| *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. | [0.383] | | [0.237] | | [0.517] | | [0.927] | | [0.038] | | [0.081] | |
| *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. | [0.994] | | [0.167] | | [0.982] | | [0.275] | | [0.567] | | [0.897] | |
| *Cont. Unemployment between relative quater -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. | [0.798] | | [0.133] | | [0.492] | | [0.988] | | [0.827] | | [0.436] | |
| 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 heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Eligible for medicaid is defined as ever being eligible in the 4 quarters prior to enrollment; 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. The coefficient reported in the row "Reg Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. The robust standard error on this coefficient is reported in the row below. The p-value of the difference in treatment effects between columns 1 and 2; 3 and 4; 5 and 6; 7 and 8; and 9 and 10 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". Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

## Table A6. Benefits, Health, Criminal Justice Outcomes, Heterogeneity

| | Employed Pre 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 food or cash 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. | [0.972] | | [0.501] | | [0.997] | | [0.872] | | [0.089] | | [0.491] | |
| *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. | [0.844] | | [0.485] | | [0.517] | | [0.593] | | [0.151] | | [0.454] | |
| *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. | [0.312] | | [0.442] | | [0.404] | | [0.306] | | [0.121] | | [0.524] | |
| *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. | [0.467] | | [0.161] | | [0.586] | | [0.807] | | [0.212] | | [0.678] | |
| *Cost to Medicaid, cumulative* | | | | | | | | | | | | |
| Control Mean | 982.32 | 966.65 | 994.61 | 911.83 | 916.96 | 1059.96 | 973.60 | 981.84 | 1216.11 | 798.81 | 459.66 | 1185.44 |
| Reg Adj. Diff | 266.06 | -373.13 | 76.82 | -351.52 | 147.08 | -332.30 | -64.86 | 67.54 | -83.57 | -2.93 | -193.66 | 13.18 |
| SE | (256.89) | (239.43) | (199.98) | (364.71) | (256.30) | (214.02) | (178.45) | (569.24) | (346.31) | (178.35) | (141.88) | (238.90) |
| P-Value of Diff. | [0.069] | | [0.303] | | [0.151] | | [0.824] | | [0.836] | | [0.457] | |
| *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. | [0.686] | | [0.032] | | [0.500] | | [0.702] | | [0.004] | | [0.458] | |
| *–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. | [0.237] | | [0.047] | | [0.270] | | [0.669] | | [0.068] | | [0.499] | |
| *–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. | [0.022] | | [0.065] | | [0.932] | | [0.997] | | [0.596] | | [0.165] | |
| *–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. | [0.640] | | [0.711] | | [0.280] | | [0.909] | | [0.804] | | [0.504] | |
| *–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. | [0.614] | | [0.030] | | [0.455] | | [0.831] | | [0.003] | | [0.275] | |
| *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. | [0.622] | | [0.695] | | [0.682] | | [0.246] | | [0.206] | | [0.283] | |
| 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 heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Eligible for medicaid is defined as ever being eligible in the 4 quarters prior to enrollment; 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. The coefficient reported in the row "Reg Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. The robust standard error on this coefficient is reported in the row below. The p-value of the difference in treatment effects between columns 1 and 2; 3 and 4; 5 and 6; 7 and 8; and 9 and 10 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". Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A7 Financial Health, Heterogeneity

| | 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 | 260.89 | 2686.76 | 1275.48 | 2067.87 | 1801.24 | 1493.53 |
| Reg Adj. Diff | 23.01 | -287.78 | -343.80 | -139.12 | -567.22* | 6.37 |
| SE | 87.47 | 363.95 | 238.50 | 382.43 | 328.00 | 295.46 |
| P-Value of Diff. | [0.407] | | [0.650] | | [0.194] | |
| *Credit Score* | | | | | | |
| Control Mean | 511.79 | 492.22 | 552.59 | 434.11 | 486.35 | 511.14 |
| Reg Adj. Diff | -2.23 | 16.10 | 16.35 | 11.57 | 12.15 | 5.09 |
| SE | 23.93 | 15.24 | 15.82 | 21.37 | 20.97 | 18.01 |
| P-Value of Diff. | [0.518] | | [0.857] | | [0.798] | |
| *Total Inquiries in Past 3 Mos* | | | | | | |
| Control Mean | 0.29 | 0.38 | 0.32 | 0.37 | 0.45 | 0.26 |
| Reg Adj. Diff | -0.06 | -0.12** | -0.11** | -0.09 | -0.16** | -0.05 |
| SE | 0.05 | 0.05 | 0.05 | 0.06 | 0.07 | 0.04 |
| P-Value of Diff. | [0.492] | | [0.749] | | [0.188] | |
| N - Control | 216 | 276 | 277 | 215 | 205 | 287 |
| N - Treatment | 158 | 176 | 175 | 159 | 126 | 208 |

*Notes:* This table reports 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. he coefficient reported in the row "Reg Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. The robust standard error on this coefficient is reported in the row below. The p-value of the difference in treatment effects between columns 1 and 2; 3 and 4; and 5 and 6 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 row "P-Value of Diff". Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A8. Athey and Imbens (2016) Heterogeneity Tests

| Outcome | Num. of Leaves | Leaf Categories (Y/N) | F-Stat | F-Stat P-Value |
|---|---|---|---|---|
| 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 | | | | |
| – 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 Health 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 | One or more 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 |
| 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 heterogeneous 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, column 3 reports the variable that was identified as having different treatment effects. Columns 4 and 5 report the F-value and p-value associated with the tests of whether the leaves are statistically different from each other. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.