Means-Tested Public Transportation Subsidies: Causal Evidence and Implications

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Abstract

This paper evaluates the effects of a novel and large-scale program in King County, Washington that provides fare-free public transportation to individuals with low incomes. Using both experimental and quasi-experimental methods together with rich administrative and survey data, we find that providing free transit has very limited effects on mobility or downstream outcomes related to health and well-being. We examine possible explanations for the modest effects, which contrast with findings from prior research. Our results suggest that changes in the external environment post-COVID-19 pandemic, including changes in norms around fare evasion, reduced the price elasticity of demand for travel by transit among individuals with low incomes.

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

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1 Introduction

Means-tested transit fare programs have grown in popularity and spread among cities around the world (Darling et al., 2021; Gómez-Lobo, Sánchez González and González Mejia, 2022). Such programs offer free or reduced-price transit based on an individual's income or socioeconomic status. By offering lower fares to populations experiencing disadvantage, these programs aim to reduce disparities in access to urban amenities and employment opportunities as well as decrease overall reliance on private vehicles. Whether means-tested transit fare reductions meet these goals depends on the extent to which travel behavior, and other downstream outcomes, respond elastically.

In this paper, we evaluate the effects of a novel and large-scale program in King County, Washington that provides fare-free public transportation to individuals with low incomes. In October 2022, King County launched a means-tested full-fare subsidy at scale. The "subsidized annual pass" (i.e., annual pass) program provides fareless transit to individuals who have income at or below 80% of the federal poverty line and participate in at least one of six cash assistance programs administered by the State of Washington. We run a randomized controlled trial (RCT) in which individuals who meet the 80% income threshold but do not participate in qualifying programs were randomly offered the annual pass for one year. We compare mobility outcomes as well as downstream health, employment, and other outcomes for those who did vs. did not receive the pass. We supplement this analysis with a quasi-experimental analysis in which we compare outcomes for people in the control group of the RCT to people who met the county's standard eligibility requirements and enrolled in the annual pass program after being introduced to it through the study survey.

Taking advantage of rich administrative and survey data, we find that providing free transit has limited effects on mobility or downstream outcomes related to health and well-being for the targeted population. This finding holds both in the RCT and in the quasi-experimental design. In the RCT, we observe marginally significant increases in transit boardings using electronic fare cards in response to free transit; relative to those in the control group, who had to pay \$1 per bus ride, those in the treatment group boarded transit an additional 0.059 times per day, which translates into approximately one additional boarding every 2.5 weeks. Focusing on the treatment on the treated, our estimates based on card taps imply a price elasticity of demand for transit of 0.13. Even this estimate overstates the true elasticity; broader survey measures of all transit use or all travel, which include trips using other payment methods or other modes, show smaller or opposite signed treatment effects. Estimates of the price elasticity of demand for public transit travel are similar for people who are already eligible for the annual pass versus those who are randomized into the study, even though these groups differ considerably on observable characteristics.

We examine potential explanations for the modest effects of fareless transit on mobility, which contrast with findings from other studies. Chizeck and Mbonu (2025), Huberts et al. (2025), and Brough, Freedman and Phillips (2022, 2025) all run RCTs measuring the mobility impacts of free fares for low-income populations in US cities and estimate price elasticities of demand for transit travel that are over three times greater than that found in the present RCT. The contrast with Brough, Freedman and Phillips (2022, 2025) is particularly stark because they study a very similar means-tested transit subsidy intervention in the same location (King County, Washington). We find that differences between our study and these prior King County studies in terms of the socioeconomic characteristics of the populations served, take-up rates, and the fare paid by the control group together explain only a small portion of the differences in results. Instead, we find suggestive evidence that a key explanation is differences in the external environment, including post-pandemic changes in norms in King County around fare evasion that may have been instigated by the suspension of fare collection for an extended period of time during COVID-19. These changes appear to have made demand for travel by transit among residents with low incomes less price elastic.

Our paper contributes to a growing literature on means-tested transit programs, which have been implemented in a number of cities across the US and many other countries (Darling et al., 2021; Gómez-Lobo, Sánchez González and González Mejia, 2022). Prior work on such programs has tended to find significant impacts of fare subsidies on transit use, but limited effects on other outcomes. As alluded to above, Chizeck and Mbonu (2025), Huberts et al. (2025), and Brough, Freedman and Phillips (2022) all document large changes in mobility among public assistance clients who receive free transit for a period of time. Chizeck and Mbonu (2025) also find some small earnings effects of full fare discounts. Huberts et al. (2025) and Brough, Freedman and Phillips (2025), meanwhile, find no signs of labor market benefits, instead seeing improvements in other dimensions of well-being (e.g., health). In another RCT, Brough et al. (2022) also find no effect of transit subsidies on court appearance rates among criminal defendants. Meanwhile, Guzman, Cardona and Ochoa (2024) and Guzman et al. (2025) find that targeted travel vouchers in Bogotá, Colombia have significant effects of travel demand and mobility, but that much of the gain in welfare to users derives from new non-work trips. In contrast, Phillips (2014), Franklin (2018), and Inga (2023) find that transport subsidies specifically targeted to unemployed people can improve job search and employment outcomes.

