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

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Abstract

We conduct a randomized controlled trial to study the employment effects of providing free public transportation to individuals with low income. A temporary subsidy that reduces the price of transit to zero has no significant effects on individuals' paid hours worked or earnings. Using rich administrative data, we also explore a range of other outcomes. We find suggestive evidence that transit subsidies improve measures of financial and physical health.

Keywords: public transportation, transit subsidies, randomized controlled trial
JEL: H4, H7, I3, R4, R5

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1 Introduction

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

This paper studies the effects of free public transit fares on employment and other outcomes 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 partially subsidized fares of $1.50 per bus ride. To measure the effects of fare-free public transit on employment outcomes, we link individuals in the experiment to payroll tax records. In supplementary analyses, we also leverage public assistance, criminal justice, and healthcare records as well as proprietary data on consumer credit and residential locations. We complement these data with our own surveys of study participants.

Our main analysis explores the effects of providing free public transit on employment outcomes. We do not detect large effects of the treatment on employment rates, paid hours worked, total earnings, or other employment-related outcomes that we can track in Washington State payroll tax records. For example, one quarter after study enrollment, individuals in the treatment group work for pay 1.6 more hours per quarter than those in the control group, on average. This gap is not statistically distinguishable from zero and is relatively small. At a statistical significance level of 5%, we can reject 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, we can reject increases in paid hours worked greater than 2% 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.

We also consider the impacts of free public transit on other, secondary outcomes. While we do not observe any effects of free transit access on receipt of food and cash benefits or on changing residences, we find some evidence that those in the treatment group use less medical care and experience improvements in their credit reports. For example, 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%. Although the presence of many outcomes could lead to false positives, the impacts on healthcare use and financial circumstances are statistically significant when we pool across different outcomes within each domain, suggesting that these effects are largely robust to accounting for multiple inference.

We might have expected meaningful effects of the transit subsidy on study participants' ability to find and maintain paid work for several reasons. First, prior work on the same experiment suggests that the subsidy had a large impact on travel, doubling transit use (Brough, Freedman and Phillips, 2022). Second, most study participants appear to have available margins for adjusting labor supply. A pre-enrollment Ashenfelter dip in paid hours worked implies that many individuals in our study arrive at public assistance offices having recently lost jobs, and study participants who are employed at baseline work, on average, about half of full-time hours and in industries with high turnover.

However, our results point to limited impacts of the treatment on paid work for individuals with low income, and while any trial's results are necessarily specific to a particular setting, some supporting evidence suggests that our results are not attributable to the particular context of this experiment. For example, in our setting, the null employment effects could be due to the temporary nature of the subsidy, which may have diminished the degree to which individuals adjusted their work behavior in response to the treatment. However, we find little evidence of stronger employment effects among those in the study who, for exogenous reasons, anticipated longer treatment durations. The muted employment effects could also be due to the composition of our study sample, but we cannot detect meaningful heterogeneity in the treatment's impacts on employment. In particular, we find no evidence of heterogeneity by baseline employment, which suggests that null employment effects do not simply follow from an over-representation of people detached from the labor force in our sample. Limited coverage or connectivity of transit may have also affected how much the subsidy expanded formal employment opportunities. However, we find that people with low income use free transit extensively and for a diffuse set of non-work activities that could positively effect their well-being. Finally, while the onset of the COVID-19 pandemic affected the context of this study, our results are robust to excluding periods directly impacted by the pandemic. Taken together, our results suggest that, more so than any context-specific explanations, transit fares represent a larger barrier to pursuing many non-work activities than they do for engaging in paid employment among individuals with low income.

This paper makes two main contributions. First, we extend the study of an increasingly popular policy, free fares on public transportation systems, to shed new light on its potential effects on employment and other downstream outcomes. Earlier work exploiting the same experiment found that providing free public transportation significantly increased public transit use; the effect on overall mobility (including modes other than transit) was potentially large but less clear (Brough, Freedman and Phillips, 2022). Studies on the effects of free transit fares in other contexts, including some RCTs, have also pointed to large effects on transit use as well as important implications for overall mobility (Volinski, 2012; Cools, Fabbro and Bellemans, 2016; Cats, Susilo and Reimal, 2017; Bull, Munoz and Silva, 2021; Busch-Geertsema, Lanzendorf and Klinner, 2021). We build on this literature by studying the effects of free public transit on outcomes beyond those related directly to mobility. Only a few RCTs study how free fares affect employment, and they focus on smaller subsidies targeted to increase job search intensity among unemployed individuals (Phillips, 2014; Franklin, 2018; Abebe et al., 2021).[1] Relative to this literature, we study a deeper subsidy covering several months, consider a much broader group of disadvantaged individuals, and measure a wider range of outcomes.

Second, our results suggest that the existing literature on transit over-emphasizes labor market benefits relative to other potential benefits. 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., 2024; Almagro and Domínguez-Iino, 2024). In that world, researchers can summarize the benefits of transit using only commute flows, wages, and rents (Severen, 2023; Tsivanidis, 2024). Similarly, a long-running empirical literature focuses on the role that differential access to jobs across neighborhoods plays in generating disparate labor market outcomes and persistent concentrations of poverty (Kain, 1968; Wilson, 1997; Holzer, Quigley and Raphael, 2003; Tyndall, 2021; Li and Wyczalkowski, 2023). Contrary to expectations from this literature, we find little evidence that transit subsidies have important labor market effects, but more evidence that such subsidies improve other aspects of individuals' lives. While we cannot extrapolate from fare subsidies to all transit investments, our results suggest that measuring the benefits of transit requires looking beyond just the labor market.

[1]Other work considering the effect of free or reduced transit fares on downstream outcomes include Rosenblum (2020), who studies their effects on healthcare use, and Brough et al. (2022), who study their effects on court appearances.

2 Context

We conducted the experiment in King County, Washington in partnership with King County Metro Transit Department (i.e., King County Metro) and Washington's Department of Social and Health Services (DSHS). 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, the Central Puget Sound Regional Transit Authority (i.e., Sound Transit), and other local transit agencies. Both maps in Figure 1 show the extent of the transit network at the time of our study. At that time, rail service largely consisted of one line running from the region's primary airport in south King County to the University of Washington north of downtown Seattle. Both rapid transit buses ("rapid ride") and regular local buses cover the remainder of the study area. In 2019, 15% of all workers in King County, and 10% of those with incomes below 150% of the federal poverty line, commuted by public transportation.[2]

With a median household income of $106,326, King County skews higher income than the U.S. as a whole at $68,703.[3] However, there is considerable heterogeneity in income levels and access to opportunity across neighborhoods in King County. Map A in Figure 1 uses data from Opportunity Insights (Chetty et al., 2018) to illustrate the heterogeneity in economic mobility across census tracts in western King County. Among children with parents earning $27,000 (the 25th percentile), average household income at age 35 for those growing up in the lower income neighborhoods south of downtown Seattle is less than half that of those from the more affluent neighborhoods north and east of downtown. As in other cities, there is also substantial mismatch between the residential and employment locations of individuals with low incomes in the Seattle area. Map B in Figure 1 uses data from the 2018 LEHD Origin-Destination Employment Statistics (LODES) to show the difference

[2] Authors' calculations based on the 2019 American Community Survey. As we describe in Section 7, individuals in our study are frequent users of transit at baseline.

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

between the number of low-wage jobs in a census block group and the number of low-wage residents there. A disproportionate share of low-income residents live south of Seattle, but many jobs held by these residents are in downtown Seattle.

In this context, fare-free transit could increase mobility, leading to greater access to employment and potentially other amenities and services. Fully subsidized transit should generate more transit use, due to the typical downward sloping demand curve and potentially also because of strong zero-price effects (Cools, Fabbro and Bellemans, 2016). Transit price reductions also have income effects, though as described in Section 7, we expect these effects to be small in our setting. To the extent that increased transit use generates new or different travel, rather than just displacing travel by other modes, it could affect labor market outcomes through several channels. In standard urban models (Fujita, 1991; Zenou, 2009; Ahlfeldt et al., 2015) and classic empirical studies (Zax and Kain, 1991), workers reject jobs when commute costs are too high. Travel costs can also limit the scope of individuals' job searches (Phillips, 2014; Franklin, 2018; Abebe et al., 2021; Banerjee and Sequeira, 2023). Additionally, commute costs can influence employers' choices of whom to hire (Phillips, 2020*a*; Diaz and Salas, 2020; Carlsson and Eriksson, 2023).

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

3 Free Transit Experiment

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

3.1 Recruitment and random assignment

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

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 $10-15 loaded on it.

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

The exact duration of the full subsidy varied across people and study cohorts. In the first study cohort, free transit access 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,

[4] Individuals who expressed interest in transit benefits but declined to participate in the experiment received an ORCA LIFT card (described in the next section). Two-thirds (67%) of those who expressed interest in transit benefits and were eligible to receive an ORCA LIFT card during our sample period enrolled in the study.

depending on when they visited the DSHS office and were issued their card. On average, the treatment group in the first cohort received 16.7 weeks of free transit. In the second cohort, treatment card passports were set to expire on June 30, 2020. The onset of the pandemic, though, prompted substantial changes to public transit services, including a suspension of fare collection for all riders, which rendered the treatment moot as of March 21, 2020.[5] As a result, while participants in the second cohort anticipated between 12 and 26 weeks (mean 18.1 weeks) of free transit, they ultimately received between 0 and 14 weeks (mean 6.1 weeks) of full subsidies prior to the onset of COVID-19.[6] Based on the average expected subsidy duration across the two cohorts of about four months, the implied subsidy size given the cost of monthly passes for low-income riders in King County was approximately $200. Four months of unlimited transit in many other major cities would cost substantially more given that the majority of transit systems do not offer discounted fare programs for all low-income residents; however, an increasing number of cities have adopted reduced-fare programs that offer subsidies on par with those of ORCA LIFT.[7]

4 Data and descriptive statistics

4.1 Participant surveys and transit use

During enrollment in the study, participants took an intake survey that collected information on demographics and baseline travel habits. We use identifiers recorded in the survey to link study participants with King County Metro's LIFT registry, which contains additional

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

[6] Transit fares were reinstated system-wide on October 1, 2020. We were able to extend the treatment group's free transit period through December 31, 2020; we sent notices to study participants in May as well as in October 2020 alerting them of this change. Including this three-month extension, individuals in the treatment group in the second cohort received between 14 and 27 weeks of free transit.

[7] For example, four 30-day passes would cost $528 in New York City, which does not currently have a reduced-fare program for low-income riders (see https://new.mta.info/fares). However, low-income riders in San Francisco and in Dallas can buy four 30-day passes for $190–$200, similar to in King County (see https://www.sfmta.com/getting-around/muni/fares and https://www.dart.org/fare/general-fares-and-overview/fares). Sites last accessed May 1, 2024.

demographic characteristics. Combining these two data sets, we have information on study participant age, race, household size, census block group of residence, language, transit use in the 30 days prior to enrollment, and usual method of payment for transit. For participants in the second cohort, we also asked about mode of transportation to the enrollment site, whether cost represents a barrier to using public transit, and their anticipated uses of transit were it free. Using the LIFT registry, we can also track individuals' transit card use, measured as "taps" on any vehicle operated by King County Metro or a partner agency.[8] We also conducted follow-up surveys with participants to collect additional information on travel behavior as well as subjective well-being.[9]

4.2 Washington State administrative records

We use multiple state administrative datasets to capture our pre-specified primary outcomes related to employment as well as our pre-specified secondary outcomes related to public benefit receipt, arrests, and healthcare utilization.[10] 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 (i.e., whether they had any earnings), and if they were working, how much they earned and their hours of paid work.[11] These data also allow us to construct measures of job stability, including job starts and separations as well as employment continuity.

Second, we link individuals to DSHS records that report participation in public benefits programs including Supplemental Nutrition Assistance Program (SNAP), Temporary Assis-

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

[9] We ran these follow-up surveys via a text message "chatbot" in the first cohort and via traditional phone and web surveys in the second cohort. See the notes to Appendix Table A.20 and Brough, Freedman and Phillips (2022) for details.

[10] Appendix Table A.1 provides a detailed description of all outcome variables in the study, categorized according to our pre-analysis plan.

[11] 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 (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.

tance for Needy Families (TANF), Washington's Aged, Blind or Disabled Cash Assistance Program (ABD), and Washington's Housing and Essential Needs Program (HEN). We have indicator variables for participation in each program in each month.[12]

Third, we measure criminal justice system contact using Washington State Patrol (WSP) records. WSP compiles data from local jurisdictions to conduct background checks. We can track felony, gross misdemeanor, and misdemeanor arrests, and can further break out arrests by specific type (e.g., theft, assault). We observe monthly indicators for each type of arrest.

Fourth, we track individuals' healthcare utilization under Medicaid. Medicaid provides health insurance to individuals and families with low to moderate incomes. The State of Washington maintains its own Medicaid billing records, and approximately 59% of the matched study sample is eligible for Medicaid at baseline. Therefore, relying on Medicaid records is reasonably complete. We can observe any Medicaid-covered healthcare visit by month of healthcare use. We can further break out healthcare visits into emergency in- and outpatient visits as well as non-emergency in- and outpatient visits. Based on Finkelstein et al. (2012) and following our pre-analysis plan, we assign expected costs to Medicaid of visits based on the average cost of different inpatient/outpatient and emergency/non-emergency combinations.[13]

Washington DSHS's Research and Data Analysis group matched study participants who completed random assignment to state administrative records based on name and date of birth as recorded in Metro's LIFT registry. Our main sample consists of individuals who completed random assignment and matched to any of these state administrative datasets prior to enrollment. We limit the sample in this way because the internal organization of these records is such that matching to one dataset provides identifiers that facilitate exact matching to others, while failing to match to at least one dataset is not a guarantee that the individual does not appear in those datasets (given the match with our study records is

[12] A description of each of these programs appears in the notes to Appendix Table A.1.

[13] 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.

probabilistic). Because we can match on a wide array of information, and because individuals in our study are by definition DSHS clients, we have a high match rate; 89% (1,598/1,797) of people who completed random assignment appear in our analysis sample. Match rates are similar across treatment (90%) and control (88%).

