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

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

January 2023

Abstract

We study the effects of providing free public transit on the employment and well-being of people participating in public assistance programs. We take advantage of a randomized controlled trial that provided free public transportation to individuals with low income together with rich administrative data on employment, public assistance receipt, consumer credit, criminal justice system contact, health care utilization, and residential mobility. Providing free public transportation does not have a large effects on hours worked or other employment outcomes among individuals receiving public benefits. On the other hand, we find imprecisely measured positive impacts on indicators of financial well-being and stronger evidence of beneficial effects on health.

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

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

1 Introduction

Cities across the globe have reduced public transit fares in recent years. Worldwide, at least 100 cities now provide public transportation with no fares at all (Barry and Rybus, 2020). Many of these cities are in Europe, but Kansas City and Washington, DC have also recently taken steps to eliminate fares on their transit systems. Efficiency gains from reducing pollution and congestion externalities associated with car travel as well as from taking advantage of returns to scale in transit motivate some of these changes. Just as often, however, equity considerations motivate transit fare reductions (Serebrisky et al., 2009). According to the 2019 U.S. Consumer Expenditure Survey, transportation was the second largest expenditure category for people with income in the bottom quintile, making up 16% of spending. Reducing transit fares for residents with limited means may benefit them economically through improved mobility and direct savings on transportation. As a result, reduced-fare programs in many places, including New York, San Francisco, and Seattle, are means-tested. However, there is little evidence on the downstream economic impacts of subsidized transit fares for populations with low incomes.

This paper studies the effects of free public transit fares on employment, public assistance receipt, credit access, health, and other outcomes among individuals with low income. We conduct a randomized controlled trial that enrolled 1,797 participants at public assistance offices in King County, Washington, which is the location of Seattle, in 2019 and early 2020. In the experiment, individuals in the treatment group received transit fare cards that provided up to six months of free public transit, passes that would otherwise cost about $200 to purchase. Individuals in the control group received the status quo means-tested transit fare card that provided reduced fares of $1.50 per bus ride. As detailed in a prior paper (Brough, Freedman and Phillips, 2022), access to free public transportation induced large changes in travel behavior, doubling travel by public transit. To measure the effects of fare-free public transit and resulting changes in travel on downstream outcomes, we link individuals in the experiment to rich administrative data from payroll tax, public assistance,

and criminal justice records as well as proprietary data on consumer credit and residential locations.

We first explore the effects of providing free public transit on a range of employment outcomes. We do not detect large effects of treatment on these outcomes. One quarter after random assignment, the treatment group works 1.6 more hours per quarter than the control group.[1] This gap is not statistically different from zero and relatively small. The 95% confidence interval excludes increases in hours worked greater than 4% of full-time employment. However, labor market attachment is low in our sample; the control group works only 77 hours per quarter (15% of full-time work), so the 95% confidence interval for hours worked includes effects up to 24% of the control group mean. 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, hours worked increased in the treatment group by no more than 3% of full-time work and 18% of the control group mean. Similarly, we do not observe large changes in employment rates, total earnings, wage rates, job transitions, or employment stability.

On the other hand, we find evidence that a zero price for transit improves well-being on other dimensions, including finances and health. Most notably, the treatment group uses less healthcare, visiting a healthcare provider within 3 months 5.6 percentage points less frequently, compared to a control group rate of 34.7%. Sub-sample survey responses suggest that this change reflects improved health rather than reduced access to healthcare. Less expensive non-emergency outpatient visits drive most of the decline in healthcare use, so improved health likely has limited impact on the cost to the state of providing healthcare. On the financial side, while we do not observe effects on employment or uptake of public benefits, we find some suggestive but imprecisely measured evidence of improved finances among a sub-sample of study participants who match to credit report data. Meanwhile,

[1]The state of Washington unemployment insurance system records more employment details than other states (Lachowska, Mas and Woodbury, 2020), so we can measure treatment effects on hours worked in addition to employment and earnings.

providing free public transit has no detectable effects on contact with the criminal justice system or the likelihood of moving residences. Overall, we find evidence that free fares for transit, and the travel it facilitates, improves welfare.

Our study contributes to several strands of literature. First, we extend the study of fare-free transit to outcomes beyond travel behavior. Earlier work exploiting the same experiment found that providing free public transportation significantly increased public transit use; the effect on overall mobility (including modes other than transit) was potentially large but less clear (Brough, Freedman and Phillips, 2022). Studies on the effects of free transit fares in other contexts, including some randomized controlled trials, have also pointed to large effects on transit use as well as important implications for overall mobility (Volinski, 2012; Cools, Fabbro and Bellemans, 2016; Cats, Susilo and Reimal, 2017; Bull, Munoz and Silva, 2021; Busch-Geertsema, Lanzendorf and Klinner, 2021). Other work examines the effects of free or reduced transit fares on particular domains, such as healthcare use Rosenblum (2020) or contact with criminal justice (Brough et al., 2022). We extend this literature by studying the effects of free public transit on a wide range of downstream outcomes for people with low income. We thus paint a more complete picture of the impacts of free transit on the well-being of individuals with low income.

Second, our results on employment test the extent to which spatial frictions limit employment prospects for people with low income. A large literature considers the causes and consequences of spatial mismatch (Kain, 1968; Wilson, 1997), or the geographic distribution of employment and residences that leads to differential access to jobs across groups. Many papers have documented how improved transportation infrastructure can improve employment outcomes for disadvantaged populations (Holzer, Quigley and Raphael, 2003; Fiorini and Sanfilippo, 2019; Abu-Qarn and Lichtman-Sadot, 2022). Most similar to the present study, a few randomized controlled trials (Phillips, 2014; Franklin, 2018; Abebe et al., 2021) show that subsidizing transportation for unemployed individuals increases the intensity of their job search efforts and leads to at least temporary improvements in labor market out-

comes. Relative to this literature, we study a more intense subsidy covering several months among a much broader group of disadvantaged individuals with a lower degree of pre-existing labor market attachment. Together with the literature, our results suggest that spatial mismatch effects may be more important on the margin for people already looking for work and less relevant for explaining why others have become disconnected from the labor market.

Finally, our study contributes to the literature on the effects of in-kind transfer programs on individuals' work behavior and well-being. A large body of work considers the impacts of in-kind transfer receipt on labor supply, and often concludes the effects are modest but negative (Hagstrom, 1996; Moffitt, 2002; East, 2018). In contrast, we find that providing free transit as an in-kind benefit to individuals with low income is, at worst, neutral for employment prospects, and could possibly improve recipients' broader financial and health situations.