A larger literature has considered how transit subsidies in general (not necessarily targeted at individuals with low incomes) affect travel behavior. For example, Bull, Munoz and Silva (2021) find in an RCT in Santiago, Chile that free transit for employees of a large firm increase transit use, but the extra trips on transit do not replace trips by private vehicle. Andor, Flintz and Vance (2024) leverage an RCT providing one month of free transit to car users and find significant increases in transit use during the subsidy period, but no persistent changes in mode choice in the longer run. In quasi-experimental work, Ofosu-Kwabe, Lim and Malalgoda (2024) find that universal free fares substantially increase ridership but have limited impacts on labor force participation or income inequality. However, using a staggered difference-in-differences design that exploits the roll-out of fare-free transit across Brazilian localities, Rodrigues, Da Mata and Possebom (2024) find significant positive employment effects and environmental benefits of subsidized public transportation.[1]

[1] A related literature considers the effects of public transportation network expansions on employment among groups experiencing disadvantage (e.g., Holzer, Quigley and Raphael (2003)). Bastiaanssen, Johnson

Our findings have important implications for the design of transit fare policies in the post-pandemic era. Across the US, transit ridership has not recovered to pre-pandemic levels (Federal Transit Administration, 2024). Among those who continue to use transit for commuting and other travel, norms around fare payment in many cities have been reshaped by COVID-era policies and practices that may have had persistent impacts on people's perceptions of the probability of being caught and punished for fare evasion, and more generally the stigmatization of fare non-payment.[2] This may call for a more concerted effort on the part of local governments and transit agencies to alter norms through both outreach and heightened enforcement. It also may argue for transit agencies relying less on individual rider payment and more on alternative sources of revenues, such as employer-sponsored transit programs. To the extent that transit agencies want to influence low-income rider travel decisions, our results suggest that such riders may be less responsive to fare changes than in the past.

2 Background

This study draws participants with very low income living in King County, Washington. King County is centered on Seattle, the largest city in Washington, with over four million people living in the metropolitan area (approximately half the population of Washington State). However, many people with low income live in areas of suburbanized poverty in south King County. King County has a transit system that primarily relies on buses but that also offers a variety of other transit options, including ferries and a newer and expanding rail network. These services are operated by a collection of agencies, the largest of which are

and Lucas (2020) review the broader literature on public transportation access and employment.

[2]The National Academies of Sciences is currently funding a project on innovations and best practices in fare enforcement, motivated in part by ongoing challenges associated with fare evasion experienced by transit agencies across the country (Transit Cooperative Research Program, 2025). Media outlets have also extensively covered this phenomenon; see, for example, `https://www.cbsnews.com/news/transit-systems-fare-evaders-riders-crime/` and `https://www.bostonglobe.com/2024/09/02/opinion/an-existential-threat-transit-new-york/`.

King County Metro and Sound Transit.[3] While transit use in the Seattle area is higher than the national average, it declined sharply during the pandemic and has not fully recovered. About 14% of City of Seattle commuters used public transit in 2023, down from nearly 25% in 2019.[4] Within King County, people with low income were more likely to use public transit prior to COVID and more likely to persist in using it throughout the pandemic (Brough, Freedman and Phillips, 2021).

In October 2022, King County Metro launched the subsidized annual pass program (commonly referred to as the annual pass program). The program is a collaboration between King County Metro, Sound Transit, and state social service agencies who assist with enrollment. Individuals who hold the pass can, at no cost, ride on and transfer between the two transit agencies' services across an extensive regional transit network, primarily Metro's bus services and Sound Transit's light rail services.

Individuals are eligible for the program's annual, renewable transit pass if they meet the following criteria: (1) they reside in the three metropolitan counties surrounding Seattle (King, Snohomish, and Pierce); (2) they have a household income less than or equal to 80% of the federal poverty level (FPL) ($21,204 for a family of four at the start of enrollment); and (3) they are enrolled in at least one of six cash benefit programs administered by the State of Washington Department of Social and Health Services (DSHS).[5] At the time of the annual pass program's launch, an estimated 106,000 people met these eligibility criteria.

The enrollment process for the annual pass operates through partner social service agencies. The initial roll-out of the annual pass program was designed to accommodate capacity constraints of the transit and enrollment agencies, with most people learning about and enrolling in the annual pass when they sought other public benefits. Most eligible individuals enroll over the phone by calling Washington State's Department of Social and Health

[3]Other agencies include Community Transit, Kitsap Transit, and Pierce Transit.