4.3 Proprietary data

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

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

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

4.4 Descriptive figures

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

5 Empirical strategy

5.1 Cross-sectional treatment effects and event studies

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

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

In this regression, which we estimate on cross-sections of individuals, i indexes individuals and τ indexes time relative to study enrollment; depending on the outcome, τ refers to weeks, months, or quarters relative to study enrollment. $Y_{i\tau}$ is an outcome (for example, paid hours of work) for person i in time period τ after random assignment. The binary variable T_i indicates random assignment to treatment, and the estimate of β_τ measures the difference in average outcomes between treatment and control at time τ. We include covariates $\mathbf{X}_i$ that adjust this raw mean difference for two reasons. First, $\mathbf{X}_i$ includes an indicator for randomization strata related to the one-time change in the probability of treatment in the middle of the study. Second, in some specifications, $\mathbf{X}_i$ includes variables that reduce residual variance by predicting $Y_{i\tau}$.[14] Since random assignment was at the individual level, we compute heteroskedasticity robust standard errors. Given the number of outcomes we consider, we also account for multiple outcomes in two ways. First, we present sharpened false discovery rate (FDR) q-values to adjust for multiple hypothesis testing (Benjamini, Krieger and Yekutieli, 2006; Anderson, 2008). Second, following Finkelstein et al. (2012), we summarize estimates within each major domain (i.e., employment, public assistance receipt, financial health, criminal justice, healthcare use, and residential mobility) by calculating the average standardized treatment effect across distinct outcomes within that domain.[15]

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.[16] However, we also show event study-type figures in which we present estimates of β_τ for a range of time periods, including both pre- and post-enrollment when possible. For most

[14] We follow our pre-analysis plan by including in $\mathbf{X}_i$ indicators for female, Black, Hispanic, the month of study enrollment, and 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.

[15] Specifically, we estimate pooled OLS for all distinct outcomes within a given domain, then take the sum over coefficient estimates scaled by the standard deviations of the relevant outcomes in the control group. Notably, these standardized treatment effects give equal weight to each outcome within a domain; therefore, we also report all underlying individual coefficient estimates.

[16] 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.

outcomes, we observe data up to 8 quarters before and 8 quarters after study enrollment. Though they are less precise, we also consider cohort-specific estimates due to minor differences in sample composition and subsidy duration as well as COVID-19's disproportionate impact on the second cohort's potential outcomes.

5.2 Pooled treatment effects

Leveraging data over multiple time periods may provide a more accurate depiction of the impacts of free fares on outcomes and could also help with precision. However, pooling treatment effects over time proves complicated for two reasons. First, the COVID-19 pandemic impacts different participants at different times relative to study enrollment. As noted above, COVID-19 both directly affected outcomes and temporarily made fare-free transit available to everyone. Since the treatment subsidy ended before 2020 for the first cohort, this shock matters more for the second cohort. However, when pooling across cohorts, the same relative quarter may reflect outcomes for individuals differentially impacted by COVID-19. Second, and more mechanically, participants enter the study continuously, but we observe downstream outcomes aggregated by calendar quarter or month.[17]

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

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

We estimate this model on a panel of individuals, again indexed by i, in relative time τ. We include fixed effects for person, relative time, and calendar time (t). A new treatment variable, $\bar{T}_{i\tau}$, measures the fraction of relative time period τ for which person i received an active treatment from the study. This variable equals 1 for a treated individual in a period during which the treatment was active the entire time, zero for treated (and control)

[17]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.

individuals in a period during which the treatment was not active the entire time (including while fares were not collected during the pandemic), and a value between 0 and 1 for a treated individual in a period during which the treatment was active only part of the time.[18] The manner in which we define $\bar{T}_{i\tau}$ allows for a simple interpretation of its coefficient, γ, which will reflect the average causal effect of having fully subsidized transit for an entire time period. Since we estimate a panel with multiple observations per person, we cluster standard errors by individual with this approach.

In the presence of dynamic and heterogeneous treatment effects, estimates based on (2) could be biased. To check robustness to such concerns, we supplement our main results with estimates using methodologies developed by De Chaisemartin and D'Haultfoeuille (2024) as well as Borusyak, Jaravel and Spiess (2024). We describe these approaches further in Section 6.2.

5.3 Baseline balance

Random assignment successfully balanced baseline characteristics across control and treatment groups. Table 1 shows baseline descriptive statistics for our main analysis sample.[19] 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.

[18] 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.

[19] Appendix Table A.2 shows baseline descriptive statistics for all study participants (including those not matched to state administrative records). For the full sample, we can show balance on additional characteristics that, for confidentiality reasons, we were not permitted to match to state records. For example, we observe self-reported baseline transit use in the full sample; at the time of study enrollment, 88% of individuals assigned to both the treatment and control groups report using transit in the prior 30 days.

Consistent with randomization, individuals assigned to treatment and control are very similar. For example, 42.3% of individuals in the control group identify as White, compared to 40.7% of those in the treatment group. The regression-adjusted difference of 1.6 percentage points is identical to the raw difference between the two groups and not statistically significant at the 5% level. About 40% of both the control and treatment groups are women,[20] 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

Using data on card "taps" on King County area transit agencies' fleet of vehicles, we measure how often study participants used their cards to board public transportation. Figure 3 shows treatment effects on total transit boardings per week, as measured by card use. The estimate for each week since study enrollment is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). Individuals in the treatment group board transit using a card 6-7 additional times per week on average in the first three months after study enrollment, or about four times as often as individuals in the control group. We arrive at a similar percentage increase when we use a measure of "trips" based on consolidating boardings that happen within one hour of each other. Some of this increase could result from the treatment group shifting from untraceable payment methods, like cash or non-payment, or from travel by people other than the intended recipient. Brough,

[20] 54% of working-age food assistance recipients in Washington are women (Pavelle et al., 2019). The slightly greater share of males in our sample likely results from differential interest in transit use by gender.

Freedman and Phillips (2022) use the sub-sample survey of actual recipients to quantify changes in payment method and conclude that overall transit use at least doubles in response to treatment, even after accounting for changes in payment methods. However, given the follow-up surveys only reached a subset of participants, they cannot completely rule out that the study cards were shared with others, nor can they conclusively determine whether the treatment increased total trips across all modes.

Taken at face value, the observed changes in transit use in response to the subsidy imply welfare benefits for study participants. For example, given the typical transit trip length of one hour in our sample and assuming the time cost of travel is equal to the average sample wage of $20 per hour, then reducing the fare from $1.50 to zero reduces the total cost of transit use by 7%.[21] A person with homothetic preferences would likewise increase transit use by 7%. While the survey data cited in Brough, Freedman and Phillips (2022) has limitations, it suggests a near doubling in transit use. This much larger increase could be explained in three ways. First, riders may have a large mass of potential trips with a value just above $20, which seems improbable. Second, riders may have a time cost lower than their wage, though it would need to be $1.50 per hour to rationalize a doubling in transit use. This value seems implausible, but if riders have a time cost that low, it would support the idea that low-income riders highly value transit subsidies relative to improvements in speed. Third, and most plausible, frictions such as credit constraints, difficulty adding value to cards, or behavioral factors could amplify transit use responses to zero prices relative to small but positive prices. Any such frictions would suggest that zero prices improve welfare for study participants by allowing them to take many more high value trips.

While Figure 3 points to 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.[22] The temporary nature of changes in travel behavior motivates

[21]The median transit trip duration as measured in trip diaries collected as part of our follow-up surveys was 60 minutes.

[22]The gradual decline in transit use observed between roughly weeks 12 and 24 largely reflects variation in the duration of the subsidy individuals in the study received, as treatment effects on transit use fade quickly

our initial focus on cross-sectional regressions measuring downstream outcomes measured at approximately three months after individuals joined the study. However, for our primary and selected secondary outcomes, we also show the full time path of treatment effects in event study figures as well as present results from panel regressions that pool treatment effects over longer time horizons.

6.2 Labor market outcomes

We observe relatively small changes in our pre-specified primary employment outcomes in response to transit subsidies. Table 2 shows mean employment-related outcomes one quarter after study enrollment for the control and treatment groups in columns (1) and (2), respectively. Column (3) displays the "simple" regression-adjusted 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. For each coefficient estimate, we present the standard error in parentheses and the associated p-value in brackets. We also present a sharpened FDR q-value adjusted for multiple hypothesis testing in braces, where the adjustments are based on all nine outcomes reported in the table.

The first row of Table 2 shows results for paid hours worked in the first full quarter after study enrollment ($\tau = +1$); the sample in this case includes those with zero recorded work hours, and therefore the measured effect captures both extensive and intensive margin adjustments. On average, the treatment group works in UI-covered jobs for 81.5 hours in the quarter after random assignment, compared to 76.8 hours in the control group. The gap of 4.7 hours between the two groups widens to 5.6 hours when controlling for the randomization regime but narrows to 1.6 hours when controlling for other baseline characteristics. Whether we rely on conventional p-values or sharpened FDR q-values, we cannot rule out that the treatment effect on paid hours worked in the quarter after study enrollment is zero. Based

after the subsidy ends (Brough, Freedman and Phillips, 2022).

on the heteroskedasticity-robust standard error reported in the table, the 95% confidence interval for the regression adjusted difference in column (5) spans -15.1 to 18.3 hours. This range includes values that are large relative to the control group mean, but that are small relative to full-time work hours. For example, 18.3 hours of paid work corresponds to 23.8% of the control group mean, but only 3.5% of full-time work hours. While these small changes in overall paid hours worked could mask larger intensive margin adjustments among a subset of individuals, we find no heterogeneity in effects by baseline employment status, as discussed in Section 6.4. Additionally, quantile regressions in Appendix Figure A.1 do not indicate any change in hours worked on the intensive margin across the distribution.

We similarly observe only small, statistically insignificant changes in the other employment-related outcomes that we can measure using administrative data. Based on our cross-sectional model with controls (column (4) of Table 2), average earnings increase by only $8 per quarter (0.5%). The control group means and treatment effects for paid work hours and earnings imply that hourly wage rates for the treatment group in the quarter after enrollment fall slightly from $19.00 to $18.70. Meanwhile, the probability of any UI-covered employment in the quarter after study enrollment is slightly lower in the treatment group than in the control group, at 29.5% vs. 32.2%. Job transitions also do not change substantially. The point estimates indicate a statistically insignificant 2.9 percentage point decline in job starts (measured as having no hours worked in $\tau = -1$ and positive hours worked in $\tau = +1$) and a 0.9 percentage point increase in job exits (measured as having positive hours worked in $\tau = -1$ and no hours worked in $\tau = +1$). We also detect no change in continuous employment between pre- and post-enrollment periods (measured as having positive hours worked in both $\tau = -1$ and $\tau = +1$), a measure of job stability; this is true regardless of whether we measure it for any employment or employment in narrowly defined industries. The likelihood of being continuously unemployed between quarters before and after study enrollment (i.e., no hours worked in either $\tau = -1$ or $\tau = +1$) is also similar between control and treatment groups. Following Finkelstein et al. (2012), we summarize these results by estimating an average standardized treatment effect. The standardized treatment effect indicates that free transit fares are associated with a 0.021 standard deviation decrease in employment-related outcomes (SE = 0.040).[23]

Treatment effects over time show similar patterns. Panels (a)-(c) of Figure 4 show treatment effects for paid hours worked, earnings, and any employment from regressions with full controls, estimated in each quarter relative to the time of study enrollment. It shows no statistically significant differences in these outcomes between treatment and control groups for at least eight quarters after random assignment. Estimates based on the panel data model (equation (2)) corroborate these results. As shown in the first row of Table 3, a model 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 -1.2 per quarter. At the 5% level, we can reject changes in paid hours worked greater than 9.8 and less than -12.2. Based on these estimates, we can reject a null that paid hours worked per quarter increase by 10% of the control group mean and 2% of full-time employment. We can similarly reject large positive effects on earnings, any employment, job gain, and job loss.

To ensure our primary two-way fixed effects model is not contaminated by known bias from dynamic, heterogeneous treatment effects, we also measure pooled treatment effects using two alternative estimators: De Chaisemartin and D'Haultfoeuille (2024) and the imputation method of Borusyak, Jaravel and Spiess (2024). To implement both estimators for our employment outcomes, we create a dichotomous treatment based on whether any part of a quarter was treated. De Chaisemartin and D'Haultfoeuille's (2024) approach accommodates the same non-absorbing treatment indicator we use in our basic pooled approach. However, to implement Borusyak, Jaravel and Spiess's (2024) imputation-based estimator,

[23]Since some outcomes are collinear, we estimate the standardized treatment effect just for hours worked, earnings, employment, job gain, and job loss. We reverse the sign of the coefficient for job loss such that positive coefficients always imply "better" employment outcomes. Including all nine outcomes (reversing the signs of both job loss and continuous unemployment) yields a standardized treatment effect of -0.011 (SE = 0.035).

we must use an absorbing treatment variable. We obtain qualitatively similar estimates regardless of the approach. The bottom two rows of Table 3 show results using De Chaisemartin and D'Haultfoeuille's (2024) and Borusyak, Jaravel and Spiess' (2024) estimators (labeled DCDH and BJS, respectively). The estimates remain statistically insignificant for all five outcomes. For paid hours worked, earnings, and any employment, the estimates are, if anything, less positive; for example, based on Borusyak, Jaravel and Spiess' (2024) estimator, we can reject any more than a one hour increase in paid hours worked. Because of the importance of partially treated quarters and to be conservative in interpreting the results, our preferred estimates are those from the basic pooled model.

While we are not powered to detect downstream effects for each cohort individually, we nonetheless separately estimate treatment effects for employment-related outcomes for each of the two cohorts. These results appear in Appendix Figures A.2 and A.3.[24] Results by cohort are helpful for testing whether COVID-19 contributed to the null effects. Point estimates for the smaller, 2019-enrolled first cohort are positive for paid hours worked in quarters 0 and 1, though imprecisely measured. However, quarter 0 results for the larger second cohort go in the opposite direction and also occur pre-COVID. We also see little effect on earnings or whether an individual is employed in either cohort. We cannot statistically rule out that differences in treatment effects for paid hours worked, earnings, or employment are different across the two cohorts, nor that any are different from zero.

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

[24] Additional employment results broken down by cohort appear in Appendix Tables A.3 and A.4.