2 Context

We conducted the experiment in King County, Washington. King County is home to Seattle, and with 2.3 million residents in 2020, it is the most populous county in Washington State. King County is served by an extensive public bus, streetcar, light rail, water taxi, and ferry network, which is overseen by the King County Metro Transit Department (i.e., King County Metro), Sound Transit, and other local transit agencies. Figure 1 shows the extent of the transit network at the time of our study. At that time, rail service largely consisted of one line running from the region's primary airport in south King County to the University of Washington north of downtown Seattle. Both rapid transit buses ('rapid ride') and regular local buses cover the remainder of the study area. As of 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]

Like many other metropolitan areas, King County exhibits significant gaps between where

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

people with low incomes live and the location of relevant jobs. With a median household income of $106,326 (ACS 2017-2021 estimates), King County skews higher income than the US as a whole at $68,703. These high income levels reflect a booming tech sector and plentiful employment downtown and in suburbs east of Seattle, like Bellevue. At the same time, King County's poverty rate is still relatively high, at 9.3% relative to a national rate of 10.5%. The shading in Figure 1 shows how income levels vary considerably across King County. In general, the northern part of Seattle and suburbs east of Seattle have very high average incomes, but other areas have much lower average incomes. In particular, the southern portion of King County that lies between Seattle and Tacoma has much lower income. Most people in our study reside in this southern portion of the county.

3 Experiment

Our experiment consisted of two separate cohorts, one that ran March-July 2019 and one that ran December 2019-March 2020.[3] The two cohorts had similar designs, reached much the same population, and delivered similar treatments. They differed primarily in their scope as well as in follow-up surveying approaches.

3.1 Recruitment and random assignment

The study 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 (e.g., SNAP or TANF). 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 during March 13-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

[3] Brough, Freedman and Phillips (2022) describe the experimental design in more detail.

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. As in much of the rest of the U.S., in King County COVID-19 prompted widespread business and school closures.

During their visit to the public benefits office, 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 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 the bus to $1.50 from $2.75. Since all recipients of SNAP and TANF 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.

The treatment group received a fully subsidized transit pass that lasted for up to six months. In particular, 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

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

which time the card reverted to an ORCA LIFT card identical to those provided to the control group.

The exact length of the full subsidy varied across people and study cohorts. In the first study cohort, the full subsidy expired on either July 31 or August 31, 2019, depending on when the passports were loaded onto the cards. As a result, individuals in the treatment group in the first RCT received as few as 4 weeks to as many as 24 weeks of free transit, depending on when they visited the DSHS office and were issued their card. On average, the treatment group in the first cohort received 16.7 weeks of free transit. In the second cohort, treatment card passports were set to expire on June 30, 2020. The onset of the pandemic, though, prompted substantial changes to public transit services, including a suspension of fare collection for all riders, which rendered the treatment moot in March 2020.[5] As a result, participants in the second cohort received between 0 and 14 weeks (mean 6.1 weeks) of full subsidies prior to the onset of COVID-19. Transit fares were reinstated system-wide on October 1, 2020. We were able to extend the treatment group's free transit period through December 31, 2020; we sent notices to study participants in May as well as in October 2020 alerting them of this change. Including this 3-month extension, individuals in the treatment group in the second RCT received between 14 and 27 weeks of free transit, though travel was relatively low in this latter period. Brough, Freedman and Phillips (2022) provide further details about the distribution of treatment intensity.

4 Data and descriptive statistics

4.1 Baseline characteristics

During enrollment in the study, participants took an intake survey that collects information on individuals' demographics and baseline travel habits. We use identifiers recorded in

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

the survey to link study participants with Metro's LIFT registry, which also tracks basic demographic characteristics.[6] Combining these two data sets, we have information on study participant age, race, household size, census block group of residence, language, transit use in 30 days prior to enrollment, and usual method of payment for transit. For participants in the second cohort, we also asked about mode of transportation to the enrollment site and whether cost represents a barrier to using public transit.

4.2 Washington State administrative records

We use several administrative datasets to capture downstream outcomes. First, we link the data to Washington State unemployment insurance (UI) records. These records allow us to track whether an individual was working each quarter, and if they were working, which employer(s) they worked for, how much they earned at each employer, and their hours of work at each employer. These data also allow us to construct measures of job stability, including job starts and exits as well as employment continuity.

Second, records from the Economic Services Administration of DSHS provides records on public assistance receipt. Using these records, we can track monthly participation in SNAP, TANF, Washington's Aged, Blind or Disabled Cash Assistance Program (ABD), and Washington's Housing and Essential Needs Program (HEN). ABD provides cash assistance to those aged 65 and over, who are blind, or who have a long-term disability and who meet certain income and resource requirements. HEN provides access to essential needs items and rental assistance to individuals with low-income and who are at least temporarily unable to work due to a physical or mental incapacity.

Third, we track individuals' health care utilization under Medicaid. Specifically, we can observe any Medicaid-funded health care visit by month of healthcare use. The State of Washington maintains its own Medicaid billing records, and approximately 63% of the

[6]This database also provides visibility into transit card use. Using these data along with follow-up survey information, Brough, Freedman and Phillips (2022) document large changes in transit use, and in particular transit use using a LIFT card, due to the treatment.

matched study sample is enrolled in Medicaid at baseline. Therefore, relying on Medicaid records is reasonably complete. We can further break these visits out into emergency in- and outpatient visits, non-emergency in- and outpatient visits, and the cost to Medicaid for any visit. When estimating the cost of healthcare, we follow Finkelstein et al. (2012) and assign expected cost to visit based on the average cost of different inpatient/outpatient and emergency/non-emergency combinations.[7] We also observe mental health and substance use diagnoses and treatments, as well as related prescriptions and prescription fills.

Finally, we measure criminal justice system contact using records from the Washington State Patrol (WSP). WSP compiles records from local jurisdictions to conduct background checks. We can track felony and misdemeanor arrests, and can further break out arrests by category including assault, theft, sex crime, domestic violence, custody-related crime, alcohol/drug crime, trespass, reckless driving, vehicle license, weapons, probation, murder, and failure to comply. We observe monthly indicators for each type of arrest.

We study a sample of people who complete random assignment and match to any of these datasets prior to enrollment. Washington DSHS's Research and Data Analysis group matched study participants who completed random assignment to state administrative records based on the name and date of birth as recorded in Metro's LIFT registry. We limit our main analysis sample to those who have some record of employment, public benefit receipt, healthcare, or arrest prior to random assignment. 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% (1598/1797) of people who complete random assignment appear in our analysis sample.

4.3 Non-government administrative records

In addition to linking individuals in the study to state administrative records, we link individuals to proprietary records to measure residential mobility and financial health.

[7] $7,523, $7958, $435, and $150 associated with non-ER inpatient care visit, ER inpatient care, ER outpatient care, and non-ER outpatient care, respectively

We measure residential mobility using consumer reference address histories. We follow Phillips (2020) in constructing measures of address moves from data compiled by Infutor Data Solutions. These data are derived from consumer reference records (e.g., cell phone bills) and cover the entire United States. They provide exact addresses and move dates by month, which we use to measure if a household moves after random assignment and, if so, where. We match study records to Infutor records using a fuzzy match based on name and date of birth within the set of people who ever show a King County address in Infutor's data. However, since some people do not generate a sufficient number of consumer records to appear in the Infutor data, these data have a lower match rate of 40% (722/1797). The low match rate limits statistical power compared to outcomes derived from state administrative data.