[4]Authors' calculations based on 1-Year American Community Survey estimates for 2023 and 2019.

[5]The cash benefit programs are Temporary Assistance for Needy Families (TANF)/State Family Assistance (SFA); Refugee Cash Assistance (RCA); Aged, Blind, or Disabled Cash Assistance (ABD); Pregnant Women Assistance (PWA); Supplemental Security Income (SSI); and Housing & Essential Needs (HEN).

Services (DSHS) or Seattle and King County's Department of Public Health. Eligible individuals can also enroll in person, e.g. at state social service offices. The enrollment agencies verify clients' identity, income, and participation in one of the qualifying cash-benefit programs; enter clients' information into Metro's annual pass program registry; and provide them with the annual pass immediately in-person or through the mail.

The annual pass is valid for one year from the date of issuance, regardless of whether the holder's income or eligibility change. After one year, the pass holder must re-enroll and meet eligibility criteria again to continue to access the annual pass. The annual pass is visually indistinguishable from the region's other transit passes (known as ORCA passes) that are commonly used by residents who ride public transit.

The annual pass program complements another means-tested transit fare program, the ORCA LIFT program. The ORCA LIFT program has existed since 2015, and offers individuals with incomes equal to or less than 200% of FPL reduced-price (but not free) fare transit. In particular, ORCA LIFT allows people to ride transit at a price of $1 on most services, compared to, for example, a regular adult fare of $2.75 on King County Metro buses. It also allows riders to transfer between most transit services at no additional cost. As described below, we use the ORCA LIFT registry as a sample frame for our study of the annual pass program.

3 Experiment

3.1 Design

The experiment occurred alongside the roll-out of the annual pass.[6] We worked with a survey firm, which contacted potentially eligible Metro clients. Early in the study, they used a variety of methods to reach potential participants, including a physical presence at community organizations. However, the vast majority of study participants came via

[6] See Appendix Figure A1 for a visual representation of the enrollment process.

contacting prior recipients of partial fare subsidies. In particular, they contacted individuals who had previously registered for the ORCA LIFT program, had not yet registered for the annual pass, and whose income-eligibility for LIFT had been verified by a means likely to still be valid.[7] This latter restriction makes it more likely that we draw a population from the ORCA LIFT registry that has income below the 80% threshold for qualification for the annual pass, but not necessarily individuals participating in one of the six cash benefit programs; i.e., a population with low income but including a mix of people who are and are not eligible for the annual pass. At the time we launched the experiment, the ORCA LIFT registry had approximately 31,000 people who met these criteria and had contact information available.

These individuals were invited to complete a brief initial survey, which we used to screen people out of or into the study. A total of 6,895 people completed the screener survey, and 3,123 were identified as eligible for the study because they reported incomes at or below 80% of FPL. People who met this criteria were subsequently invited to participate in the study. Those who responded completed a baseline survey including an informed consent process and a short questionnaire on mobility, health, and well-being. This survey could be completed by web, mail, or phone, though the vast majority chose a web survey. They received a $50 survey incentive.

We group respondents into those who report participating in one of the six cash benefit programs that determine annual pass program eligibility and those who do not. Those in the former group (i.e., the "already eligible" group) were immediately referred to Metro, which coordinated verifying eligibility and then enrolled them in the annual pass program. Among those in the latter group, Metro staff used a computer random number generator created by the research team to randomly and independently assign households with equal probability to receive either a regular partial fare ORCA LIFT card (the "control" group) or the annual

[7]These included either verified use of other benefits with an income test (cash, food, and healthcare assistance) or checking against earnings as recorded by the unemployment insurance system.

pass (the "treatment" group).[8] Thus, in addition to randomized control and treatment groups whose outcomes we can compare, we have a third group of treated individuals who met program eligibility requirements and with whom we can also compare outcomes.

For all groups, the end of the baseline survey collected information on all household members. This information was passed to Metro staff, who processed requests for cards and sent fare cards by mail to the person's preferred address. The already eligible and treatment groups received the annual pass (i.e., a fully subsidized ORCA card) and an information packet describing the pass' use and benefits, while the control group received a typical ORCA LIFT partial-subsidy card and standard informational materials. Cards were provided for all members of the household. This process was relatively quick but not instantaneous. A median of 40 days and a 90th percentile of 96 days elapsed between the baseline survey and a card being processed for mailing by Metro.