6.3 Secondary outcomes

Following our pre-analysis plan, we consider a number of secondary outcomes. The cross-sectional treatment effects for these outcomes are reported in Table 4.[25] As in Table 2, we present control and treatment group means along with regression-adjusted differences. For each coefficient estimate, we again report a heteroskedasticity-robust standard error and p-value as well as a sharpened FDR q-value that adjusts for multiple hypothesis testing. We also calculate standardized treatment effects across distinct outcomes within each domain.

6.3.1 Public assistance

We find little evidence that transit subsidies help connect study participants to public benefits. The first panel of Table 4 shows these results. For indicators of receiving any benefits and receiving food benefits three months after study enrollment, we observe null effects of the treatment. However, there is limited scope for the transit subsidy to affect these outcomes; due to the way in which study enrollment was conducted at DSHS offices, over 90% of individuals in the experiment receive SNAP in the first quarter after random assignment. Control group rates of receiving TANF cash assistance or other program benefits are lower, at 2% and 13%, respectively. Still, the treatment group appears no more likely to access these assistance programs, suggesting that free transit does not help people sign up for or maintain public benefits.[26]

6.3.2 Finances

Despite no change in access to financial resources from employment or public benefits, we find some suggestive evidence that transit subsidies improve the financial situation of the treatment group, at least in the short run. We match a sub-sample of study participants

[25] All secondary results broken down by cohort are reported in Appendix Tables A.5 and A.6.

[26] Event studies and panel regressions confirm the absence of any impacts of the treatment on public benefit receipt; see Appendix Figure A.4 and Appendix Table A.7. The average standardized treatment effect for the three separate outcomes of receiving SNAP, TANF, or other public benefits is a statistically insignificant -0.033 (SE = 0.032).

to credit records. The second panel of Table 4 shows results using credit-related outcomes in the first full quarter after enrollment.[27] Based on our regressions with full controls (column (6)), total debt balances are $97 (5%) lower for the treatment group and credit scores are 13 points (3%) higher. In this smaller sample, neither of these estimates is statistically significant even prior to adjusting for multiple hypothesis testing. However, the point estimates are large; for example, the credit score effect is over half the size of that associated with economically important events like being evicted (Collinson et al., 2024) or having a bankruptcy removed from one's record (Gross, Notowidigdo and Wang, 2020). Consistent with the strong immediate impact of free fares on transit use, any effects on treated participants' financial situations also appear soon after random assignment, as shown in the event studies in panels (a) and (b) of Figure 5. Other variables observed on credit reports further suggest improved financial situations. For instance, we see members of the treatment group seeking less new credit after random assignment. Measured one quarter after study enrollment, individuals in the treatment group have made 0.08 (24%) fewer new credit inquiries in the past three months. This difference suggests that the financial situation of those who receive free transit improves such that they do not need to open new lines of credit.

The standardized treatment effect corroborates these apparent improvements in financial health. Calculated over all three outcomes in Panel B of Table 4, the average standardized treatment effect indicates that the transit subsidy is associated with a statistically significant 0.091 standard deviation improvement in financial health (SE = 0.038).[28] Notably, however, the treatment effect estimates pooled over time are mixed for financial health outcomes, suggesting that improvements in financial circumstances may be short-lived.[29]

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

[28] In calculating the standardized treatment effect for financial outcomes, we reverse the signs of balance in collection and total inquiries, under the assumption that greater debt and more inquiries correspond to greater financial hardship.

[29] See Appendix Table A.8.

6.3.3 Contact with the criminal justice system

We find some indication that the transit subsidy reduces contact with the criminal justice system, although estimated effects on arrests are imprecisely measured. As the third panel of Table 4 shows, arrest rates among individuals in the treatment group in the three months after study enrollment are 1.5 percentage points lower than those in the control group, at 11.1% vs. 13.6%. While the cross-sectional estimate is not statistically significant, it amounts to an economically meaningful 11% decline in the likelihood of arrest within three months. In addition, we find a very similar magnitude (-1.4 percentage points) and statistically significant effect of free transit access on arrests when we pool post-enrollment periods with our panel approach.[30] 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.[31] These arguably represent the types of crimes that improved mobility, or the eased financial constraint owing to free transit, might help to avert. In contrast, we see no evidence of impacts of free transit fares on crimes with less of a financial motive or where transportation is less likely to have posed an important obstacle, such as assaults, sex crimes, domestic violence, custody violations, alcohol/drug violations, or weapons violations. Although they are only suggestive, taken together these results indicate that free public transportation may reduce individuals' likelihood of coming into contact with the criminal justice system.

6.3.4 Healthcare use

People receiving transit subsidies appear less likely to use healthcare. The fourth panel of Table 4 shows average healthcare use during the first three months after study enrollment, as measured by Medicaid claims records. Our first pre-specified healthcare outcome, the

[30] See Appendix Table A.7. We show event study estimates for arrests in panel (c) of Figure A.4.

[31] See Appendix Table A.9. Standardized treatment effects are negative, but statistically insignificant when estimated over individual crime categories.

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; a test with a 5% significance level cannot reject a decline of \$404, or 41% of the control group mean. We have greater power for detecting changes in healthcare visits. In the control group, 34.7% of participants have a healthcare visit of some kind within three months of random assignment. This value is 5.6 percentage points lower in the treatment group; the simple regression-adjusted difference in the probability of a healthcare visit is statistically significant at the 5% level based on the unadjusted p-value (and the 10% level based on the sharpened FDR q-value). Panel (c) of Figure 5 shows that the effect on healthcare visits materializes within three months of study enrollment and does not grow in magnitude subsequently. Our pooled treatment effect estimates further confirm that the impacts are concentrated in the months immediately following random assignment.[32] 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. Estimated over the four different types of healthcare visits in Panel D of Table 4, the average standardized treatment effect implies that free transit fares are associated with a marginally statistically significant 0.063 standard deviation decrease in healthcare utilization in the quarter after study enrollment (SE = 0.033).

6.3.5 Residential location

We do not detect large changes in residential mobility in response to transit subsidies. The final panel of Table 4 shows these results for the sub-sample of study participants that match to consumer reference records. Overall rates of moving are relatively low. In the three months after random assignment, only 1.2% of the control group made any residential move. Move rates within three months are somewhat lower in the treatment group at 1.0%;

[32] See Appendix Table A.7.

the regression-adjusted difference is -0.3 percentage points. While the point estimate is small in magnitude, the standard error is large. Our pooled treatment effect estimates are more precise and closer to zero, but we still cannot rule out sizable impacts of free fares on residential mobility.[33]

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

6.4 Heterogeneous effects

The average treatment effects we estimate may mask heterogeneity in impacts across subgroups. Understanding any heterogeneity in effects is important from a program targeting perspective. It can also speak to how specific our results are to the particular study sample. For example, the lack of observed effects on paid hours worked and other employment-related outcomes may stem in part from study participants' relatively low overall attachment to the labor force. If few individuals in our study are on the margin of working for pay, then public transit access might have a muted average effect on employment in our sample but a large effect in the full population of people with low income.[34]

In the data, we find limited evidence of heterogeneity in effects for most outcomes, including for our primary employment-related outcomes. 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 were and were not

[33] See Appendix Table A.10. We show event study estimates for residential moves in panel (b) of Figure A.4. Standardized treatment effects estimated over the four types of moves in Panel E of Table 4 imply a statistically insignificant 0.002 standard deviation increase in residential mobility in the quarter after study enrollment (SE = 0.059).

[34] 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.

employed at baseline, where employed at baseline is defined as ever having positive earnings in the four quarters prior to study enrollment. Conditional on being employed at baseline, individuals in the control group work for pay an average of 118 hours in the quarter after random assignment. Those in the treatment group work 8 more hours on average. This subgroup treatment effect is somewhat larger than the full-sample estimate, but is small in practical terms and not statistically different from either zero or the subgroup effect for people not employed at baseline. As shown in subsequent panels of Table 5, we also find no indication of heterogeneity in treatment effects on other primary and secondary outcomes based on pre-enrollment employment status.

Similarly, we find little evidence of stronger treatment effects among people who anticipated longer subsidy durations. Columns (3) and (4) of Table 5 split the sample into people who at the time of random assignment were offered below median versus above median subsidy duration, relative to their cohort. The above median group does not have noticeably more positive effects on employment or other outcomes. Breakouts by participant race and gender also point to little heterogeneity in effects on employment outcomes. However, we find some limited evidence of heterogeneity in effects on healthcare use. As shown in the fifth panel of Table 5, there are relatively large declines in healthcare use for participants who are White.[35]

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 hours worked and earnings, though some for having any employment.[36] Their method also identifies some heterogeneity in effects for healthcare outcomes, pointing to potentially stronger impacts of free transit for those with a recent history of medical visits. Finally, there appears to be some heterogeneity in effects for arrests, with impacts varying by

[35] For additional heterogeneity tests, see Appendix Tables A.11–A.17.

[36] See Appendix Table A.18. We provide more details on the methodology in the notes to the table.

educational attainment, gender, and prior public assistance receipt. However, an omnibus F-test of heterogeneity cannot reject the null of no heterogeneity in the causal forest for any outcome domain.

7 Discussion

We might expect free transit fares to impact employment outcomes for several reasons. First, prior work, including several RCTs, has documented impacts of transit subsidies on job search behavior (Phillips, 2014; Franklin, 2018; Abebe et al., 2021; Banerjee and Sequeira, 2023). Second, the proximate cause for many visits to DSHS offices and, in turn, enrollment in our study may have been a recent job loss or reduction in hours of paid work (see Figure 2); this, together with the fact that many programs in which individuals enroll at DSHS (e.g., SNAP and TANF) have some form of work requirement for able-bodied adult participants, might suggest that a significant fraction of individuals in our study would be looking for work. Third, employment among individuals in our study tends to be concentrated in lower-wage, higher-turnover industries; among those formally employed in the two years prior to study enrollment, the most common industries of work are restaurants and temporary work agencies.[37] Fourth, people in our study who are employed generally have scope to increase hours worked; for example, people in the control group who are employed work 240 hours per quarter, on average, or 46% of full-time hours.

However, our results suggest that transit subsidies have little effect on our primary employment-related outcomes. There could be several reasons for the null effects. First, as the subsidy was temporary, individuals may not have made decisions regarding employment due to the subsidy alone. Longer term access to free transit may have induced changes in behavior around paid work. The subsidy's temporary nature also may be part of the reason for the seeming contrast between our results and those of studies that have focused on

[37]See Appendix Table A.19 for a breakdown of the most common 4-digit industries in which study participants were employed in the 12 quarters prior to enrolling in the experiment, based on UI tax records.

the the implications of permanent expansions in transit infrastructure on employment outcomes for individuals with low income (e.g., Holzer, Quigley and Raphael (2003)). However, as noted above, study participants tend to work in short-term, high turnover jobs. Further, we find no evidence that participants who anticipated longer subsidy durations had stronger positive treatment effects on employment-related outcomes.

Second, as previously discussed, many individuals in our sample might be inframarginal with respect to joining the labor force, such that the treatment has a muted effect of employment-related outcomes. In this case, our results may not generalize to a broader population, and in particular one with stronger attachment to the labor force. However, we find limited evidence of differentially large treatment effects for employment or any other outcomes when we focus on a subset of participants with observed past engagement in formal paid work, which suggests that the null employment effects are not likely attributable to this feature of participants in our study.

Third, limitations of the existing transit infrastructure in Seattle may be so significant that they blunt the impact of free fares on travel for work purposes. While Seattle's transit system is more extensive than that of many other U.S. cities, it still may fail to adequately connect lower income communities to job locations. However, individuals with low income in the Seattle area are frequent transit riders, suggesting that access alone is not a major barrier to use. At baseline, 88% of study participants reported using transit at least once in the prior month, and on average individuals reported using it 15 of the previous 30 days.[38] Additionally, not only did fare-free transit sharply increase transit use among study participants, but treatment effects on transit use varied little by transit accessibility (whether a bus stop or station was located in one's own home block group) (Brough, Freedman and Phillips, 2022). This result implies that transit service quality and accessibility alone may not be a key driver behind the results.[39]

[38]See Appendix Table A.2.

[39]We unfortunately cannot test for heterogeneity in treatment effects on downstream outcomes based on transit accessibility due to data sharing agreements that precluded linking residence information to state administrative records.

Taken together, our results suggest that transit fares are more of a barrier to engaging in non-work activities than engaging in paid work among individuals with low income. Thus, to the extent that free transit improves low-income individuals' well-being, it does so for reasons other than increasing paid employment. While small scale and potentially selective, a follow-up survey corroborates this interpretation. Relative to those in the control group, those in the treatment group report improvements in well-being across several domains, including transportation, finances, employment, and health (but not housing or education).[40] 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. While 52% of study participants said they would use it to travel to work, this category only ranked sixth out of ten. More participants expected to use to the transit card for shopping (71%), errands (62%), visiting family and friends (61%), using healthcare (60%), and visiting the public benefits office (56%).[41] Measuring trip purposes for actual trips taken is more difficult; we must rely on follow-up surveys for a small and selected sample. 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.[42]

In principle, the money freed up by a transit subsidy could be driving the seeming positive effects on some aspects of people's lives. In that case, the cash equivalent of the in-kind transfer might have similar impacts. We believe this is unlikely for three reasons. First, based on either per trip fares or the cost of monthly passes, the cash equivalent of the transfer was at most $200, which represents only about 2.5% of average annual earnings

[40]See Appendix Table A.20 for details on self-reported well-being in the follow-up survey. We are cautious in interpreting these both because of the small and potentially selected sample, but also because of potential demand effects.

[41]See Appendix Figure A.5 for details.

[42]The bottom panel of Appendix Table A.20 shows how people who have at least one transit trip sampled for the survey split their transit trips across different trip purposes.

among individuals in our sample. Second, the transit subsidy sharply increased transit use relative to what would be expected with a cash transfer. According to the 2019 Consumer Expenditure Survey, the budget share for transportation as a whole (not just for public transit) among households in the bottom quintile of the income distribution is 16%; even if individuals would have allocated that entire fraction of a $200 transfer to transit alone, it would translate into 21 additional trips, or less than one-fifth the additional trips we observed as a result of the treatment. Finally, other recent work on one-time small-scale cash transfers to similar populations point to little impact on measures of hardship or subjective well-being (Jacob et al., 2022; Jaroszewicz et al., 2023; Pilkauskas et al., 2023).