We measure financial health using quarterly cross-sections of credit records from Experian. The Experian data allow us to observe individuals' debt balances, credit scores, predicted incomes, debt-to-income ratios, bill delinquency, and credit inquiries. Experian conducts a match to the universe of credit reports using data on name, date of birth, and address; however, Experian requires an address to complete a match. Since our sample includes a non-negligible number of people experiencing homelessness or with an unstable address, these data have a lower match rate of 44% (796/1797). Again, this limits statistical power compared to outcomes derived from state administrative data.

4.4 Sub-sample survey

To complement administrative records, we gathered data on travel behavior and subjective well-being via a survey. We conducted this survey via a text message chatbot during the first cohort and via a traditional phone survey in the second cohort. Respondents completed questions about travel on the prior day, including information on trip quantity, mode, purpose, and payment method. Brough, Freedman and Phillips (2022) provide additional details about the survey instruments. In the present paper, we draw on a small number of

questions on subjective well-being that were asked among the second cohort only. These questions ask, for example, "In the past two months, how much has your health situation changed?" and we place responses on a 1 to 5 Likert scale.

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

4.5 Descriptive figures

Figure 2 shows mean hours worked, credit scores, and number of medical visits by cohort, over calendar time. Three important features of our study sample appear. First, our sample represents a disadvantaged group of participants with limited labor force attachment. In both cohorts, the average study participant has worked just over 100 hours, 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 limit of 660. Second, many participants enroll in the study soon after experiencing a major shock. For example, the enrollment period for the first cohort is shown shaded in dark gray. Panel (a) of Figure 2 shows that hours worked per quarter for the first cohort decline from just over 100 to just under 80 hours between the quarter before and the quarter of study entry. Medical visits similarly show an increase in panel (c) of Figure 2. These spikes in negative outcomes are not surprising for a group of people enrolling in public benefits. Third, and perhaps most important, 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 influence our empirical strategy, which we discuss in the next section.

5 Empirical strategy

5.1 Estimating treatment effects in a cross-section

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

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

We estimate this regression on cross-sections of individuals i observed at relative time period τ. In this regression, $Y_{i\tau}$ is an outcome (for example, hours of employment) for person i in quarter τ after random assignment. The 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 X_i that adjust this raw mean difference for two reasons. First, X_i includes an indicator for randomization strata related to the one-time change in the probability of treatment in the middle of the study. Second, it includes variables that reduce residual variance by predicting $Y_{i\tau}$.[8] Since random assignment was at the individual level, we compute heteroskedasticity robust standard errors.

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

5.2 Pooling treatment effects across time

Pooling treatment effects over time proves complicated for two reasons. First, the COVID-19 pandemic hits different participants at different moments. As noted above, COVID-19 both directly affects outcomes and made fare-free transit available to everyone for a time, including the control group. This shock matters more for the second cohort than the first cohort, as the treatment subsidy ended before 2020 for the first cohort. As a result, pooling treatment effects across cohorts in certain relative quarters (e.g., two quarters after random assignment) becomes complicated. Second, and more mechanically, participants enter the study continuously but we observe outcomes aggregated by calendar month or quarter. For example, the state measures hours worked, employment, and earnings at the quarterly level. For each person, relative quarter 0 will include a mix of pre- and post-enrollment outcomes. These pre-enrollment outcomes will not only not be affected by treatment but, as noted above, may also be particularly volatile for participants.

To address these concerns, we pool treatment effects 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:

$$Y_{i\tau} = \gamma \bar{T}_{i\tau} + \nu_i + \mu_\tau + \xi_{t(\tau,i)} + u_{i\tau} \tag{2}$$

We estimate this model on a panel of individuals, i, in relative time, τ. We include fixed effects for person, relative time, and calendar time $t(\tau, i)$. A new treatment variable, $\bar{T}_i$, measures the fraction of relative time period τ for which person i received an active treatment from the study. In particular, this variable is positive only if they were randomly assigned to treatment and the treatment was active, which for cohort 1 happens in relative quarters 0 and 1 and for cohort 2 happens in calendar quarters 2019Q4 and 2020Q1. We do not include 2020Q4 because of low card use after Metro re-instated fare payment. When turned on, it measures only the fraction of time post-study enrollment, for example, equalling one-third

for relative quarter 0 for a person enrolled in February. The manner in which we define $\bar{T}_{i\tau}$ allows for a simple interpretation of its coefficient, γ, which will reflect the average causal effect of having fully subsidized transit for an entire time period. Since we estimate a panel with multiple observations per person, we cluster standard errors by individual with this approach.

5.3 Baseline balance

Random assignment successfully balanced baseline characteristics across control and treatment groups in our randomized controlled trial. Table 1 shows baseline descriptive statistics for our main analysis sample. Columns (1) and (3) show means for the control and treatment groups, respectively, with sample sizes in columns (2) and (4). Column (5) shows a difference in means between the two groups (adjusting only for the change in randomization regime).

Consistent with randomization, individuals assigned to treatment and control are very similar. For example, 42.3% of the control identifies as White, compared to 40.7% of the treatment group. The regression-adjusted difference of 1.6 percentage points is identical to raw difference between the two groups and not statistically significant at the 5% level. The variables in different panels of the table come from different data sources, and sample sizes vary by data source. The first panel shows characteristics from the Metro's ORCA LIFT registry. The remaining panels show lagged outcomes from state administrative records, credit reports, and consumer reference address histories, respectively. None of these characteristics differ at the 5% level. Of note, lagged outcomes show balance across all linked datasets, which 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 low.

6 Results

6.1 First-stage effects on travel behavior

In response to a full transit subsidy, participants ride transit much more frequently. For all ORCA LIFT cards, including those issued by the study, we can observe how often study participants used the card to board transit. Figure 3 shows treatment effects on total transit boardings, as measured by card use. These results indicate that the treatment group boards transit using a card four times as often as the control group. As discussed in Brough, Freedman and Phillips (2022), some of this increase could result from the treatment group shifting from untraceable payment methods, like cash, or from non-payment. That paper uses the sub-sample survey to quantify these changes in payment method and concludes that overall transit use at least doubles in response to treatment, even after accounting for changes in payment methods.

The results on transit use suggest that the treatment represents a meaningful subsidy. First, the implied elasticity of transit demand is large, indicating that transit trips at least double in response to reducing the fare from $1.50 to $0. Second, the cash value of the treatment is large. If the card induces additional travel of one boarding per day for 16 weeks, that would cost the control group $168 in fares. The price of purchasing the actual monthly passes provided to the treatment group is similar. For a group of people with average earnings of $8,000 per year, that is a fairly large expenditure.