3.2 Empirical Approach

Randomly offering the annual pass to those who meet the income threshold but are ineligible simply because they are not enrolled in one of the six qualifying cash benefits programs creates an RCT among people with incomes less than or equal to 80% of FPL. We use regression analysis to compare outcomes for otherwise ineligible survey respondents who are offered the annual pass to outcomes for those who are not offered the annual pass. The randomization allows us to attribute any observable differences in outcomes between these two groups to the annual pass intervention. Following our pre-analysis plan, our starting point is a regression of the following form:

$$y_i = \alpha_0 + \beta_0 T_i + \mathbf{X}_i \delta_0 + \epsilon_i \tag{1}$$

where y_i is the outcome (for example, transit card use) for individual i, T_i is an indicator for

[8] Multiple screener survey responses from the same person or from members of the same household were grouped and randomly assigned together.

i being offered the annual pass, $\mathbf{X}_i$ is a vector of controls measured at baseline,[9] and ϵ_i is the error term. The sample in this case is restricted to people who respond to survey screening questions that indicated they would not be eligible for the annual pass outside the study. Throughout the analysis, we calculate standard errors robust to heteroskedasticity.[10]

In this regression, $\hat{\beta}_0$ is the estimate of the change in outcomes resulting from being offered the annual pass as compared to being enrolled in the ORCA LIFT discounted fare program. As specified, the above equation delivers intent-to-treat (ITT) effects, which compares outcomes for all individuals offered the annual pass to those not offered the annual pass. If all clients offered the annual pass were to take it, then the ITT and the treatment-on-the-treated effect (TOT) – i.e., the effect of receiving the annual pass – would be identical. To the extent that there is incomplete take-up, we can rescale the ITT effect by take-up rates to estimate the TOT.

We also estimate a difference-in-differences specification. This specification is particularly useful when we analyze outcomes for people who receive annual passes because they are already eligible, rather than due to random assignment, though we estimate the model for both. We run difference-in-differences models of the form:

$$y_{it} = \alpha_1 + \beta_1 Post_t * T_i + \gamma_1 Post_t + \phi_1 T_i + \mathbf{X}_i \delta_1 + v_{it} \tag{2}$$

where y_{it} is the outcome for individual i in time t, $Post_t$ is an indicator for time t being after random assignment, and the remaining variables are defined as above. In this case, the β_1 coefficient captures standard difference-in-differences estimates of ITT treatment effects. Again, we can scale the ITT effects to obtain TOT effects using information on take-up rates of the subsidy. For panel data models with multiple observations per person, we estimate

[9]These controls include age, age squared, race dummies, transit frequency dummies (transit use 0, 1-3, 4-7 times in prior week) at baseline, and the corresponding outcome at baseline.

[10]While random assignment is technically at the household level, we do not cluster by household. We can only observe household membership in survey and county administrative records, not state administrative records, making it impossible to compute clustered standard errors in those records. In practice, very few participants are in the same household, and in regressions using datasets for which we can compute clustered standard errors, they are almost identical to heteroskedasticity robust standard errors.

standard errors clustered by person.

4 Data

4.1 Surveys

Our data are derived from several sources. As described above, screener and baseline surveys conducted prior to random assignment provide pre-treatment variables. The screener records variables related to study eligibility and basic demographic characteristics. The baseline survey focused heavily on pre-randomization measures of travel and health, with a small number of additional questions on employment, housing/neighborhood, and nutrition. Participants completed the baseline survey between May and December 2022. Participants were then contacted for two interim surveys, one in February 2023 and the other between March and May 2023. Both interim surveys served primarily to maintain and update contact information, though the second interim survey also measured a small number of short-run outcomes. Throughout this paper, we ignore the first interim survey and refer to these latter questions as "the interim survey." Finally, a one-year final survey (nearly identical to the baseline survey) was conducted during August to October 2023. Respondents were paid $50 to complete the baseline and final surveys and $10 for the interim touchpoints.

One primary focus of the surveys is travel behavior. Both the baseline and final surveys include a "travel day" module, asking respondents to talk through the preceding day chronologically, listing all locations they visited, where they traveled, trip duration, trip purpose, travel mode, and payment method. The surveys also include a simple question on the number of days respondents used transit in the past week. Due to its much shorter length, the interim survey only asks summary questions on the total number of trips completed by mode and a multiple response question on whether any trips in the prior day had particular purposes (e.g. "going to work").

The other focus of the surveys is respondent health and well-being. Our pre-analysis plan defines the Kessler 6 (K6) measure of mental distress (Kessler et al., 2003) as a primary outcome. The Kessler 6 is a standard measure used to screen for mental distress that runs on a scale from zero to 24, with higher values indicating more distress.[11] We ask the Kessler 6 in both baseline and final surveys. We also ask a simple, standard self-reported health status question (from poor to excellent) on the baseline, interim, and final surveys.