8 Conclusion

This paper reports the results of a randomized controlled trial that provided several months of fare-free public transportation to individuals with low income. Among a group of people enrolling in public benefits in the Seattle area during 2019 and 2020, we compare how recipients of free transit differ from people who pay $1.50 per bus ride on a rich set of outcomes derived from administrative and proprietary data. We do not detect large effects of free transit access on employment outcomes. Although only suggestive, we find some evidence that transit has benefits outside the confines of the formal labor market. Transit fares may therefore represent a larger barrier to pursuing many non-work activities than they do for engaging in paid employment among individuals with low income.

The results from this study might not generalize to a broader population of low-income individuals, or to other cities with different levels of transit service coverage, frequency, and reliability. It is also possible that, while sufficient to affect some aspects of individuals' lives, the subsidy did not last long enough to influence decisions about employment. Finally, the current transit environment may differ from that of the study, which took place prior to and during the early stages of the COVID-19 pandemic. Future work leveraging the introduction of permanent, at-scale free-fare programs may be able to speak to these issues as well as shed more light on the potential general equilibrium implications of subsidized fare policies.

Nonetheless, our findings can help to inform current debates around the optimal level of public investment in transit provision, and in particular to the trade-offs associated with a move toward free public transit. Even in the absence of changes in overall mobility or employment, scale economies associated with public transit as well as the negative externalities associated with private vehicular travel may imply efficiency gains from fare reductions if they induce travel mode substitution. Means-tested transit subsidies could also have important redistributional benefits. Our results suggest that, more so than from increasing employment, these benefits accrue from improving other aspects of the lives of individuals with low incomes, potentially by expanding access to amenities and services. Any welfare gains for low-income riders arising from free transit therefore flow mostly, if not entirely, from sources other than formal employment.

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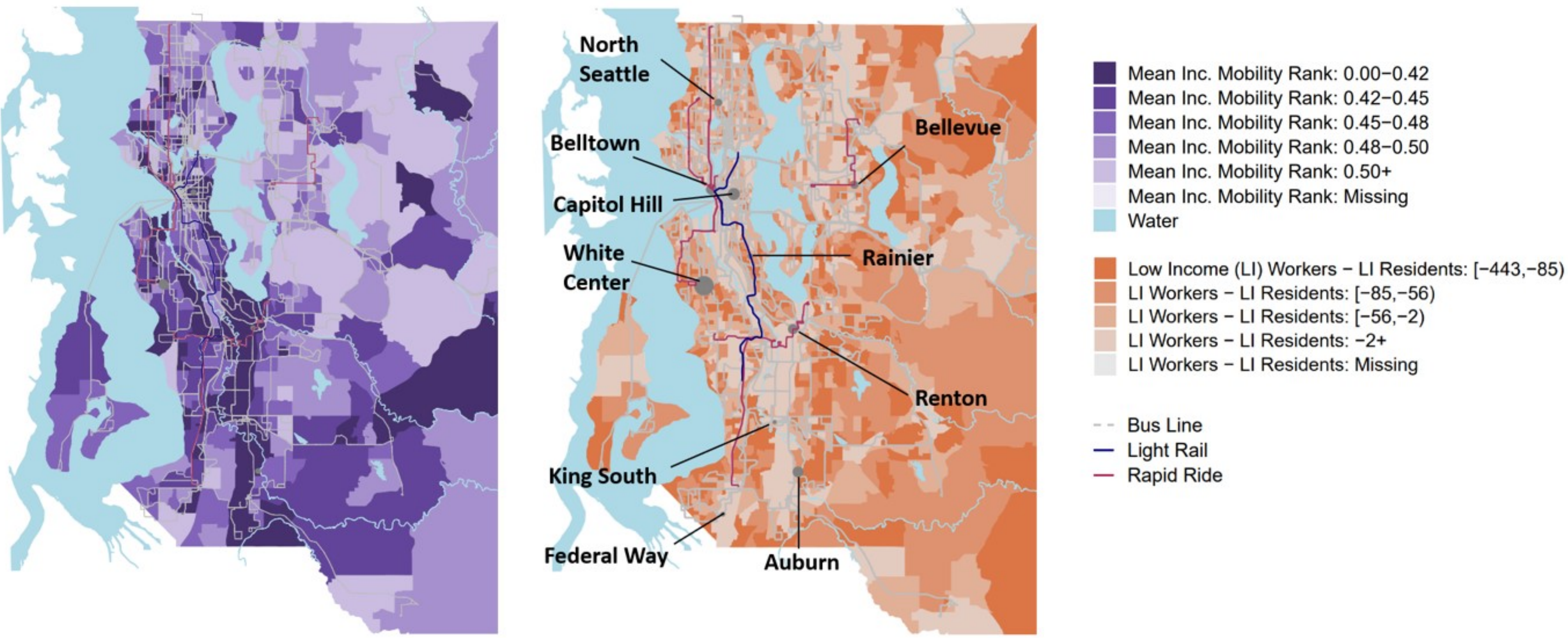
Figures

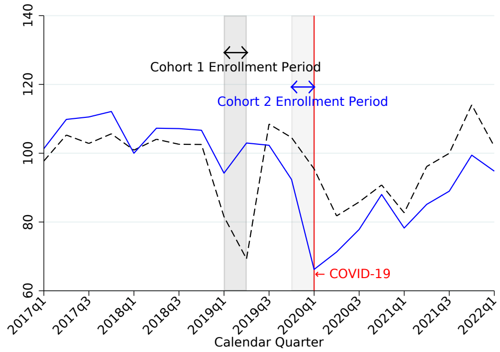
Figure 1. Economic Mobility and Spatial Mismatch in King County, Washington



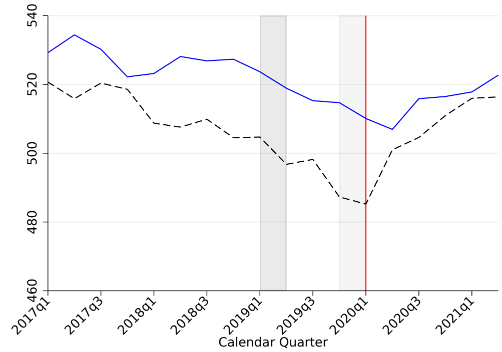
Map A **Map B**

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

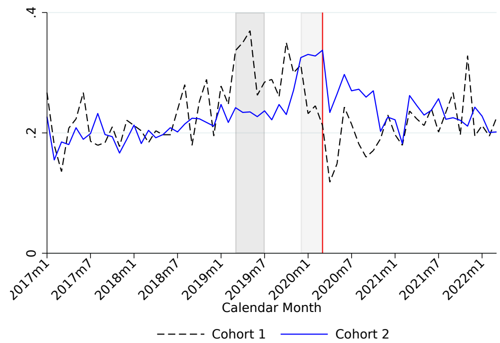
Figure 2. Mean Outcomes, by Calendar Time and Cohort



(a) Paid Hours Worked



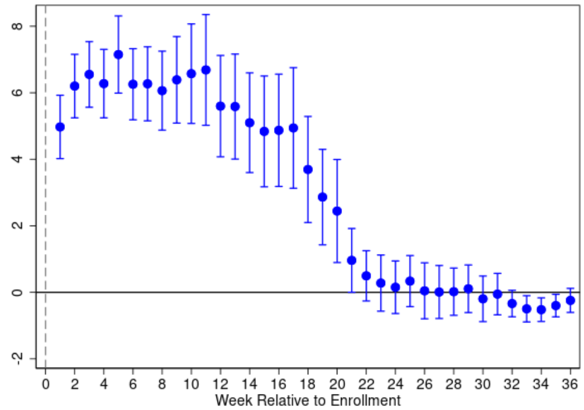
(b) Credit Scores



(c) Medical Visits

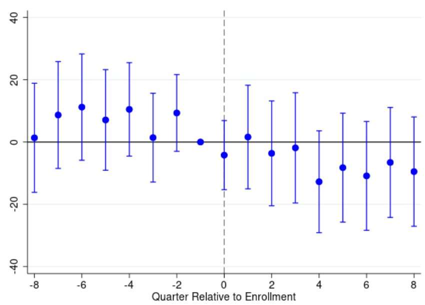
Notes: These figures display trends in mean (a) paid hours worked, (b) credit scores, and (c) Medicaid-covered doctor, clinic, or hospital visits by cohort. Paid hours worked (Washington State UI records) and credit scores (Experian) are measured at a quarterly frequency, while Medicaid visits are measured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

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

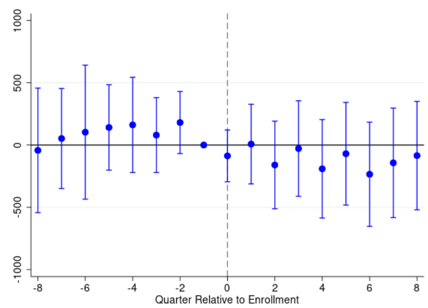


Notes: This figure depicts treatment effects on transit card use over time. Each dot measures the treatment effect of receiving free public transit at the relative week indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression,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

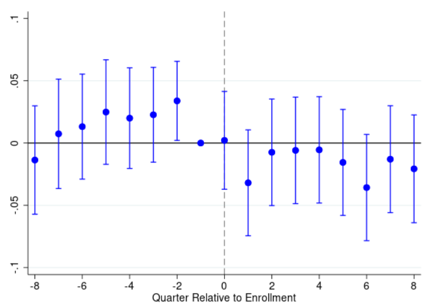
Figure 4. Treatment Effects on Employment Outcomes, by Relative Time



(a) Quarterly Paid Hours Worked



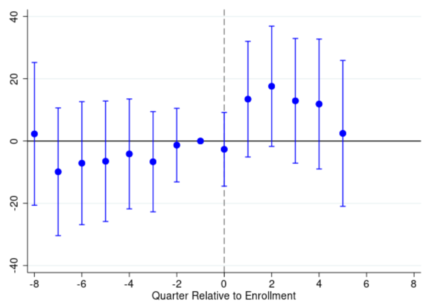
(b) Quarterly Earnings



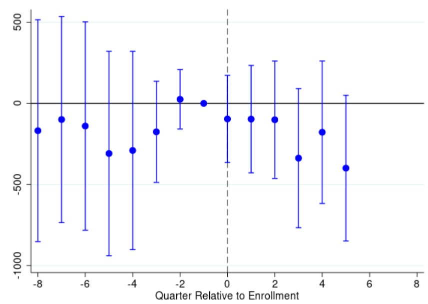
(c) Any Paid Employment

Notes: This figure depicts treatment effects on (a) paid hours worked, (b) earnings, and (c) any paid employment over time. Each dot measures the treatment effect of receiving free public transit at the relative quarter indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). Outcomes are measured using Washington State UI records. Control variables are the outcome in the quarter 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.

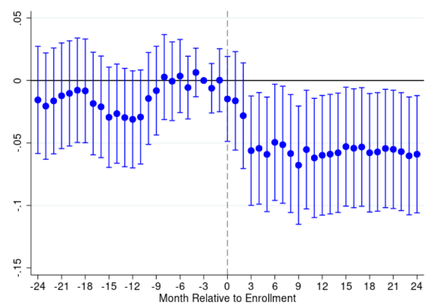
Figure 5. Treatment Effects on Financial and Health Outcomes, by Relative Time



(a) Credit Score



(b) Balance in Collections



(c) Any Medical Visit, Cumulative

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

Tables

Table 1. Mean Baseline Characteristics by Treatment Assignment

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

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

Table 2. Employment Outcomes, One Quarter After Study Enrollment

	(1) **Control**	(2) **Treatment**	(3) **Simple Reg. Adj. Diff.**	(4) **Reg. Adj. Diff.**
	Mean		Coef. Est. (SE) [P-Value] {Q-Value}	
(a) Paid hours worked	76.8	81.5	5.6 (8.9)	1.6 (8.5)
			[0.53] {1.00}	[0.85] {1.00}
(b) Earnings ($)	1,459	1,477	48 (170)	8 (163)
			[0.78] {1.00}	[0.96] {1.00}
(c) Any paid employment	0.32	0.30	-0.02 (0.02)	-0.03 (0.02)
			[0.33] {1.00}	[0.14] {1.00}
(d) Job gain	0.13	0.11	-0.03 (0.02)	-0.03 (0.02)
			[0.10] {1.00}	[0.08] {1.00}
(e) Job loss	0.14	0.15	0.01 (0.02)	0.01 (0.02)
			[0.58] {1.00}	[0.61] {1.00}
(f) Continuous employment	0.19	0.19	0.004 (0.02)	0.001 (0.02)
			[0.84]{1.00}	[0.96]{1.00}
(f.i) Continuous sector employment	0.13	0.13	0.003 (0.02)	0.004 (0.02)
			[0.85] {1.00}	[0.83] {1.00}
(f.ii) Continuous industry employment	0.11	0.11	0.004 (0.02)	0.006 (0.02)
			[0.80] {1.00}	[0.69] {1.00}
(g) Continuous unemployment	0.54	0.55	0.01 (0.03)	0.02 (0.03)
			[0.61] {1.00}	[0.47] {1.00}
Stand. treatment effect (SE), (a)-(e)			-0.021 (0.040)	
N	977	621		

Notes: This table presents means and regression-adjusted differences in means for employment outcomes measured in the quarter after enrollment ($\tau = +1$) using Washington State UI records. Continuous employment, job gains, and job losses are measured by comparing the quarter before and the quarter after enrollment. Sectors and industries are defined by 2-digit and 6-digit NAICS codes, respectively. Columns (3) and (4) show results from estimating equation (1) with different sets of controls. 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, and month of study enrollment; for paid hours worked, earnings, and any paid employment, it also adjusts for the relevant outcome one quarter prior to study enrollment. The second to last row reports the standardized treatment effect, which tests whether the weighted sum of beta coefficient in (a)-(e) divided by the standard deviation of each outcome variable of the control group is equal to zero. The sample is limited to individuals who go through random assignment and match to any Washington State administrative record prior to study enrollment. Heteroskedasticity-robust standard errors are reported in parentheses and the associated p-values are reported in brackets. Sharpened FDR q-values that adjust for multiple hypothesis testing are reported in braces. The standardized treatment effect tests whether the weighted sum of coefficients of specified outcomes divided by the standard deviations of the outcomes for the control group is equal to zero.