6.2 Labor market outcomes

We observe relatively small changes in employment in response to transit subsidies. Table 2 shows mean employment characteristics for the control and treatment groups in columns (1) and (2), respectively. Column (3) displays a simple difference between the two group means, only controlling for the change in treatment probability over time, and column (4) also includes pre-specified baseline control variables. On average, the treatment group works

for 81.5 hours in the quarter after random assignment, compared to 76.8 hours in the control group. The gap of 4.7 hours between the two groups widens to 5.6 hours when controlling for the randomization regime but narrows to 1.6 hours when controlling for other baseline characteristics.

The change in hours worked in the quarter after study enrollment is not statistically different from zero at conventional levels. The 95% confidence interval for the effect on hours worked spans -15.0 to 18.2 hours. This range includes values that are large relative to the control group mean, but are small relative to full-time work hours. For example, the upper bound of the 95% confidence interval for hours worked per quarter corresponds to 24% of the control group mean, but only 4% of full-time work hours.

As shown in panel (a) of Figure 4, these small differences in hours worked persist for at least eight quarters after random assignment. Because dynamic effects are similar in the several quarters following random assignment, we can pool results to gain greater precision. As shown in Table 3, a panel data model that pools results across post-treatment quarters (and takes 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 hours worked of -0.5 with a 95% confidence interval spanning -15.0 to 14.2. Based on our estimates from the panel data model, hours worked per quarter increase by no more than 18% of the control group mean and 3% of full-time employment.

We observe only small, statistically insignificant changes in other employment-related outcomes. Average earnings increase by only $8 per quarter (0.5%), with a 95% confidence interval ranging from -$312 to $327. The control group means and treatment effects for hours and earnings imply that hourly wage rates fall slightly from $19.00 per hour to $18.70 per hour. Similarly, the probably of any employment drops slightly by 3.2 percentage points from a control group mean of 32%. Job transitions also do not change substantially. Point estimates indicate a marginally significant 2.9 percentage point decline in job starts and a 0.9 percentage point increase in job exits. We also detect no change in continuous employ-

ment between pre- and post-periods, a measure of job stability, whether measured for any employment or employment in narrowly defined industries.

6.3 Finances

As an alternative to employment, transit subsidies might connect participants to public benefits. However, we find little evidence that the treatment group is more likely to access cash or food benefits. The first panel of Table 4 shows these results. For indicators of receiving any benefits and receiving food benefits, we observe null effects because the study enrolls a group of people who almost universally receive benefits from the Supplemental Nutrition Assistance Program (SNAP) already. In the control group, 91% of participants receive SNAP in the first quarter after random assignment, so it is not a surprise this value does not change in response to receiving free transit. On the other hand, control group rates of receiving cash assistance from either Temporary Aid for Needy Families (TANF) or other programs are low, at 2% and 13%, respectively. Still, the treatment group appears no more likely to access these cash assistance programs, suggesting that transit access does not help people sign up for public benefits.

Despite no change in access to financial resources from employment or public benefits, we do find some suggestive evidence that transit subsidies help improve the financial situation of the treatment group. We match a sub-sample of the study participants to credit records. The second panel of Table 4 shows these result. Based on our regressions with full controls (column (6)), total debt balances are $97 (5%) lower for the treatment group and credit scores 13 points (3%) higher. In this smaller sample, neither of these effects are statistically significant, but they are large, similar in magnitude to the effect of being evicted (Collinson et al., 2022) or having a bankruptcy removed from one's record (Gross, Notowidigdo and Wang, 2020). Any effects also appear at the time of random assignment, as shown in Figures 5.a and 5.b. Other variables observed on credit reports suggest improved financial situations. We do not detect changes in rare events like delinquencies, but we do see, for instance,

members of the treatment group seeking less new credit after random assignment. Measured one quarter after study enrollment, individuals in the treatment group have made 0.08 (24%) fewer new credit inquiries in the past three months. This difference, which is statistically significant at the 5% level, suggests that the financial situation of those that receive free transit improves such that they do not need to open new lines of credit.

6.4 Health and well-being

People receiving transit subsidies are less likely to use healthcare. The third panel of Table 4 shows average healthcare use, as measured by Medicaid claims records. Our pre-specified healthcare outcome, cost of Medicaid services, does not change noticeably because it is imprecisely measured. While the treatment effect on costs is not different from zero statistically, it has a wide confidence interval; the lower bound of the 95% confidence interval corresponds to a decline of $404, or 41% of the baseline mean. We have greater power for detecting changes in healthcare visits. In the control group, 35% 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 difference between the two groups in the probability of healthcare visit is statistically significant at the 5% level. Figures 5.c and 5.d show that the effect on healthcare visit accumulates entirely within 3 months. 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 31%. That outpatient visits drive the main result and are also less expensive than inpatient visits explains why we cannot detect effects on total cost measures.

We interpret the reduction in healthcare visits as an improvement in health. In principle, fewer healthcare visits could also indicate reduced healthcare access. We find this unlikely for two reasons. First, theory would suggest that free transit access should make it easier rather than harder for participants to visit a doctor, hospital, or clinic. Second, small-sample survey results suggest that self-reported well-being improves. As noted above, we surveyed a

sub-sample of 249 participants from the second cohort and asked a series of questions about well-being in different areas of life. Outcomes in each case are measured on a Likert scale from 1 to 5. The fourth panel of Table 4 reports these results. The treatment group reports higher well-being ratings in the areas of transportation, employment, finances, and health. While these differences are noisily measured in the small sample, they are consistent with the idea that reductions in healthcare use reflect improvements in well-being rather than reduction in access.

Improved well-being does not necessarily extend to all areas of life. The survey reports for subjective well-being in the areas of housing and education do not increase. Additionally, we do not observe any change in contact with the criminal justice system. As reported in the fifth panel of Table 4, the study sample is at relatively high risk of contact with criminal justice; 3.4% of the control group is arrested in the three months after random assignment. Providing free public transportation does not appear to affect participants' likelihood of coming into contact with the criminal justice system, as a statistically indistinguishable and economically similar fraction of the treatment group (4.2%) are arrested within three months of study enrollment.

6.5 Residential location

Any changes in residential location in response to transit subsidies appear to be small. Standard urban location theory predicts that people choose residential locations taking into account the cost of traveling from home to work and amenities; therefore, reducing the cost of travel would change residential location. We are able to match a sub-sample of 722 study participants to consumer reference address history data, which we use to measure rates of residential moves. The final panel of Table 4 displays these results. Overall rates of moving are relatively low. In the three months after random assignment, only 1.2% of the control group made any residential move. Move rates within three months are somewhat lower in the treatment group at 1.0%; the regression-adjusted treatment effect is -0.3 percentage points.