The study surveys have high and well-balanced response rates for a sample of people with low income in a major US metro area. The final survey had an 87% overall response rate, with rates of 89% in the treatment group, 86% in the control group, and 86% in the already eligible group. These high rates of response likely reflect a combination of intensive and responsive participant engagement by the survey firm, the $50 respondent payment, the survey's short length, and recruiting participants who already displayed a willingness to complete screener and baseline surveys. The overall response rate for the interim survey was 80%, with rates of 80%, 82%, and 78% in the treatment, control, and already eligible groups. These rates are somewhat lower, likely due to a much lower respondent payment, but still quite high.

4.2 Administrative Records

We link individuals in the study to a host of other administrative data. Using King County Metro records, we measure usage of ORCA cards by the primary survey respondent. ORCA cards are typical proximity cards that are tapped to terminals when boarding transit to pay the fare. Using card serial numbers for ORCA cards provided to all three groups, we can measure the number, time, and route of such taps. These taps include only trips paid for by study ORCA cards. Trips not associated with an ORCA card (cash payment, fare evasion, etc.) and travel by other modes are not included in this measure.

[11] The Kessler 6 module asks six questions of the form, "During the past 30 days, about how often did you feel..." various negative emotions. The question cycles through endings of nervous, hopeless, restless or fidgety, so depressed that nothing could cheer you up, that everything was an effort, and worthless. Respondents answer on a scale from "none of the time" to "all of the time", which we assign zero to four points and sum across questions.

We also match study records to linked Washington State administrative records for the original baseline survey respondents. The database we access includes records covering participation in state-administered public benefit programs (SNAP, TANF, etc.), contact with the criminal justice system (arrests tracked by the Washington State Patrol), employment and earnings (Unemployment Insurance records), and healthcare utilization (Medicaid claims data). Due to a combination of data confidentiality protections and the nature of the underlying records, we receive data at different frequencies: healthcare is annual, employment is quarterly, and other data are monthly. The database includes anyone who has enrolled in food, cash, or medical benefits in the state of Washington at any time over multiple decades. We match to this information using name and date of birth and then restrict the sample for this analysis to people who have some state administrative record prior to random assignment. Overall, we match 86% of study participants. Because they must be enrolled in cash benefit programs, 97% of the already eligible group matches. Among the RCT treatment and control groups, the rates are 85% and 83%, respectively; while lower, these rates are still quite high because treatment and control individuals have participated in food or medical benefits at some point in the past.

4.3 Descriptive Statistics

Table 1 provides descriptive statistics for the study sample based on questions asked in the screener and baseline surveys. The first column shows mean values for people randomly assigned to the control group, while the second and third columns show means for people randomly assigned to the treatment group as well as for those already eligible for an annual pass. The final two columns show differences between the treatment and control and differences between already eligible and control, along with heteroskedasticity robust standard errors (in parentheses).

Comparing columns (1) and (2), the characteristics of our control and treatment groups are very similar, consistent with randomization. The typical age of a participant in one of these groups is about 40, 60% are women, and just under half are non-White. The average participant has between 2 and 3 household members, and about 60% receive some form of in-kind or cash benefits from Washington State. Over 80% typically use transit at least one day per week, and close to 40% typically use it at least four days per week. They took about 2 trips in the prior day, about a third of which were by transit. They already rely on ORCA cards, with about three-quarters reporting using an ORCA card of some kind (either a full-fare ORCA card or a reduced-fare ORCA LIFT card) to pay for transit trips. With regard to health status, individuals in the randomized groups have average Kessler 6 scores between 8 and 9 (out of 24), which corresponds to moderate but not severe mental distress (Prochaska et al., 2012). Over 60% self-report having good or excellent health, while less than 20% report having a disability. The only statistically significant differences are in self-reported travel in the prior day (fewer in the treatment group) and in the Kessler 6 score (which is slightly higher in the treatment group). A small number of statistically meaningful differences like this are expected by chance, and the groups are otherwise very similar on observable characteristics. The differences that do appear happen to be for our primary outcomes. To be cautious in accounting for these baseline differences, we report multiple treatment effect estimates, including ones controlling linearly for lagged outcomes (as pre-specified) and ones based on a difference-in-differences specification.[12]

As expected, column (5) of Table 1 shows much greater differences between people who are already eligible for an annual pass and the participants in the RCT. Relative to the randomized groups, individuals in the already eligible group must be participating in one of six cash assistance programs (most commonly SSI and TANF) in addition to having income at or below 80% of FPL. Because of the mixture of programs, gender composition is similar. But individuals in the already eligible group tend to be older. They are also more likely to be Black and, mechanically, more likely to be receiving public benefits. Most notably, they are less healthy. The group of already eligible people has a higher Kessler 6 score

[12]We observe similar patterns of balance and imbalance among the set of people who complete follow-up surveys or match to administrative data. See Appendix Tables A2, A3, and A4.

(i.e., more psychologically distressed), worse reported overall health, and double the rate of disability. Perhaps due to these health issues, the already eligible group takes fewer trips overall, although they travel by transit at similar rates as participants in the RCT.