Table 3. Employment Outcomes, Panel Regressions

	(1) **Paid Hours**	(2) **Earnings**	(3) **Any Employment**	(4) **Job Gain**	(5) **Job Loss**
Pooled	-1.2	-16	0.004	-0.003	0.005
	(5.6)	(120)	(0.015)	(0.01)	(0.01)
DCDH	-10.9	-145	-0.03	0.03	-0.042
	(20.0)	(410)	(0.049)	(0.031)	(0.035)
BJS	-15.4	-232	-0.03	-0.001	-0.0002
	(8.2)	(187)	(0.022)	(0.005)	(0.006)
Control Mean	96	1,822	0.31	0.05	0.05
Observations	27,166	27,166	27,166	27,166	27,166
Individuals	1,598	1,598	1,598	1,598	1,598

Notes: Each cell in this table presents the estimate of the coefficient on treatment from a separate panel data regression of the listed outcome on an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. Pooled, DCDH, and BJS in rows 1-3 refer to results based on three separate estimators. Row 1 ("Pooled") presents results from a standard panel regression (equation (2)) where the treatment variable is equal to zero for individuals in the control group and equals the fraction of the quarter in which the treatment is active for those in the treatment group. Row 2 and 3 use alternative estimators robust to dynamic treatment effects. Row 2 ("DCDH") uses the De Chaisemartin and D'Haultfoeuille (2024) estimator and is defined as a binary treatment indicator equal to 1 if the active treatment variable is greater than 0. This is a non-absorbing treatment variable. Effects are estimated for switchers into treatment for at most 3 relative quarters upon enrollment; this is because the active treatment is at most 3 consecutive quarters. Finally, row 3 ("BJS") uses the Borusyak, Jaravel and Spiess (2024) estimator. This estimator is absorbing by nature, so treats all quarters following enrollment as treated for those assigned to the treatment group. For all specifications, the panel consists of 8 quarters prior to study enrollment and 8 quarters following study enrollment for all sample individuals. The sample is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are reported in parentheses.

Table 4. Secondary Outcomes, One Quarter After Enrollment

	(1) Control Mean	(2) Control N	(3) Treatment Mean	(4) Treatment N	(5) Simple Reg. Adj. Diff. Coef. Est. (SE) [P-Value] {Q-Value}	(6) Reg. Adj. Diff. Coef. Est. (SE) [P-Value] {Q-Value}
A. Public Assistance Receipt, measured three months post enrollment						
Any food or cash benefits	0.93	977	0.91	621	-0.02 (0.01) [0.18] {1.00}	-0.02 (0.01) [0.24] {1.00}
(a) SNAP	0.91	977	0.89	621	-0.02 (0.02) [0.12] {0.90}	-0.02 (0.02) [0.16] {0.99}
(b) TANF	0.02	977	0.03	621	0.01 (0.01) [0.41] {1.00}	0.003 (0.01) [0.71] {1.00}
(c) Other	0.13	977	0.11	621	-0.02 (0.02) [0.23] {1.00}	-0.01 (0.01) [0.33] {1.00}
Stand. treatment effect (SE), (a)-(c)					-0.033 (0.032)	
B. Financial Health, measured in the third month of the quarter post enrollment						
(a) Balance in collection ($)	1,622	492	1,364	334	-220 (220) [0.32] {1.00}	-97 (169) [0.57] {1.00}
(b) Credit Score	501	492	514	334	9 (14) [0.50] {1.00}	13 (9) [0.16] {0.99}
(c) Total inquiries in past 3 months	0.34	492	0.26	334	-0.10 (0.04) [0.01] {0.09}	-0.08 (0.04) [0.05] {0.44}
Stand. treatment effect (SE), (a)-(c)					0.091 (0.038)	
C. Criminal Justice, measured three months post enrollment						
Any arrest, cumulative	0.14	977	0.11	621	-0.02 (0.02) [0.20] {1.00}	-0.02 (0.02) [0.35] {1.00}
(a) Any misdemeanor, cumulative	0.02	977	0.01	621	-0.002 (0.01) [0.79] {1.00}	-0.0007 (0.01) [0.90] {1.00}
(b) Any gross misdemeanor, cumulative	0.05	977	0.04	621	-0.01 (0.01) [0.51] {1.00}	-0.01 (0.01) [0.59] {1.00}
(c) Any felony, cumulative	0.06	977	0.05	621	-0.003 (0.01) [0.77] {1.00}	-0.002 (0.01) [0.85] {1.00}
Stand. treatment effect (SE), (a)-(c)					-0.020 (0.035)	
D. Healthcare, measured three months post enrollment						
Cost to Medicaid, cumulative ($)	975	977	913	621	-43 (176) [0.81] {1.00}	-77 (167) [0.64] {1.00}
Any Medicaid visit, cumulative	0.35	977	0.28	621	-0.06 (0.02) [0.01] {0.09}	-0.06 (0.02) [0.012] {0.29}
(a) Emergency outpatient	0.25	977	0.21	621	-0.03 (0.02) [0.12] {0.90}	-0.03 (0.02) [0.12] {0.99}
(b) Emergency inpatient	0.04	977	0.04	621	-0.01 (0.01) [0.43] {1.00}	-0.01 (0.01) [0.39] {1.00}
(c) Non-emergency outpatient	0.30	977	0.24	621	-0.06 (0.02) [0.01] {0.09}	-0.05 (0.02) [0.021] {0.29}
(d) Non-emergency inpatient	0.02	977	0.02	621	-0.0009 (0.01) [0.91] {1.00}	0.00004 (0.01) [1.00] {1.00}
Stand. treatment effect (SE), (a)-(d)					-0.063 (0.033)	
E. Residential Mobility, measured three months post enrollment						
Any move	0.012	432	0.010	290	-0.003 (0.008) [0.73] {1.00}	-0.003 (0.008) [0.71] {1.00}
(a) Any move in state	0.007	432	0.010	290	0.003 (0.006) [0.66] {1.00}	0.002 (0.007) [0.75] {1.00}
(b) Any move out of state	0.005	432	0.003	290	-0.003 (0.005) [0.55] {1.00}	-0.002 (0.005) [0.67] {1.00}
(c) Any move in county	0.005	432	0.010	290	0.005 (0.006) [0.42] {1.00}	0.004 (0.007) [0.51] {1.00}
(d) Any move out of county	0.007	432	0.003	290	-0.005 (0.005) [0.34] {1.00}	-0.004 (0.005) [0.43] {1.00}
Stand. treatment effect (SE), (a)-(d)					0.002 (0.059)	

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

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

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-White	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Paid hours worked								
Control Mean	42.51	118.19	82.71	68.41	51.18	95.61	80.94	70.85
Reg. Adj. Diff.	-4.53	8.00	5.26	-1.78	3.74	1.54	-5.35	9.49
SE	(9.08)	(14.86)	(11.77)	(11.49)	(9.31)	(12.68)	(10.93)	(13.42)
P-Value for Diff.	[0.47]		[0.67]		[0.75]		[0.39]	
Employed								
Control Mean	0.17	0.51	0.32	0.33	0.24	0.38	0.32	0.32
Reg. Adj. Diff.	-0.04	-0.04	-0.03	-0.04	-0.004	-0.05	-0.04	-0.02
SE	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value for Diff.	[1.00]		[0.85]		[0.20]		[0.65]	
Any public benefits								
Control Mean	0.94	0.92	0.93	0.94	0.96	0.91	0.93	0.93
Reg. Adj. Diff.	-0.01	0.01	-0.01	0.01	0.0001	-0.004	-0.003	0.01
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
P-Value for Diff.	[0.28]		[0.45]		[0.91]		[0.61]	
Any arrest, cumulative								
Control Mean	0.17	0.09	0.15	0.12	0.14	0.13	0.18	0.07
Reg. Adj. Diff.	-0.02	-0.01	-0.02	-0.01	-0.02	-0.01	-0.003	-0.03
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
P-Value for Diff.	[0.47]		[0.81]		[0.78]		[0.30]	
Any Medicaid Visit, cumulative								
Control Mean	0.37	0.32	0.34	0.35	0.43	0.29	0.33	0.37
Reg. Adj. Diff.	-0.01	-0.02	-0.01	-0.04	-0.08	0.03	-0.02	-0.02
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value for Diff.	[0.70]		[0.49]		[0.01]		[0.82]	
N - Control	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

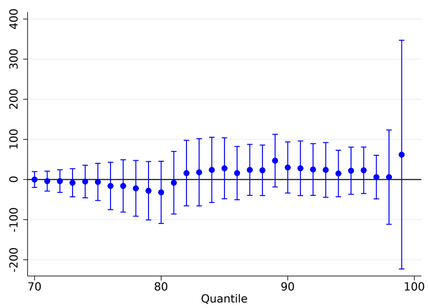
Notes: This table reports tests for heterogeneous treatment effects. Each outcome is measured one quarter after enrollment. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff." is based on running equation (1) for the outcome of interest on a treatment indicator, randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Similarly, we do not control for employment outcomes in the quarter prior to enrollment in columns (1)-(2). Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The *p*-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value for Diff.".

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

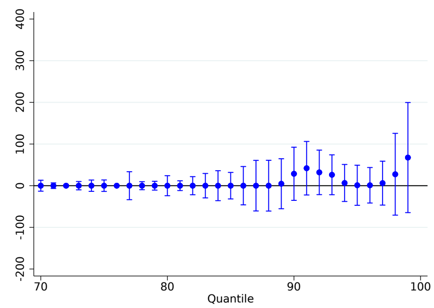
Online Appendix

Rebecca Brough, Matthew Freedman, and David C. Phillips

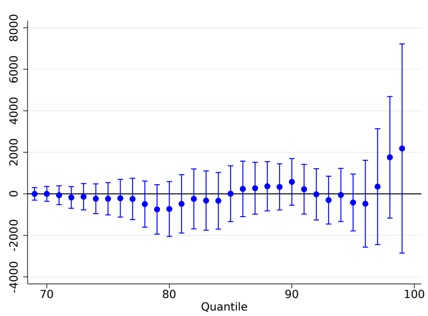
Figure A.1. Conditional Quantile Treatment Effects on Paid Hours and Earnings, One Quarter After Enrollment



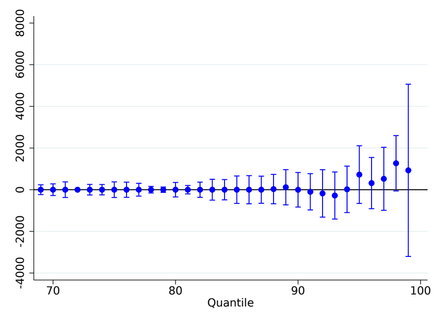
(a) Paid Hours Worked,
Simple Diff.



(b) Paid Hours Worked,
Regression-Adjusted Diff.



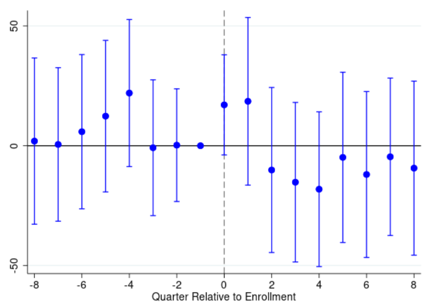
(c) Earnings, Simple Diff.



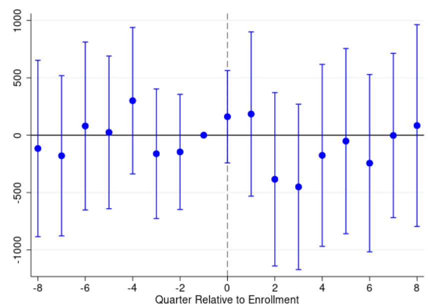
(d) Earnings,
Regression-Adjusted Diff.

Notes: This figure depicts conditional quantile treatment effects and 95% confidence intervals on paid hours worked and earnings. Panels (a)-(d) estimate conditional quantile treatment effects on hours worked (panels (a) and (b)) and earnings (panels (c) and (d)) in the first quarter after enrollment. Panels (a) and (c) are conditional on randomization regime alone; panels (b) and (d) are conditional on 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. The horizonal axis represents the quantile being estimated. Estimates are not generated for quantiles less than 70 (69) for hours (earnings) since these estimates are identically zero. Standard errors are robust. The vertical lines represent 95% confidence intervals.