While the point estimate is not large in magnitude, given the size of the confidence interval, we cannot rule out increases in move rates of up to 1.3 percentage points. This suggests that the vast majority of people do not move, but we cannot rule out treatment effects that are large relative to baseline move rates.

The residential address data also help address concerns of attrition from data for our other outcomes. The data on employment, public benefit use, healthcare use, and arrest all cover the state of Washington; people moving out of state will exit those data. The address history data indicate such attrition is low. Only 0.5% of the control group and 0.3% of the treatment group move out of state within 3 months.

7 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 data. We do not detect large effects on employment outcomes, rejecting increases in hours worked of more than 2% of full-time employment. However, transit appears to have significant benefits for low-income individuals outside the labor market. People receiving free transit appear to be healthier; they are 16% less likely to use healthcare. Similarly, data from credit reports suggest improvements in their financial situation.

Our results for employment outcomes suggest that spatial mismatch effects in the labor market may be limited to people with stronger labor market attachment. A long theoretical and literature since at least Kain (1968) and Wilson (1997) argues that poverty persists in particular neighborhoods because of limited geographic access to employment. Prior studies, including multiple randomized controlled trials, indicate that transportation subsidies can

increase search intensity and employment for people with low income who are looking for work (Phillips, 2014; Franklin, 2018; Abebe et al., 2021). Compared to these studies, we observe quite modest effects on employment. One possibility is that the broad set of public benefit recipients in the study sample includes many individuals with limited labor force attachment, and spatial mismatch effects operate primarily for people who are actively looking for work rather than those who are more persistently detached from the labor market.

However, our results also suggest that free-fare transit generates important welfare benefits that would be missed by existing economic models. Typical models quantify benefits of transit access based on changes in travel from home to work (Tsivanidis, 2019), but, in principle, spatial frictions matter for any activity requiring travel: working, using public benefits, accessing healthcare, visiting family, and so on. Our results indicate that travel behavior of low-income individuals responds elastically to the price of transit and that this additional travel generates health and financial benefits, despite little change in employment or residential location. Even in a context where public transportation has limited effects on employment and residential location, it can have important welfare benefits for people with low income.

References

Abebe, G., S. Caria, P. Falco, S. Franklin, M. Fafchamps, and S. Quinn. 2021. "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City." *Review of Economic Studies*, 88(3): 1279–1310.

Abu-Qarn, Aamer, and Shirlee Lichtman-Sadot. 2022. "The Trade-Off Between Work and Education: Evidence from Public Transportation Penetration to Arab Towns in Israel." *Journal of Policy Analysis and Management*, 41(1): 193–225.

Barry, Ellen, and Greta Rybus. 2020. "Should Public Transit Be Free? More Cities Say, Why Not?" *The New York Times*, last accessed on January 26, 2023.

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

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

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

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

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

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

Collinson, Robert, John Humphries, Nicholas Mader, Davin Reed, Daniel Tannenbaum, and Winnie Van Dijk. 2022. "Eviction and Poverty in American Cities." National Bureau of Economic Research Working Paper No. 30382.

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

East, Chloe. 2018. "Immigrants' Labor Supply response to Food Stamp Access." *Labour Economics*, 51: 202–226.

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

Fiorini, Matteo, and Marco Sanfilippo. 2019. "Roads and Jobs in Ethiopia." 2019/116, WIDER Working Paper.

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

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

Hagstrom, Paul A. 1996. "The Food Stamp Participation and Labor Supply of Married Couples: An Empirical Analysis of Joint Decisions." *Journal of Human Resources*, 383–403.

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

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

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

Moffitt, Robert. 2002. "Welfare Programs and Labor Supply." In . Vol. 4 of *Handbook of Public Economics*, 2393–2430. Elsevier.

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

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

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

Serebrisky, Tomás, Andrés Gómez-Lobo, Nicolás Estupiñán, and Ramón Muñoz-Raskin. 2009. "Affordability and Subsidies in Public Urban Transport: What Do We Mean, What Can Be Done?" *Transport Reviews*, 29(6): 715–739.

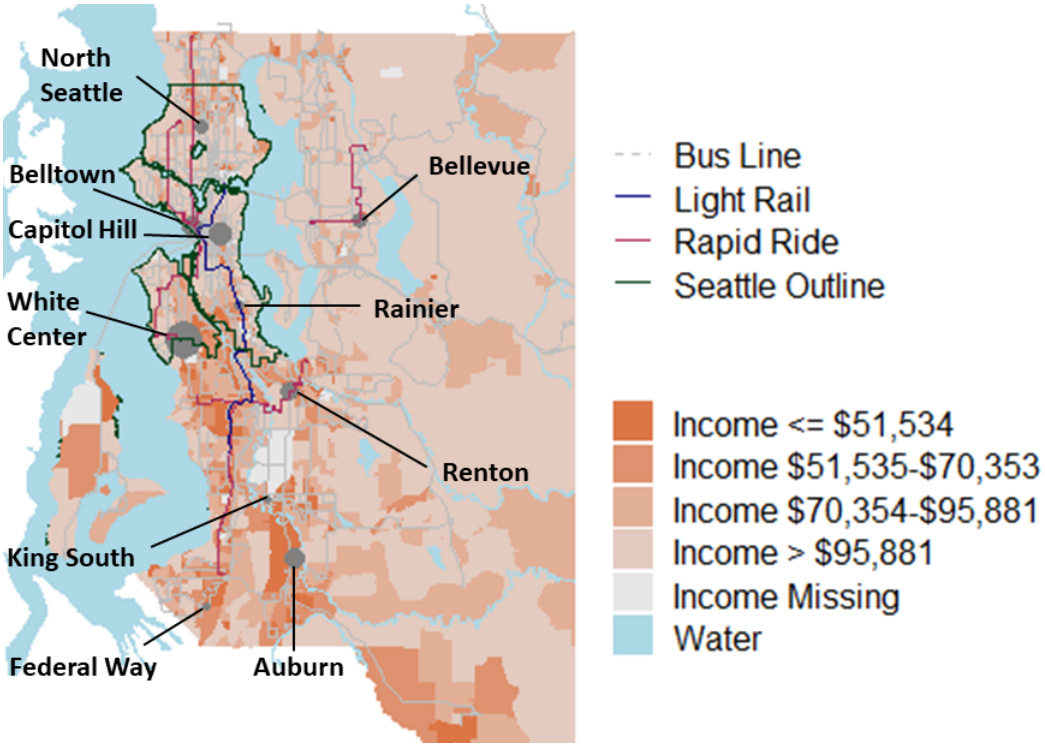
Tsivanidis, Nick. 2019. "Evaluating the Impact of Urban Transit Infrastructure: Evidence from Bogota's TransMilenio." UC Berkeley Working Paper.