The differences in baseline characteristics between participants in the RCT and those in the already eligible group have two key implications for interpreting the results below. First, while it is reasonable to measure treatment effects by comparing levels of outcomes between the randomly assigned treatment and control groups, measuring effects for the already eligible group requires adjusting for selection into eligibility, which we do via difference-in-differences models. Second, the treatment and already eligible groups include very different populations, such that comparing treatment effects for these two groups provides a strong test for whether the results of the RCT are externally valid.

5 Results

5.1 Travel Outcomes

We find limited effects of the annual pass on travel behavior, including on transit use and mobility more generally. The results appear in Figure 1 and Table 2. Figure 1 shows mean travel outcomes over time, with each panel showing a different outcome. Table 2 presents the same set of outcomes, along with estimates of differences across groups based on equations (1) and (2). The first panel of Table 2 presents results from the interim survey, which most participants took approximately six months after study enrollment. While individuals in the treatment and already eligible groups reported more transit trips as well as more trips overall the day prior to completing the interim survey as compared to the day prior to completing the baseline survey, individuals in the control group also reported more transit and overall trips.[13] As a result, the differences between treatment and control are relatively muted. The

[13]Reported trip numbers in both groups increase in the interim survey. This increase appears to be due to question wording rather than seasonality. The brief interim survey only allowed for a summary travel question, rather than full travel day module. Neither administrative records on transit travel nor survey

regression results shown in columns (4) and (5) measure treatment effects within the RCT, first conditioning on pre-specified controls, including lagged outcomes, as in equation (1) and then implementing a difference-in-differences specification as in equation (2). Treatment effects of -0.102 and -0.044 for transit trips are negative, small, and statistically insignificant. The point estimate of the effect of free transit for the already eligible group is somewhat larger. Column (6) displays the difference-in-difference estimate from comparing already eligible to control, indicating 0.294 additional transit trips per day, a 20% increase. However, this moderate increase is not statistically significant in this smaller sample.

Turning to final survey outcomes in the second panel of Table 2, we similarly find little evidence of meaningful differences in travel behavior or transit use in the longer run. In fact, people across all groups report similar rates of transit use in the past week at 12 months after enrollment as they did at baseline. The one difference that is statistically significant in column (4) points to a negative effect of the treatment on transit use. The relationship also weakens in the difference-in-differences model, suggesting it likely reflects the randomly slightly greater travel by transit among the control group at baseline. Similarly, there are no signs of positive treatment effects on travel for the already eligible group, indicating any short-run increase in transit use for them has disappeared within one year.

We also find little evidence of persistent effects on transit use in administrative records generated by transit cards taps. Administrative data record only the subset of transit trips for which people pay with the study ORCA cards, but they are not subject to bias from survey attrition or respondent error. Figure 1.d shows the number of times study participants use their ORCA cards to board transit, by months since random assignment. Card taps are low initially until participants receive their cards in the mail. Starting at about three months after random assignment, both the treatment and already eligible groups begin using transit cards at moderately greater rates. These differences narrow over time. The final panel of Table 2 reports transit use across groups as measured by administrative data from King

questions that were consistent across waves, such as overall health rating, show this pattern.

County Metro on card taps. As in the graphical evidence, the treatment group uses transit cards somewhat more frequently 3-6 months after random assignment. The treatment group taps 0.059 more times per day, a difference that is marginally statistically significant. To be comparable with survey responses, the second row of the panel narrows the sample slightly to include only people for whom we observe follow-up survey responses, and the third row limits the outcome to card taps on the day covered by the final survey. Sample composition matters only slightly, but any treatment effect fades over time. A year after entering the study, we find little difference between treatment and control.[14] Overall, we observe a temporary increase in transit card use for the treatment group that fades over time. Combined with survey responses indicating no increase in transit use, these patterns suggest that the only effect of free transit for the treatment group is short-term shift in payment methods.

5.2 Downstream Outcomes

We do not observe improvements in our primary, pre-specified health outcomes. Table 3, Figure 2.a, and Figure 2.b report results for the first set of those outcomes based on self-reports in surveys. Consistent with the limited effects of the annual pass on mobility, we find little evidence of any downstream effects on self-reported physical or mental health. If anything, annual pass receipt is associated with slightly worse self-reported health, but the differences are not large. The relatively high K6 measure in treatment vs. control, and for already eligible vs. control, in Table 3 and Figure 2.b would indicate higher levels of mental distress for those who receive the annual pass vs. do not. However, the differences are not statistically significant and are sensitive to the inclusion of baseline characteristics. Overall, the results do not indicate that the annual pass leads to meaningful improvements in well-being by facilitating greater mobility.