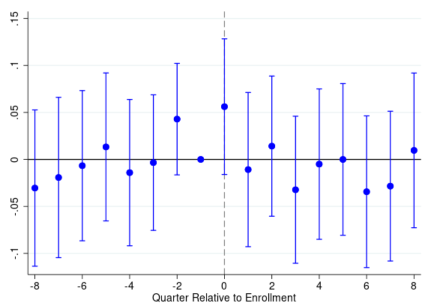
Figure A.2. Treatment Effects on Employment Outcomes, by Relative Time – Cohort 1



(a) Quarterly Paid Hours Worked



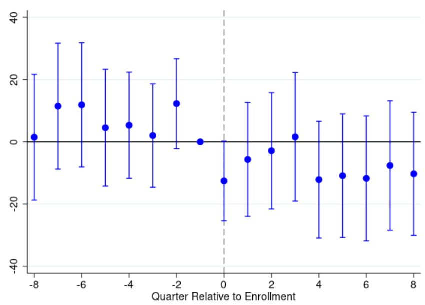
(b) Quarterly Earnings



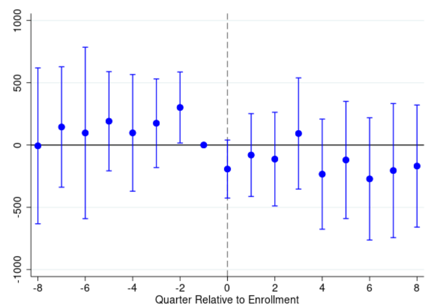
(c) Any Paid Employment

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

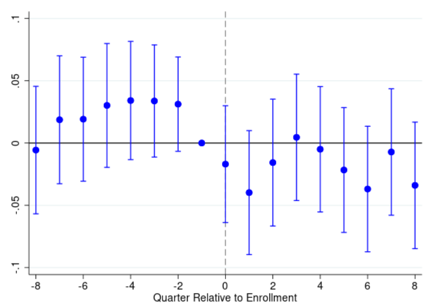
Figure A.3. Treatment Effects on Employment Outcomes, by Relative Time – Cohort 2



(a) Quarterly Paid Hours Worked



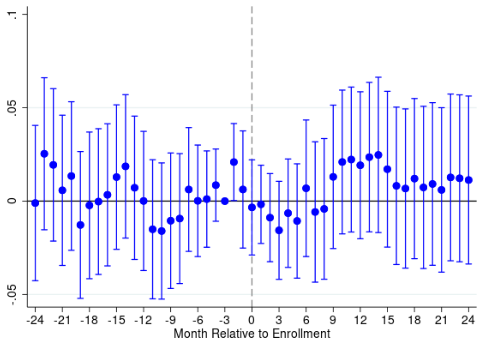
(b) Quarterly Earnings



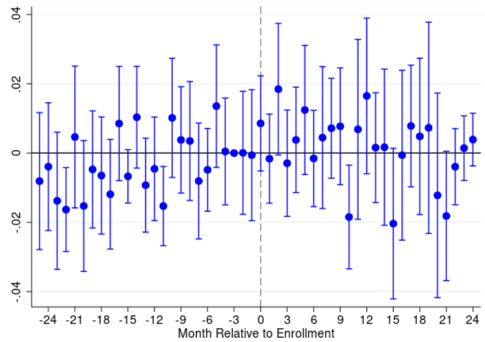
(c) Any Paid Employment

Notes: This figure depicts treatment effects on (a) paid hours worked, (b) earnings, and (c) any paid employment over time for cohort 2 participants. Each dot measures the treatment effect of receiving free public transit at the relative quarter indicated on the horizontal axis. Each treatment effect is measured as a regression-adjusted difference in means from a separate regression, as specified in equation (1). Outcomes are measured using Washington State UI records. Control variables are the outcome in the period prior to random assignment as well as indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment; participant age is not available in the state administrative records. The vertical lines represent 95% confidence intervals, computed using heteroskedasticity-robust standard errors.

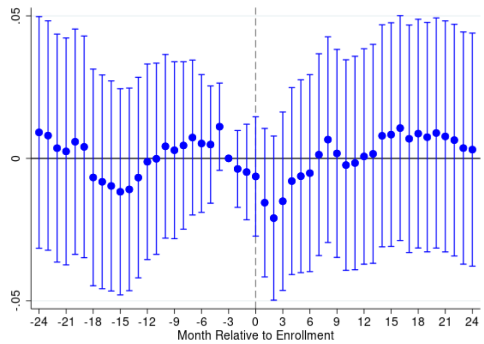
Figure A.4. Treatment Effects on Secondary Outcomes, by Relative Time



(a) Any public benefits



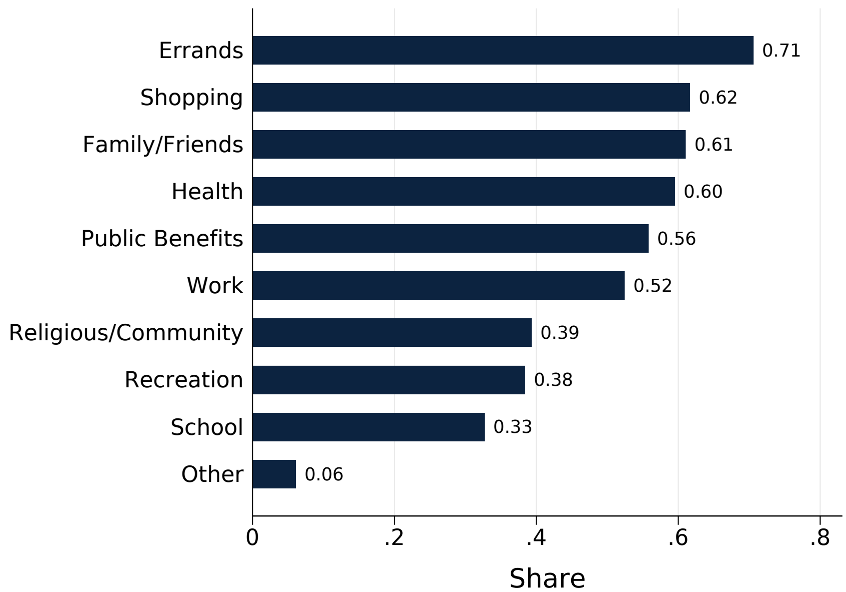
(b) Any move



(c) Any arrest, cumulative

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

Figure A.5. 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 (99%) 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.

Table A.1. Description of Outcome Variables

	Source	Frequency	Description
Primary Outcomes			
Paid hours worked	RDA, ESD	Quarterly	Sum of hours across all UI-covered jobs in WA
Earnings	RDA, ESD	Quarterly	Sum of earnings across all UI-covered jobs in WA
Any earnings	RDA, ESD	Quarterly	Indicator for any positive earnings
Job gain	RDA, ESD	Quarterly	Indicator for switching from zero earnings to positive earnings between two periods
Job loss	RDA, ESD	Quarterly	Indicator for switching from positive earnings to zero earnings between two periods
Continuous employment	RDA, ESD	Quarterly	Indicator for continuous positive earnings between two periods
Continuous sector employment	RDA, ESD	Quarterly	Indicator for continuous employment within a 2-digit NAICS code sector between two periods
Continuous industry employment	RDA, ESD	Quarterly	Indicator for continuous employment within a 6-digit NAICS code industry between two periods
Continuous unemployment	RDA, ESD	Quarterly	Indicator for continuous zero earnings between two periods
Secondary Outcomes			
Any food or cash benefits	RDA, DSHS	Monthly	Indicator for individual receiving DSHS services
SNAP†	RDA, DSHS	Monthly	Indicator for individual receiving SNAP benefits
TANF‡	RDA, DSHS	Monthly	Indicator for individual receiving TANF benefits
Other benefits§	RDA, DSHS	Monthly	Indicator for individual receiving Aged, Blind, Disabled (ABD) benefits or being eligible for Housing and Essential Needs (HEN)
Balance in collections	Experian	Quarterly*	Total current balance in debt collections
Credit Score	Experian	Quarterly*	VantageScore 4.0 credit score
Total inquiries in last 3 months	Experian	Quarterly*	Count of credit inquiries made in past 3 months
Any arrest, cumulative	RDA, WSP	Monthly	Indicator for any arrest made in WA measured cumulatively since study enrollment month in post period measured retrospectively from study enrollment month in pre period
Any misdemeanor, cumulative	RDA, WSP	Monthly	Indicator for any misdemeanor charge in WA measured cumulatively since study enrollment month in post period measured retrospectively from study enrollment month in pre period
Any gross misdemeanor, cumulative	RDA, WSP	Monthly	Indicator for any gross misdemeanor charge in WA measured cumulatively since study enrollment month in post period measured retrospectively from study enrollment month in pre period
Any felony, cumulative	RDA, WSP	Monthly	Indicator for any felony charge in WA measured cumulatively since study enrollment month in post period measured retrospectively from study enrollment month in pre period
Cost to Medicaid, cumulative	RDA, HCA	Monthly	Estimate of cost to Medicaid, based on cumulative Medicaid health visits Costs are based on Finkelstein et al. (2012) Each Non-ER Inpatient visit costs $7523 Each ER Inpatient visit costs $7688 Each ER Outpatient visit costs $435 Each Non-ER Outpatient visit costs $150
Any Medicaid visit, cumulative	RDA, HCA	Monthly	indicator for any Medicaid health visit measured cumulatively since study enrollment month in post period measures retrospectively from study enrollment month in pre period
– Emergency outpatient	RDA, HCA	Monthly	Indicator for any emergency outpatient Medicaid health visit
– Emergency inpatient	RDA, HCA	Monthly	Indicator for any emergency inpatient Medicaid health visit
– Nonemergency outpatient	RDA, HCA	Monthly	Indicator for any nonemergency outpatient Medicaid health visit
– Nonemergency inpatient	RDA, HCA	Monthly	Indicator for any nonemergency inpatient Medicaid health visit
Any move	Infutor	Monthly	Indicator for any address change between two months
Any move in state	Infutor	Monthly	Indicator for address state change between two months
Any move out of state	Infutor	Monthly	Indicator for an address change out of state between two months
Any move in county	Infutor	Monthly	Indicator for an address within county between two months
Any move out of county	Infutor	Monthly	Indicator for an address change out of county between two months

Notes: RDA denotes Research and Data Analysis Department at the Washington Department of Social and Health Services. ESD denotes Washington State Employment Security Department. DSHS denotes Washington Department of Social and Health Services. WSP denotes Washington State Patrol. HCA denotes Washington Health Care Authority.

†The Supplemental Nutrition Assistance Program (SNAP) provides individuals and families with low incomes monthly benefits that can be used to buy food.

‡Temporary Assistance for Needy Families (TANF) offers temporary cash assistance to children and families in need.

§Other benefits include Washington's Aged, Blind or Disabled Cash Assistance Program (which 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) and Washington's Housing and Essential Needs Program (which 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).

*Experian records are monthly snapshots taken in March, June, September, and December.

Table A.2. Mean Baseline Characteristics by Treatment Assignment

	(1)	(2)	(3)	(4)	(5)
	Control		**Treatment**		**Simple Reg. Adj. Diff.**
	Mean	N	Mean	N	Coef. Est. (SE)
Demographics at Baseline					
Age at enrollment	39.66	1105	40.88	692	1.05 (0.63)
White	0.41	1105	0.39	692	-0.03 (0.02)
Black	0.29	1105	0.29	692	0.00 (0.02)
Hispanic	0.09	1105	0.08	692	-0.01 (0.01)
Asian	0.03	1105	0.05	692	0.02 (0.01)
American Indian	0.01	1105	0.01	692	0.00 (0.01)
Pacific Islander	0.02	1105	0.03	692	0.01 (0.01)
Multi-racial	0.05	1105	0.05	692	0.01 (0.01)
Missing race	0.04	1105	0.03	692	-0.01 (0.01)
Transit Use at Baseline					
Used transit at all in past 30 days	0.88	1105	0.88	692	0.01 (0.02)
No. days used transit in 30 days prior to enrollment	15.10	1105	15.94	692	1.00 (0.53)
Enrollment Location					
Auburn CSO	0.11	1105	0.08	692	-0.02 (0.01)
Belltown CSO	0.08	1105	0.11	692	0.02 (0.01)
Capitol Hill CSO	0.14	1105	0.12	692	-0.02 (0.02)
Federal Way CSO	0.01	1105	0.02	692	0.01 (0.01)
King East CSO	0.04	1105	0.04	692	-0.01 (0.01)
King South CSO	0.01	1105	0.01	692	-0.002 (0.005)
North Seattle CSO	0.04	1105	0.03	692	-0.01 (0.01)
Rainier CSO	0.02	1105	0.01	692	-0.01 (0.01)
Renton CSO	0.08	1105	0.09	692	0.01 (0.01)
White Center CSO	0.47	1105	0.50	692	0.03 (0.02)

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

Table A.3. Employment Outcomes, One Quarter After Study Enrollment – Cohort 1

	(1) **Control**	(2) **Treatment**	(3) **Simple Reg. Adj. Diff.**	(4) **Reg. Adj. Diff.**
	Mean		Coef. Est. (SE)	
Paid hours worked	89	118	29.0	18.6
			(20.1)	(17.9)
Earnings ($)	1773	2119	346	184.46
			(409)	(365)
Any paid employment	0.35	0.36	.0067	-0.01
			(0.047)	(0.042)
Job gain	.14	0.12	-0.022	-0.02
			(0.033)	(0.034)
Job loss	.11	0.1	-0.011	-0.01
			(0.03)	(0.031)
Continuous employment	0.21	0.24	0.029	0.02
			(0.04)	(0.04)
–Cont. sector employment	0.13	0.16	0.033	0.03
			(0.035)	(0.035)
–Cont. industry employment	0.11	0.13	0.017	0.01
			(0.032)	(0.032)
Continuous unemployment	0.54	0.54	0.0047	0.02
			(0.049)	(0.049)
N	300	157		

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

Table A.4. Employment Outcomes, One Quarter After Study Enrollment – Cohort 2

	(1) **Control**	(2) **Treatment**	(3) **Simple Reg. Adj. Diff.**	(4) **Reg. Adj. Diff.**
	Mean		Coef. Est. (SE)	
Paid hours worked	71	69	-3.0	-5.7
			(9.6)	(9.3)
Earnings ($)	1321	1260	-62	-80
			(175)	(170)
Any paid employment	0.31	0.27	-0.03	-0.04
			(0.028)	(0.025)
Job gain	0.13	0.1	-.029	-0.03
			(0.019)	(0.019)
Job loss	0.15	0.17	.018	0.02
			(0.022)	(0.022)
Continuous employment	0.18	0.17	-0.005	-0.005
			(0.023)	(0.023)
–Cont. sector employment	0.14	0.13	-0.008	-0.01
			(0.02)	(0.02)
–Cont. industry employment	0.11	0.1	-0.001	0.004
			(0.018)	(0.018)
Continuous unemployment	0.54	0.56	0.016	0.02
			(0.03)	(0.03)
N	677	464		