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

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

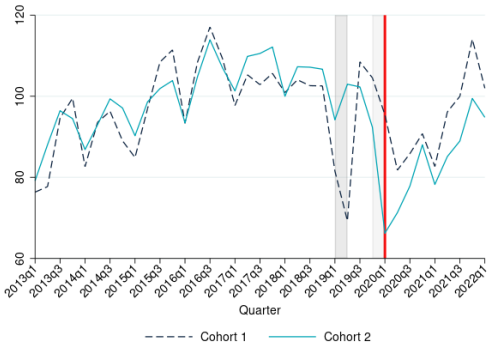
Figures

Figure 1. King County Metro Routes and CSOs

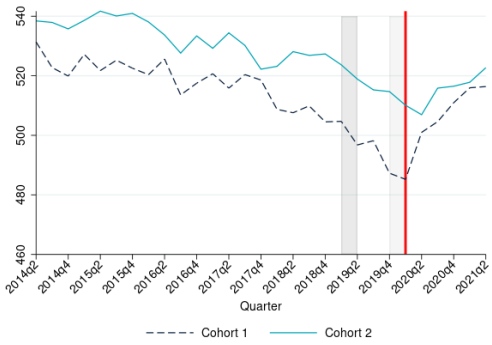


Notes: This figure is a map of western King County, shaded by income quartile using data from the 2014-2018 American Community Survey. The extent of the transit network is shown as of 2019. The 10 King County Community Service Offices (CSOs) in which enrollment occurred are marked by gray dots. The size of the dots correspond to the proportion of the sample who enrolled from each CSO office.

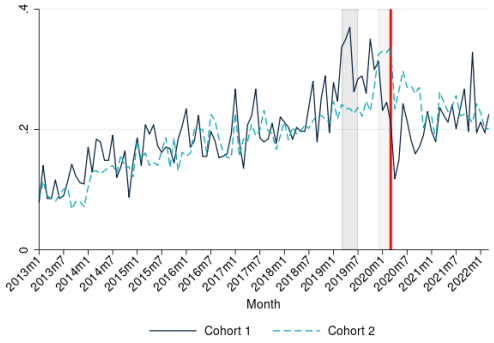
Figure 2. Mean Outcomes, by Calendar Time and Cohort



(a) Hours Worked



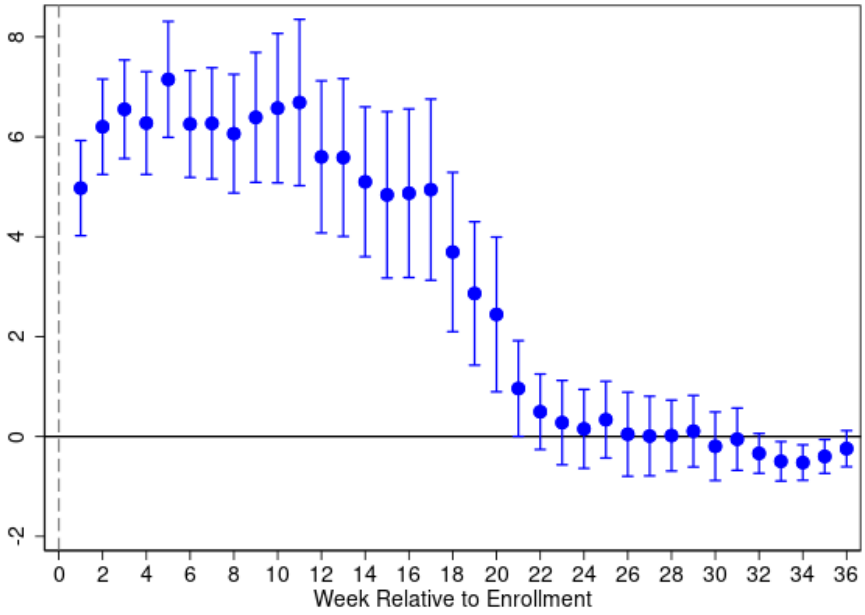
(b) Credit Score



(c) Number of Medical Visits

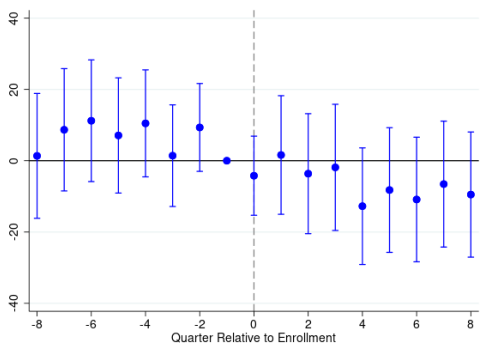
Notes: These figures display trends in mean hours worked, credit score, and number of monthly medicaid visits. The dark gray shading corresponds to the period during which cohort 1 entered the study. The light gray shading corresponds to the period during which cohort 2 entered the study. The red line 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

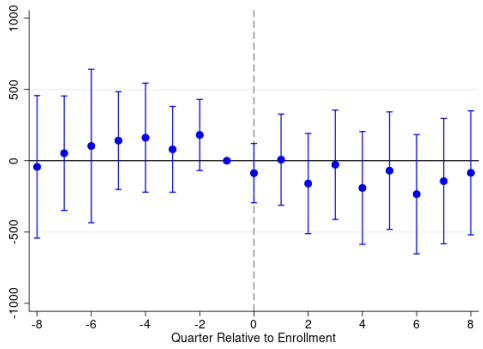


Notes: Each dot measures the treatment effect of receiving free public transit at the relative time horizon indicated on the horizontal axis. Each treatment effect is measured as regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcome is the number of transit boardings paid for with an ORCA card. Control variables are indicators for randomization regime, female, Black, Hispanic, other race (excluding White), age, age squared, and the month of study enrollment. The whiskers show a 95% confidence interval, computed using heteroskedasticity-consistent standard errors.