Administrative records on other downstream outcomes similarly show no evidence of improvements. Figure 2.c shows trends in the annual likelihood of any Medicaid-paid medical

[14] We do not formally measure treatment effects for the already eligible because the administrative records do not include the baseline data necessary to estimate difference-in-difference models.

visit. In the four years prior to the study start, treatment and control group use of Medicaid services track each other, and they do not diverge in the year after the start of the study. The already eligible group's use of Medicaid services does diverge from that of the control group, but it exhibits both different trends and levels prior to the study, suggesting that difference-in-differences estimates should be interpreted with caution. Using state UI earnings records, Figure 2.d also shows little difference between treatment and control groups in paid hours worked before or after the intervention. Appendix Figure A4 shows similar trends for other outcomes in state administrative records.

Quantitative results for secondary outcomes from administrative records appear in Table 4. In Panel A, we see limited effects of the annual transit pass on the likelihood that individuals in the treatment group participate in State public benefits programs overall, and for SNAP and TANF specifically, as measured nine months after study enrollment. Participation in these programs among the already eligible is substantially higher than either treatment or control, potentially reflective of higher (or rising) participation at baseline that is not picked up by included controls. Panel B of Table 4 shows results for employment-related outcomes. Paid hours worked, employment rates, and earnings measured three quarters after study enrollment are substantially higher in the randomized groups relative to the already eligible group, but differences between treatment and control are very small and always statistically insignificant. In Panel C of Table 4, we also observe very muted effects of the annual pass on contact with the criminal justice system. The measures in Panel C are cumulated over the nine months following enrollment. The likelihood of either a misdemeanor, gross misdemeanor, or felony arrest is slightly lower among those in the treatment group than those in the control group, but we have limited statistical power to detect changes in these rare events such that any differences are not statistically meaningful. Arrest rates among those in the already eligible group are on par with those in the control group. Finally, Panel D shows results for healthcare utilization, as measured in Medicaid claim records. Those in the randomized groups tend to use less healthcare than those in the already eligible group in the year after study enrollment, but healthcare use among those in treatment and control groups are very similar in the post-enrollment period.

6 Comparison to Prior Studies

The experimental results on the effects of subsidized annual pass differ along some dimensions relative to the results on the effects of previous means-tested transit programs (Chizeck and Mbonu, 2025; Huberts et al., 2025; Brough, Freedman and Phillips, 2022, 2025). The starkest contrast is with Brough, Freedman and Phillips (2022, 2025), who randomized several months worth of free transit to public assistance clients in King County and found significant impacts of the subsidy on travel as well as some indication of positive impacts on participant health. There are several reasons our results could differ from this past work.

6.1 Different Treatments

One possibility is that the treatments between the present study and past studies differ. There are two main potential differences that could account for lower treatment effects here relative to in Brough, Freedman and Phillips (2022, 2025). First, they differ in the counterfactual fare. Our study reduces fares from \$1.00 to zero, while Brough, Freedman and Phillips (2022, 2025) decrease fares from \$1.50 to zero.[15] The pass in this study is also of longer duration, but that would imply, if anything, larger effects. Second, individuals in the current study did not take up the transit subsidy to the same extent as people in the previous study. In the prior study, enrollment and card receipt happened in person at public assistance offices throughout King County. As a result, 83% of people in the treatment group ever used their card. In the current study, cards were mailed to individuals in the treatment group as well as those in the already eligible group after confirming eligibility. It is possible cards were mistakenly disposed of before use, or that the subsidy was less salient by virtue

[15] In early 2022, King County lowered LIFT single-ride LIFT fares from \$1.50 to \$1.

of mail receipt. Because of this, only 67% of participants in the treatment group ever used their cards.[16] Thus, we estimate that 81% (.67/.83) of the treatment group ever received a card.

We compare across studies by computing the price-elasticity of transit use; that is, elasticities based on treatment-on-the-treated effects. For example, both studies use administrative records for trips taken by ORCA taps. In the current study, we observe an 0.059 intent-to-treat effect on taps per day, which corresponds to a 0.12 treatment-on-the-treated effect among the 81% who we estimate to receive cards. Given a 0.24 control group tap rate and a price change from \$1.00 to zero, this effect implies a price elasticity of 0.13.[17] Results from Brough, Freedman and Phillips (2022) do not require adjusting for incomplete take-up but otherwise can be calculated similarly. They observe a 0.9 increase in taps per day from a base of 0.23 for a \$1.50 price change, implying an elasticity of 0.66. Results for measures of transit travel that include payment methods other than ORCA cards are similarly different. Brough, Freedman and Phillips (2022) report both direct survey measures and combinations of administrative and survey records, which imply a price-elasticity of transit travel of either 0.08 or 0.35. The point estimates from our difference-in-differences specification imply a price-elasticity of the opposite sign, and even the 0.083 effect corresponding to the top end of the interview survey 95% confidence interval would imply a very low elasticity of demand of 0.04.[18]

The price elasticity of transit demand that we measure is also lower than that observed in other recent experiments studying the effects of subsidized transit fares. Chizeck and Mbonu (2025) and Huberts et al. (2025) test free versus half-price fare subsidies in RCTs. Results on card taps in Chizeck and Mbonu (2025) imply an elasticity of 0.55.[19] Results on

[16]See Appendix Figure A5.