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

Table A.5. Secondary Outcomes, One Quarter After Enrollment - Cohort 1

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		**Treatment**		**Simple Reg. Adj. Diff.**	**Reg. Adj. Diff.**
	Mean	N	Mean	N	Coef. Est. (SE)	
A. Public Assistance Receipt, measured three months post enrollment						
Any food or cash benefits	0.92	300	0.92	157	-0.01	-0.0002
					(0.03)	(0.03)
–SNAP	0.89	300	0.90	157	0.01	0.02
					(0.03)	(0.03)
–TANF	0.02	300	0.02	157	0.002	0.001
					(0.01)	(0.01)
–Other	0.10	300	0.09	157	-0.01	-0.02
					(0.03)	(0.02)
B. Financial Health, measured in the third month of the quarter post enrollment						
Balance in collection	2,148.84	125	1,935.40	73	-213.44	-227.61
					(585.51)	(598.05)
Credit score	513.44	125	479.26	73	-34.18	-10.56
					(27.25)	(17.87)
Total inquiries in past 3 months	0.39	125	0.22	73	-0.17	-0.13
					(0.08)	(0.08)
C. Criminal Justice, measured three months post enrollment						
Any arrest, cumulative	0.19	300	0.14	157	-0.05	-0.003
					(0.04)	(0.03)
–Any misdemeanor, cumulative	0.02	300	0.02	157	-0.004	-0.001
					(0.01)	(0.01)
–Any gross misdemeanor, cumulative	0.05	300	0.06	157	0.01	0.02
					(0.02)	(0.02)
–Any felony, cumulative	0.09	300	0.08	157	-0.01	-0.0003
					(0.03)	(0.03)
D. Healthcare, measured three months post enrollment						
Cost to Medicaid, cumulative	1,256.71	300	684.29	157	-572.41	-655.95
					(269.54)	(268.76)
Any Medicaid Visit, cumulative	0.40	300	0.26	157	-0.14	-0.13
					(0.05)	(0.04)
–Emergency outpatient	0.30	300	0.22	157	-0.08	-0.08
					(0.04)	(0.04)
–Emergency inpatient	0.05	300	0.04	157	-0.01	-0.02
					(0.02)	(0.02)
–Non-emergency outpatient	0.34	300	0.24	157	-0.10	-0.09
					(0.04)	(0.04)
–Non-emergency inpatient	0.04	300	0.01	157	-0.03	-0.03
					(0.01)	(0.01)
E. Residential Mobility, measured three months post enrollment						
Any move	0.014	140	0.000	76	-0.014	-0.017
					(0.010)	(0.012)
–Any move in state	0.007	140	0.000	76	-0.007	-0.009
					(0.007)	(0.009)
–Any move out of state	0.007	140	0.000	76	-0.007	-0.008
					(0.007)	(0.008)
–Any move in county	0.007	140	0.000	76	-0.007	-0.009
					(0.007)	(0.009)
–Any move out of county	0.007	140	0.000	76	-0.007	-0.008
					(0.007)	(0.008)

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

Table A.6. Secondary Outcomes, One Quarter After Enrollment - Cohort 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Control		**Treatment**		**Simple Reg. Adj. Diff.**	**Reg. Adj. Diff.**
	Mean	N	Mean	N	Coef. Est. (SE)	
A. Public Assistance Receipt, measured three months post enrollment						
Any food or cash benefits	0.94	677	0.91	464	-0.02	-0.02
					(0.02)	(0.02)
–SNAP	0.92	677	0.89	464	-0.04	-0.03
					(0.02)	(0.02)
–TANF	0.03	677	0.04	464	0.01	0.002
					(0.01)	(0.01)
–Other	0.15	677	0.12	464	-0.02	-0.01
					(0.02)	(0.02)
B. Financial Health, measured in the third month of the quarter post enrollment						
Balance in collection	1,442.22	367	1,203.80	261	-228.91	20.68
					(222.19)	(122.94)
Credit score	496.51	367	523.70	261	22.69	21.20
					(15.73)	(11.15)
Total inquiries in past 3 months	0.33	367	0.27	261	-0.07	-0.06
					(0.04)	(0.05)
C. Criminal Justice, measured three months post enrollment						
Any arrest, cumulative	0.11	677	0.10	464	-0.01	-0.01
					(0.02)	(0.02)
–Any misdemeanor, cumulative	0.01	677	0.01	464	-0.001	-0.0003
					(0.01)	(0.01)
–Any gross misdemeanor, cumulative	0.05	677	0.04	464	-0.01	-0.01
					(0.01)	(0.01)
–Any felony, cumulative	0.04	677	0.04	464	0.001	-0.0004
					(0.01)	(0.01)
D. Healthcare, measured three months post enrollment						
Cost to Medicaid, cumulative	850.47	677	989.93	464	157.79	134.95
					(220.18)	(204.75)
Any Medicaid Visit, cumulative	0.32	677	0.29	464	-0.03	-0.03
					(0.03)	(0.03)
–Emergency outpatient	0.22	677	0.20	464	-0.02	-0.01
					(0.02)	(0.02)
–Emergency inpatient	0.04	677	0.03	464	-0.01	-0.01
					(0.01)	(0.01)
–Non-emergency outpatient	0.28	677	0.24	464	-0.04	-0.04
					(0.03)	(0.03)
–Non-emergency inpatient	0.02	677	0.02	464	0.01	0.01
					(0.01)	(0.01)
E. Residential Mobility, measured three months post enrollment						
Any move	0.010	292	0.014	214	0.002	0.001
					(0.010)	(0.010)
–Any move in state	0.007	292	0.014	214	0.007	0.006
					(0.009)	(0.009)
–Any move out of state	0.003	292	0.005	214	-0.001	-0.001
					(0.006)	(0.006)
–Any move in county	0.003	292	0.014	214	0.010	0.009
					(0.008)	(0.008)
–Any move out of county	0.007	292	0.005	214	-0.004	-0.004
					(0.007)	(0.007)

Notes: This table presents means and regression-adjusted differences in means for outcomes measured in the quarter after enrollment for cohort 2. Public assistance receipt comes from Washington State Economic Services Administration records and is measured 3 months after random assignment. Financial measures cover the sample that matches to a repeated cross-section of quarterly Experian credit reports and reflect outcomes measured 1 quarter after random assignment. Criminal justice contact measures come from Washington State Patrol records and are measured cumulatively between random assignment and three months later. Healthcare information come from Washington State administrative records on Medicaid claims and is also measured cumulatively between random assignment and 3 months later; cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Residential moves cover a sample that matches to any address from Infutor consumer reference data prior to random assignment; moves are measured cumulatively between random assignment and 3 months later. Column (5) presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Column (6) additionally adjusts for indicators for race, month of study enrollment, and the relevant outcome 1 quarter prior to study enrollment; results in Panels A, C, and D also include controls for gender; results in Panels B and E control for age and age squared. Heteroskedasticity-robust standard errors are reported in parentheses.

Table A.7. State Administrative Outcomes, Panel Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Medical						Benefits				Criminal Justice
	Cost to Medicaid Monthly	Any Medicaid visit Monthly	Emergency outpatient	Emergency inpatient	Non-emergency inpatient	Non-emergency outpatient	Any food or cash benefits	SNAP	TANF	Other	Any Arrest
Treated	-18	-0.014	-0.003	-0.001	-0.0003	-0.012	-0.006	0.001	-0.001	-0.004	-0.014
	(41)	(0.010)	(0.008)	(0.003)	(0.002)	(0.010)	(0.018)	(0.018)	(0.006)	(0.011)	(0.006)
Person Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Calendar Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Relative Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	142	0.089	0.052	0.008	0.003	0.072	0.620	0.506	0.025	0.055	0.030
Observations	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302
Individuals	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598

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

Table A.8. Financial Health Outcomes, Panel Regressions

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

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

Table A.9. Criminal Justice Outcomes, One Quarter After Enrollment

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

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

Table A.10. Residential Mobility Outcomes, Panel Regressions

	(1)	(2)	(3)	(4)	(5)
	Any Move	Any Move In WA	Any Move Outside WA	Any Move In King County	Any Move Outside King County
Treated	0.006	0.0003	0.006	0.001	0.006
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)
Person Fixed Effects	✓	✓	✓	✓	✓
Calendar Month Fixed Effects	✓	✓	✓	✓	✓
Relative Month Fixed Effects	✓	✓	✓	✓	✓
Control Mean	0.014	0.011	0.003	0.009	0.006
Observations	34,790	34,790	34,790	34,790	34,790
Individuals	710	710	710	710	710

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

Table A.11. Heterogeneity Tests for Selected Outcomes, One Quarter After Enrollment, No Controls

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-white	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hours worked								
Control Mean	42.51	118.19	82.71	68.41	51.18	95.61	80.94	70.85
Reg Adj. Diff.	-4.47	11.67	7.96	2.89	2.70	6.58	-2.86	18.02
SE	(9.08)	(15.29)	(12.39)	(11.99)	(9.85)	(13.38)	(11.25)	(13.42)
P-Value of Diff.	[0.36]		[0.77]		[0.82]		[0.26]	
Employed								
Control Mean	0.17	0.51	0.32	0.33	0.24	0.38	0.32	0.32
Reg Adj. Diff.	-0.04	-0.03	-0.03	-0.01	-0.01	-0.04	-0.04	-0.002
SE	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.83]		[0.75]		[0.48]		[0.47]	
Any public benefits								
Control Mean	0.94	0.92	0.93	0.94	0.96	0.91	0.93	0.93
Reg Adj. Diff.	-0.01	0.01	-0.01	0.01	-0.01	0.001	-0.01	0.003
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
P-Value of Diff.	[0.49]		[0.55]		[0.71]		[0.67]	
Any arrest, cumulative								
Control Mean	0.17	0.09	0.15	0.12	0.14	0.13	0.18	0.07
Reg Adj. Diff.	-0.02	-0.01	-0.03	-0.01	-0.02	-0.02	-0.01	-0.03
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
P-Value of Diff.	[0.72]		[0.46]		[0.78]		[0.37]	
Any Medicaid visit, cumulative								
Control Mean	0.37	0.32	0.34	0.35	0.43	0.29	0.33	0.37
Reg Adj. Diff.	-0.02	-0.03	-0.02	-0.03	-0.09	0.03	-0.02	-0.03
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.78]		[0.92]		[0.01]		[0.79]	
N - Control Mean	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

Notes: This table reports tests for heterogeneous treatment effects. Each outcome is measured one quarter after enrollment. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in row "Reg. Adj. Diff." is based on a regression of the outcome of interest on a treatment indicator and randomization regime for the listed sub-group. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in row "P-Value of Diff.".

Table A.12. Employment Outcomes, Heterogeneity, With Controls

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-white	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hours worked in relative quarter 1								
Control Mean	42.51	118.19	82.71	68.41	51.18	95.61	80.94	70.85
Reg Adj. Diff.	-4.53	8.00	5.26	-1.78	3.74	1.54	-5.35	9.49
SE	(9.08)	(14.86)	(11.77)	(11.49)	(9.31)	(12.68)	(10.93)	(13.42)
P-Value of Diff.	[0.47]		[0.67]		[0.75]		[0.39]	
Earnings in relative quarter 1								
Control Mean	765.22	2296.13	1534.72	1351.62	971.76	1816.45	1564.68	1306.19
Reg Adj. Diff.	-96.54	31.35	129.28	-119.38	-34.02	2.44	-211.09	281.61
SE	(156.47)	(291.64)	(226.38)	(224.57)	(169.18)	(235.08)	(196.05)	(279.70)
P-Value of Diff.	[0.70]		[0.44]		[0.94]		[0.15]	
Employed in relative quarter 1								
Control Mean	0.17	0.51	0.32	0.33	0.24	0.38	0.32	0.32
Reg Adj. Diff.	-0.04	-0.04	-0.03	-0.04	-0.004	-0.05	-0.04	-0.02
SE	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[1.00]		[0.85]		[0.20]		[0.65]	
Cont. employment between relative quarter -1 and 1								
Control Mean	0.00	0.42	0.18	0.21	0.15	0.22	0.17	0.21
Reg Adj. Diff.	*	-0.03	0.001	0.001	-0.02	0.02	-0.03	0.04
SE	*	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
P-Value of Diff.	[0.47]		[1.00]		[0.37]		[0.11]	
–Cont. sector employment between relative quarter -1 and 1								
Control Mean	0.01	0.29	0.12	0.15	0.10	0.16	0.12	0.16
Reg Adj. Diff.	0.005	-0.01	-0.01	0.02	0.02	-0.003	-0.02	0.04
SE	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.58]		[0.58]		[0.44]		[0.11]	
–Cont. Industry Employment between relative quarter -1 and 1								
Control Mean	0.00	0.24	0.09	0.13	0.08	0.12	0.09	0.13
Reg Adj. Diff.	*	-0.002	0.004	0.002	0.003	0.01	-0.01	0.03
SE	*	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.96]		[0.95]		[1.00]		[0.25]	
Job gain between relative quarter -1 and 1								
Control Mean	0.17	0.09	0.14	0.12	0.09	0.16	0.15	0.11
Reg Adj. Diff.	-0.04	-0.01	-0.03	-0.02	0.01	-0.06	-0.02	-0.04
SE	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.42]		[0.79]		[0.04]		[0.51]	
Job loss between relative quarter -1 and 1								
Control Mean	0.00	0.30	0.14	0.13	0.15	0.13	0.14	0.14
Reg Adj. Diff.	*	0.01	-0.01	0.04	-0.001	0.02	0.01	0.004
SE	*	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.82]		[0.26]		[0.55]		[0.83]	
Cont. unemployment between relative quarter -1 and 1								
Control Mean	0.83	0.19	0.54	0.54	0.60	0.49	0.54	0.54
Reg Adj. Diff.	0.04	0.03	0.04	-0.02	0.01	0.02	0.04	-0.001
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)
P-Value of Diff.	[0.84]		[0.33]		[0.82]		[0.48]	
N - Control Mean	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

Notes: This table reports tests for heterogeneous treatment effects on various employment measures from Washington State UI records. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling for randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Similarly, we do not control for employment outcomes in the quarter prior to enrollment in columns 1-4. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The p-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.". Identically zero estimates are denoted by "*".