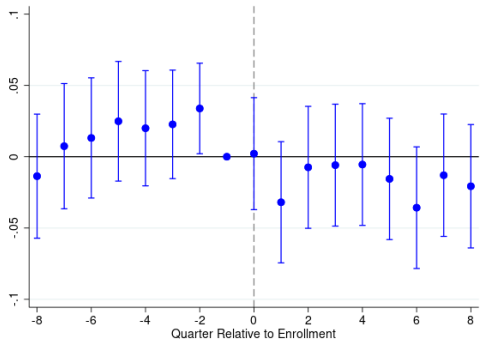
Figure 4. Treatment Effects on Employment Outcomes, by Relative Time



(a) Quarterly Hours



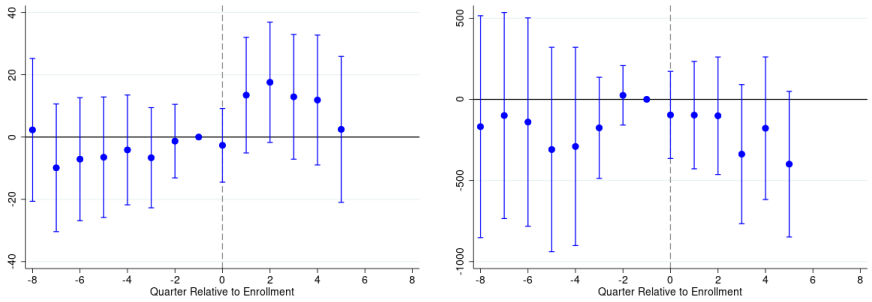
(b) Quarterly Earnings



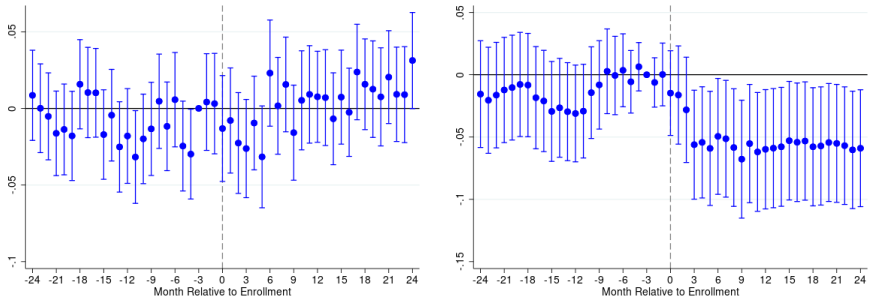
(c) Employed

Notes: Each dot measures the treatment effect of receiving free public transit at the relative time horizon indicated on the horizontal axis. Each treatment effect is measured as regression-adjusted difference in means from a separate regression, as specified in equation (1). Outcomes are measured using Washington unemployment insurance records. Control variables are the outcome in the period prior to random assignment and indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment. The whiskers show a 95% confidence interval, computed using heteroskedasticity-consistent standard errors.

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



(a) Credit Score (b) Balance in Collections



(c) Any Health Visit, Flow (d) Any Health Visit, Cumulative

Notes: Each dot measures the treatment effect of receiving free public transit at the relative time horizon indicated on the horizontal axis. Each treatment effect is measured as regression-adjusted difference in means from a separate regression, as specified in equation (1). The outcomes in (a) and (b) come from quarterly cross sections of Experian credit reports while (c) and (d) come from monthly summaries of Medicaid records. Control variables are the outcome 3 months (or 1 quarter) prior to random assignment and indicators for randomization regime, female, Black, Hispanic, other race (excluding White), and the month of study enrollment. Figures (a) and (b) additionally control for age and age squared. The whiskers show a 95% confidence interval, computed using heteroskedasticity-consistent standard errors.

Tables

Table 1. Mean Baseline Characteristics by Treatment Assignment

	(1) Control	(2)	(3) Treatment	(4)	(5) Simp Reg
	Mean	N	Mean	N	Adj. Diff
State Administrative Records					
White	0.423	977	0.407	621	-0.016
					(0.025)
Hispanic	0.088	977	0.076	621	-0.013
					(0.014)
Black	0.279	977	0.288	621	0.010
					(0.023)
Female	0.407	977	0.391	621	-0.017
					(0.025)
Years of Education	11.945	849	12.103	552	0.168
					(0.111)
Owns Vehicle	0.195	977	0.169	621	-0.024
					(0.020)
Hours employed	99.404	977	108.615	621	8.916
					(9.069)
Total earnings	1,955.365	977	2,110.329	621	45.528
					(190.243)
Any Formal Employment	0.331	977	0.361	621	0.014
					(0.024)
Cost to Medicaid, cumulative	612.962	977	806.055	621	162.471
					(132.024)
Any Medicaid visit, cumulative	0.243	977	0.245	621	-0.001
					(0.022)
Any food or cash benefits	0.599	977	0.588	621	-0.008
					(0.025)
Any Arrest	0.036	977	0.029	621	-0.007
					(0.009)
Financial Health – Experian Data					
Credit Score	515.691	473	508.610	323	-8.389
					(13.150)
Balance in Collection	1,929.994	473	1,557.749	323	-310.858
					(331.588)
Residential Mobility – Infutor Data					
Any Move	0.012	432	0.014	290	0.000
					(0.009)

Notes: This table presents means and regression-adjusted differences in means for baseline characteristics. Each panel shows data on the set of participants who match to the dataset listed in the panel's title prior to random assignment; different match rates result in different sample sizes. Educational attainment data is incomplete for individuals matching to state administrative records, and so is only reported for 1401 individuals. Column 5 presents the regression-adjusted difference in means between treatment and control groups, adjusting for the randomization regime used upon study enrollment. Demographics are measured at the time of data linking. Employment is measured 1 quarter prior to enrollment and all other variables at 3 months prior to enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 2. Employment Outcomes, 1 Quarter After Study Enrollment

	(1) Control	(2) Treatment	(3) Simp Reg. Adj. Diff	(4) Reg. Adj. Diff.
Hours worked in relative qtr 1	76.827	81.520	5.649	1.608
			(8.949)	(8.489)
Earnings in relative quarter 1	1,459.380	1,476.942	48.370	7.527
			(170.232)	(163.043)
Employed in relative quarter 1	0.322	0.295	-0.023	-0.032
			(0.024)	(0.022)
Employed in -1, employed in +1	0.190	0.188	0.004	0.001
			(0.020)	(0.020)
–Cont. sector Employment between relative quarter -1 and +1	0.133	0.134	0.003	0.004
			(0.018)	(0.018)
–Cont. industry Employment between relative quarter -1 and +1	0.107	0.108	0.004	0.006
			(0.016)	(0.016)
Unemployed in -1, employed in +1	0.132	0.106	-0.027	-0.029*
			(0.017)	(0.017)
Employed in -1, unemployed in +1	0.138	0.151	0.010	0.009
			(0.018)	(0.018)
Unemployed in -1, unemployed in +1	0.539	0.554	0.013	0.019
			(0.026)	(0.026)
N	977	621		

Notes: This table presents means and regression-adjusted differences in means for employment outcomes measured in the quarter after enrollment using Washington state unemployment insurance records. Continuous employment, job gains, and job losses are measured comparing the quarter before and the quarter after enrollment. Sectors and industries are defined by 2-digit and 6-digit NAICS codes, respectively. Column 3 presents the regression-adjusted difference in mean 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 1 quarter prior to study enrollment (for hours, earnings, and any formal employment outcomes only). The sample here is limited to individuals who go through random assignment and match to any Washington state administrative record prior to study enrollment. Heteroskedasticity-robust standard errors are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 3. Employment Outcomes, Panel Regressions

	(1) Hours	(2) Earnings	(3) Any Employment
Treat	-0.461	-48.119	0.001
	(7.468)	(147.724)	(0.022)
Person FE	×	×	×
Calendar Qtr FE	×	×	×
Relative Qtr FE	×	×	×
Control Mean	96.298	1,822.148	0.314
Observations	27166	27166	27166
Individuals	1598	1598	1598

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable is defined as the fraction of the quarter for which the individual is after study enrollment, multiplied by the fraction of the quarter for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 8 quarters prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 4. Secondary Outcomes, One Quarter After Enrollment

	(1) Control Mean	(2) N	(3) Treatment Mean	(4) N	(5) Simp Reg Adj. Diff	(6) Reg. Adj. Diff
Public Assistance Receipt, measured 3 months post enrollment						
Any food or cash benefits	0.932	977	0.915	621	-0.018	-0.016
					(0.014)	(0.013)
–SNAP	0.912	977	0.889	621	-0.024	-0.022
					(0.016)	(0.015)
–TANF	0.024	977	0.032	621	0.007	0.003
					(0.009)	(0.008)
– Other	0.132	977	0.111	621	-0.020	-0.014
					(0.017)	(0.015)
Financial Health, measured in the third month of the quarter post enrollment						
Balance in Collection	1,621.746	492	1,363.698	334	-220.409	-96.984
					(219.839)	(168.849)
Credit Score	500.813	492	513.991	334	9.214	13.448
					(13.658)	(9.471)
Total Inquiries in Past 3 Mos	0.343	492	0.257	334	-0.098**	-0.082**
					(0.039)	(0.041)
Healthcare, measured 3 months post enrollment						
Cost to Medicaid, cumulative	975.211	977	912.660	621	-43.029	-77.361
					(176.374)	(167.001)
Any Medicaid visit, cumulative	0.347	977	0.282	621	-0.062***	-0.056**
					(0.024)	(0.022)
–Emergency outpatient	0.246	977	0.208	621	-0.034	-0.032
					(0.021)	(0.021)
–Emergency inpatient	0.044	977	0.035	621	-0.008	-0.008
					(0.010)	(0.010)
–Non-emergency inpatient	0.024	977	0.021	621	-0.001	0.000
					(0.007)	(0.007)
–Non-emergency outpatient	0.298	977	0.237	621	-0.059***	-0.050**
					(0.023)	(0.022)
Well-Being Measures, from self-reported survey						
Transportation well-being	3.016	125	3.214	124	0.207	0.249*
					(0.137)	(0.139)
Employment well-being	2.516	126	2.710	124	0.203	0.238
					(0.157)	(0.160)
Financial well-being	2.437	126	2.681	124	0.246	0.258
					(0.160)	(0.168)
Health well-being	2.980	125	3.053	123	0.068	0.125
					(0.126)	(0.123)
Housing well-being	2.972	125	2.988	125	0.019	-0.005
					(0.136)	(0.138)
Education well-being	3.357	122	3.319	124	-0.034	-0.041
					(0.122)	(0.119)
Criminal Justice, measured 3 months post enrollment						
Any Arrest	0.034	977	0.042	621	0.009	0.009
					(0.010)	(0.010)
Residential Mobility, measured 3 months post enrollment						
Any Move	0.012	432	0.010	290	-0.003	-0.003
					(0.008)	(0.008)
Any Move in State	0.007	432	0.010	290	0.003	0.002
					(0.006)	(0.007)
Any Move out of State	0.005	432	0.003	290	-0.003	-0.002
					(0.005)	(0.005)
Any Move in County	0.005	432	0.010	290	0.005	0.004
					(0.006)	(0.007)
Any Move out of County	0.007	432	0.003	290	-0.005	-0.004
					(0.005)	(0.005)

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

Table 5. State Administrative Outcomes, Panel Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Medical						Benefits				Criminal Justice
	Cost to Medicaid Monthly	Any Medicaid visit Monthly	Emergency outpatient	Emergency inpatient	Non-emergency inpatient	Non-emergency outpatient	Any food or cash benefits	SNAP	TANF	Other	Any Arrest
Treat	-18.469	-0.014	-0.003	-0.001	-0.000	-0.012	-0.006	0.001	-0.001	-0.004	-0.014**
	(41.314)	(0.010)	(0.008)	(0.003)	(0.002)	(0.010)	(0.018)	(0.018)	(0.006)	(0.011)	(0.006)
Person FE	×	×	×	×	×	×	×	×	×	×	×
Calendar Month FE	×	×	×	×	×	×	×	×	×	×	×
Relative Month FE	×	×	×	×	×	×	×	×	×	×	×
Control Mean	141.742	0.089	0.052	0.008	0.003	0.072	0.620	0.506	0.025	0.055	0.030
Observations	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302	78302
Individuals	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598	1598

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable is defined as the fraction of the month for which the individual is after study enrollment, multiplied by the fraction of the month for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 24 months prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to any King County administrative record prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 6. Residential Mobility Outcomes, Panel Regressions

	(1) Any Move	(2) Any Move In WA	(3) Any Move Outside WA	(4) Any Move In King County	(5) Any Move Outside King County
Treat	0.006	0.000	0.006*	0.001	0.006
	(0.005)	(0.004)	(0.004)	(0.003)	(0.004)
Person FE	×	×	×	×	×
Calendar Qtr FE	×	×	×	×	×
Relative Qtr FE	×	×	×	×	×
Control Mean	0.014	0.011	0.003	0.009	0.006
Observations	34790	34790	34790	34790	34790
Individuals	710	710	710	710	710

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome an active treatment variable and calendar month, relative month, and individual fixed effects. The active treatment variable is defined as the fraction of the month for which the individual is after study enrollment, multiplied by the fraction of the month for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 24 months prior to and post study enrollment for all sample individuals. The sample here is limited to individuals matching to Infutor consumer reference data prior to random assignment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 7. Financial Health Outcomes, Panel Regressions

	(1) Balance in Collections	(2) Credit Score	(3) Credit Inquiries in Past 3 Months
Treat	165.978	-0.974	-0.022
	(186.794)	(6.365)	(0.034)
Person FE	×	×	×
Calendar Qtr FE	×	×	×
Relative Qtr FE	×	×	×
Control Mean	1,838.708	515.945	0.326
Observations	11061	11061	11061
Individuals	872	872	872

Notes: Each column of this table presents the estimate of the coefficient on treatment in a separate panel data regression of the listed outcome an active treatment variable and calendar quarter, relative quarter, and individual fixed effects. The active treatment variable is defined as the fraction of the quarter for which the individual is after study enrollment, multiplied by the fraction of the quarter for which the treatment group receives free fares differentially from the control group, multiplied by the treatment status. The panel consists of 8 quarters prior to and 5 quarters post study enrollment for all sample individuals. The sample here is limited to individuals matching to any credit report prior to study enrollment. Standard errors clustered by individual are in parentheses. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.