Table A.13. Employment Outcomes, Heterogeneity, No Controls

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-white	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hours worked in relative qtr 1								
Control Mean	42.51	118.19	82.71	68.41	51.18	95.61	80.94	70.85
Reg Adj. Diff.	-4.47	11.67	7.96	2.89	2.70	6.58	-2.86	18.02
SE	(9.08)	(15.29)	(12.39)	(11.99)	(9.85)	(13.38)	(11.25)	(14.68)
P-Value of Diff.	[0.36]		[0.77]		[0.82]		[0.26]	
Earnings in relative qtr 1								
Control Mean	765.22	2296.13	1534.72	1351.62	971.76	1816.45	1564.68	1306.19
Reg Adj. Diff.	-101.48	112.45	134.52	-72.36	-54.17	96.94	-159.56	353.21
SE	(160.25)	(298.96)	(233.02)	(235.02)	(180.27)	(256.77)	(204.65)	(294.99)
P-Value of Diff.	[0.53]		[0.53]		[0.63]		[0.15]	
Employed in relative qtr 1								
Control Mean	0.17	0.51	0.32	0.33	0.24	0.38	0.32	0.32
Reg Adj. Diff.	-0.04	-0.03	-0.03	-0.01	-0.01	-0.04	-0.04	-0.002
SE	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
P-Value of Diff.	[0.83]		[0.75]		[0.48]		[0.47]	
Cont. employment between relative quarter -1 and 1								
Control Mean	0.00	0.42	0.18	0.21	0.15	0.22	0.17	0.21
Reg Adj. Diff.	*	-0.02	0.005	0.003	-0.02	0.02	-0.02	0.04
SE	*	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
P-Value of Diff.	[0.61]		[0.98]		[0.35]		[0.18]	
–Cont. sector employment between relative quarter -1 and 1								
Control Mean	0.01	0.29	0.12	0.15	0.10	0.16	0.12	0.16
Reg Adj. Diff.	0.003	-0.02	-0.00	0.01	0.02	-0.01	-0.02	0.04
SE	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.59]		[0.67]		[0.44]		[0.12]	
–Cont. industry employment between relative quarter -1 and 1								
Control Mean	0.00	0.24	0.09	0.13	0.08	0.12	0.09	0.13
Reg Adj. Diff.	*	-0.01	0.01	0.001	0.003	0.003	-0.01	0.03
SE	*	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.83]		[0.89]		[1.00]		[0.29]	
Job gain between relative quarter -1 and 1								
Control Mean	0.17	0.09	0.14	0.12	0.09	0.16	0.15	0.11
Reg Adj. Diff.	-0.04	-0.01	-0.03	-0.02	0.01	-0.06	-0.02	-0.04
SE	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.38]		[0.62]		[0.04]		[0.52]	
Job loss between relative quarter -1 and 1								
Control Mean	0.00	0.30	0.14	0.13	0.15	0.13	0.14	0.14
Reg Adj. Diff.	*	0.002	-0.01	0.04	-0.002	0.02	0.01	0.01
SE	*	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.99]		[0.28]		[0.57]		[0.98]	
Cont. unemployment between relative quarter -1 and 1								
Control Mean	0.83	0.19	0.54	0.54	0.60	0.49	0.54	0.54
Reg Adj. Diff.	0.04	0.03	0.03	-0.02	0.01	0.02	0.03	-0.01
SE	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)
P-Value of Diff.	[0.80]		[0.29]		[0.83]		[0.49]	
N - Control Mean	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

Notes: This table reports tests for heterogeneous treatment effects on various employment measures from Washington State UI records. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns are calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in the row "P-Value of Diff.". Identically zero estimates are denoted by "*".

Table A.14. Public Benefits, Health, and Criminal Justice Outcomes, Heterogeneity, With Controls

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-white	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any food or cash benefits								
Control Mean	0.94	0.92	0.93	0.94	0.96	0.91	0.93	0.93
Reg Adj. Diff	-0.01	0.01	-0.01	0.01	0.0001	-0.004	-0.003	0.01
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
P-Value of Diff.	[0.28]		[0.45]		[0.91]		[0.61]	
SNAP								
Control Mean	0.92	0.90	0.90	0.92	0.94	0.89	0.92	0.90
Reg Adj. Diff	-0.01	0.01	-0.01	0.01	0.01	-0.01	0.001	0.002
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
P-Value of Diff.	[0.42]		[0.45]		[0.73]		[0.96]	
TANF								
Control Mean	0.01	0.03	0.02	0.02	0.01	0.03	0.01	0.05
Reg Adj. Diff	0.003	-0.01	0.01	-0.01	-0.01	0.003	-0.01	0.01
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.005)	(0.02)
P-Value of Diff.	[0.49]		[0.25]		[0.46]		[0.44]	
Other benefits								
Control Mean	0.16	0.09	0.12	0.15	0.16	0.11	0.15	0.11
Reg Adj. Diff	-0.00	-0.03	-0.01	-0.02	-0.04	-0.004	-0.02	0.003
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.37]		[0.73]		[0.33]		[0.33]	
Cost to Medicaid, cumulative								
Control Mean	982.32	966.65	1003.86	934.24	1216.11	798.81	916.96	1059.96
Reg Adj. Diff	293.19	-324.91	-103.55	141.92	27.82	-13.35	68.19	-112.47
SE	(173.41)	(135.83)	(126.22)	(208.01)	(204.22)	(123.35)	(153.22)	(146.12)
P-Value of Diff.	[0.01]		[0.31]		[1.00]		[0.39]	
Any Medicaid visit, cumulative								
Control Mean	0.37	0.32	0.34	0.35	0.43	0.29	0.33	0.37
Reg Adj. Diff	-0.01	-0.02	-0.01	-0.04	-0.08	0.03	-0.02	-0.02
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.70]		[0.49]		[0.01]		[0.82]	
–Emergency outpatient								
Control Mean	0.26	0.23	0.25	0.25	0.29	0.21	0.25	0.24
Reg Adj. Diff	0.01	-0.01	0.0001	-0.01	-0.05	0.03	0.01	-0.02
SE	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.55]		[0.73]		[0.03]		[0.35]	
–Emergency inpatient								
Control Mean	0.04	0.05	0.04	0.05	0.06	0.03	0.05	0.04
Reg Adj. Diff	0.02	-0.02	-0.005	-0.002	-0.01	0.004	-0.004	-0.0005
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.02]		[0.87]		[0.23]		[0.81]	
–Non-emergency inpatient								
Control Mean	0.02	0.02	0.02	0.03	0.03	0.02	0.01	0.04
Reg Adj. Diff	0.01	-0.01	0.003	0.01	0.01	-0.002	0.01	0.003
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.04]		[0.59]		[0.22]		[0.81]	
–Non-emergency outpatient								
Control Mean	0.31	0.29	0.30	0.29	0.37	0.24	0.28	0.33
Reg Adj. Diff	-0.02	-0.02	-0.01	-0.04	-0.07	0.02	-0.004	-0.05
SE	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.94]		[0.42]		[0.05]		[0.27]	
Any arrest, cumulative								
Control Mean	0.17	0.09	0.15	0.12	0.14	0.13	0.18	0.07
Reg Adj. Diff	-0.02	-0.01	-0.02	-0.01	-0.02	-0.01	-0.003	-0.03
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
P-Value of Diff.	[0.47]		[0.81]		[0.78]		[0.30]	
N - Control Mean	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

Notes: This table reports tests for heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling for randomization regime, race, gender, month of enrollment, and the outcome variable in the quarter (3 months) prior to enrollment for the listed sub-group. Gender and race controls are omitted when we test for heterogeneity by race and gender, respectively. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns is calculated by regressing the outcome variable on the aforementioned controls (a), a treatment variable (b), an indicator for being in the even-numbered column (c), and the interaction of c with b and c with a. The p-value of the interaction of the treatment variable with the sub-group of interest is reported in row "P-Value of Diff.".

Table A.15. Public Benefits, Health, and Criminal Justice Outcomes, Heterogeneity, No Controls

	Employed at Baseline		**Subsidy Duration above Median**		**Race**		**Gender**	
	No	Yes	No	Yes	White	Non-white	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any food or cash benefits								
Control Mean	0.94	0.92	0.93	0.94	0.96	0.91	0.93	0.93
Reg Adj. Diff.	-0.01	0.01	-0.01	0.01	-0.01	0.001	-0.01	0.003
SE	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
P-Value of Diff.	[0.49]		[0.55]		[0.71]		[0.67]	
SNAP								
Control Mean	0.92	0.90	0.90	0.92	0.94	0.89	0.92	0.90
Reg Adj. Diff.	-0.01	0.01	-0.01	0.01	0.001	-0.001	-0.004	0.002
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
P-Value of Diff.	[0.53]		[0.58]		[0.94]		[0.84]	
TANF								
Control Mean	0.01	0.03	0.02	0.02	0.01	0.03	0.01	0.05
Reg Adj. Diff.	0.01	-0.01	0.02	-0.01	-0.01	0.01	-0.002	0.02
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
P-Value of Diff.	[0.20]		[0.10]		[0.23]		[0.36]	
Other benefits								
Control Mean	0.16	0.09	0.12	0.15	0.16	0.11	0.15	0.11
Reg Adj. Diff.	-0.02	-0.02	-0.01	-0.03	-0.05	-0.001	-0.03	-0.01
SE	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.91]		[0.65]		[0.17]		[0.63]	
Cost to Medicaid, cumulative								
Control Mean	982.32	966.65	1003.86	934.24	1216.11	798.81	916.96	1059.96
Reg Adj. Diff.	291.56	-289.33	-97.06	192.97	30.68	11.68	97.62	-112.21
SE	(179.57)	(135.44)	(127.97)	(211.28)	(214.01)	(122.09)	(160.53)	(147.23)
P-Value of Diff.	[0.01]		[0.24]		[0.94]		[0.34]	
Any Medicaid visit, cumulative								
Control Mean	0.37	0.32	0.34	0.35	0.43	0.29	0.33	0.37
Reg Adj. Diff.	-0.02	-0.03	-0.02	-0.03	-0.09	0.03	-0.02	-0.03
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
P-Value of Diff.	[0.78]		[0.92]		[0.01]		[0.79]	
–Emergency outpatient								
Control Mean	0.26	0.23	0.25	0.25	0.29	0.21	0.25	0.24
Reg Adj. Diff.	0.01	-0.02	0.0001	-0.01	-0.05	0.03	0.01	-0.02
SE	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
P-Value of Diff.	[0.45]		[0.89]		[0.04]		[0.50]	
–Emergency inpatient								
Control Mean	0.04	0.05	0.04	0.05	0.06	0.03	0.05	0.04
Reg Adj. Diff.	0.01	-0.02	-0.004	-0.0003	-0.01	0.01	-0.003	-0.001
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.03]		[0.85]		[0.26]		[0.86]	
–Non-emergency inpatient								
Control Mean	0.02	0.02	0.02	0.03	0.03	0.02	0.01	0.04
Reg Adj. Diff.	0.01	-0.01	0.002	0.01	0.01	-0.001	0.01	0.002
SE	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
P-Value of Diff.	[0.05]		[0.60]		[0.20]		[0.70]	
–Non-emergency outpatient								
Control Mean	0.31	0.29	0.30	0.29	0.37	0.24	0.28	0.33
Reg Adj. Diff.	-0.03	-0.03	-0.02	-0.03	-0.08	0.01	-0.01	-0.05
SE	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
P-Value of Diff.	[0.99]		[0.80]		[0.02]		[0.39]	
Any arrest, cumulative								
Control Mean	0.17	0.09	0.15	0.12	0.14	0.13	0.18	0.07
Reg Adj. Diff.	-0.02	-0.01	-0.03	-0.01	-0.02	-0.02	-0.01	-0.03
SE	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
P-Value of Diff.	[0.72]		[0.46]		[0.78]		[0.37]	
N - Control Mean	534	443	575	402	413	564	579	398
N - Treatment	322	299	402	219	253	368	378	243

Notes: This table reports tests for heterogeneous treatment effects on benefits use, health, and criminal justice outcomes. Each outcome is measured 3 months post enrollment. Employed at baseline is defined as ever having positive UI-covered earnings in the 4 quarters pre-enrollment. Subsidy duration is based on the length of anticipated time between card receipt and subsidy expiration. All other variables are defined as before. The coefficient reported in the row "Reg. Adj. Diff" is the estimated treatment effect from equation (1), controlling only for randomization regime. Heteroskedasticity-robust standard errors are reported in parentheses. The difference in treatment effects between pairs of columns are calculated by regressing the outcome variable on the randomization regime, a treatment variable, an indicator for being in the even numbered column, and the interaction of these last two variables. The p-value of the interaction term is reported in the row "P-Value of Diff.".

Table A.16. Financial Health, Heterogeneity, With Controls

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

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

Table A.17. Financial Health, Heterogeneity, No Controls

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

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

Table A.18. Athey and Imbens (2016) Heterogeneity Tests

Outcome	Num. of Leaves	Leaf Categories (Y/N)	F-Stat	P-Value
Paid hours worked				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Earnings				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Employed for pay				
– 1 Qtr Post Enrollment	2	Ever eligible for Medicaid	0.1822	0.6696
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	6	Earnings prior to enrollment (x2) received benefits prior to enrollment (x2) hours worked prior to enrollment	1.02	0.404
Any arrest				
– 1 Qtr Post Enrollment	6	HS diploma; sex; received benefits prior to enrollment (x2); eligible for Medicaid prior to enrollment	1.5485	0.1727
– 2 Qtr Post Enrollment	7	Earnings prior to enrollment received benefits prior to enrollment (x3) any employment prior to enrollment eligible for medicaid prior to enrollment	0.36	0.90
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Any Medicaid visit				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	2	Any outpatient visit prior to enrollment	0.0211	0.8846
– 3 Qtrs Post Enrollment	2	Any outpatient visit prior to enrollment	0.0313	0.8596
Credit score				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Balance in collections				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA
Credit inquiries				
– 1 Qtr Post Enrollment	1	NA	NA	NA
– 2 Qtr Post Enrollment	1	NA	NA	NA
– 3 Qtrs Post Enrollment	1	NA	NA	NA

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

Table A.19. Most Common 4-Digit Industry Groups Before Enrollment, Treatment and Control Groups

Rank	Occupation	Share
1	Unemployed	0.66
2	Restaurants and Other Eating Places	0.065
3	Employment Services	0.059
4	Individual and Family Services	0.016
5	Traveler Accommodation	0.015
6	Grocery Stores	0.012
7	Services to Buildings and Dwellings	0.012
8	General Merchandise Stores	0.010
9	Special Food Services	0.010
10	Investigation and Security Services	0.006
11	Building Finishing Contractors	0.006

Notes: This table depicts the share of study participants engaged in work in each of the 10 most common 4-digit NAICS industry groups, as well as unemployment, in the 12 quarter prior to study enrollment. The sample is limited to individuals who go through random assignment and match to any Washington State administrative record prior to study enrollment.

Table A.20. Follow-Up Survey Results

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

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