[17]We compute arc elasticities based on the midpoint, specifically: $\frac{[.059/(.67/.83)]/[(0.24+0.24+.059/(.67/.83))/2]}{(1)/((1+0)/2)}$.

[18]We compute this as follows: $\frac{[.083/(.67/.83)]/[(1.21+1.21+.083/(.67/.83))/2]}{(1)/((1+0)/2)}$.

[19]We calculate this from their Table 3 as $\frac{[3.24/(.67/.83)]/[(.298+.298+3.24/(.67/.83))/2]}{2}$. Our elasticity differs from that reported in their Table 4 because we use the midpoint rather than the control mean as the base and assume imperfect take-up at a rate similar to our study.

total trips in Huberts et al. (2025) imply an elasticity of 0.52.[20] Overall, the price-elasticity that we observe in the current study is much smaller, perhaps by a factor of four, than that observed previously.

6.2 Different Study Populations

Another possibility is that the population served in the current experiment is different than the populations served in prior experiments. Again, the closest comparison is Brough, Freedman and Phillips (2022, 2025), whose randomized sample is very similar to ours on many dimensions. Both studies recruit people with low-income and demonstrated interest in public transit. Baseline data show that both study samples are already heavy transit users. They have mostly similar demographics, with median ages in the 40s and about half people of color. However, they do differ on other dimensions. Our participants are more likely to be female (59% vs. 41%) and less likely to be Black (15% vs. 28%). The prior experiment also enrolled people at the moment they were starting or renewing public benefits and so selects on likely having recently experienced a negative shock. As a result, in our randomized group, hours worked are twice as large and rates of SNAP receipt are half as large at the time of study enrollment, compared to the previous experiment. At the same time, though, employment history is similar across study samples a year or more before study enrollment. Altogether, it is possible that the more muted effects we observe stem from how price-elasticities of transit use vary across demographic groups or people's financial circumstances.

However, the fact that we do not observe dramatically different results for the RCT treatment group versus the already eligible group, despite the fact that they differ dramatically at baseline, suggests limited scope for heterogeneous effects. As noted above, the group of study participants who were eligible for the annual pass outside of the study were much more likely to be receiving public benefits, Black, and living with a disability. If transit demand

[20] We calculate this from their Figure 10 as $\frac{8.90/[(4.1+4.1+8.9)/2]}{2}$. This calculation assumes perfect take-up because participants receive cards in person and their usage rate of 77% in their Table 4 is similar to Brough, Freedman and Phillips (2022).

elasticities varied enough across groups to account for the different results across studies, we would not expect to see different results for an already eligible group and an RCT treatment group that differ so greatly. That we find similarly modest effects on transit use for such different groups suggests that differences in sample composition are unlikely to explain the discrepancies in results across studies.

Variation across sub-groups in their responsiveness to transit fares within the RCTs similarly does not explain much of the difference in results across studies. Brough, Freedman and Phillips (2022) find little evidence for heterogeneous treatment effects. We also test for heterogeneous effects in the current study. Figure 3 shows treatment effects of the annual transit pass on ORCA card taps for different sub-groups. The diamond in the top row plots our full-sample estimate, that card taps increase by 0.059 per day. The bars show confidence intervals; our full-sample estimate is statistically significant at the 10% level. The subsequent rows repeat this for particular sub-groups. We do see heterogeneity on some dimensions, such as stronger effects for people with disabilities, people of color, people living in neighborhoods with any bus stop, and people who used transit at all last week. However, these sub-group differences cannot explain differences across studies because participants in the two studies are similar on these dimensions. On most dimensions where the study populations differ, there are few signs of heterogeneity. For example, people who were receiving public benefits at baseline have results very similar to the full-sample results. In other words, it does not appear that the current RCT finds lower price-elasticities because it includes fewer public benefit recipients. Similarly, effects for Black participants are comparable to other race and ethnicity groups. Figure 3 does indicate very different effects for female and male participants, and 59% of our participants are female compared to 41% in Brough, Freedman and Phillips (2025). However, even if we assume that all observed effects on transit use comes from men and scale our estimates up proportionally to match their gender composition, our estimated elasticity increases only from 0.13 to 0.19, compared to the other study's estimate of 0.66. Thus, the variation in sub-group effects that we do observe does not appear to explain much of the difference across studies.

6.3 Different External Environment

Finally, the differences in results could stem from changes in the external environment in King County. Brough, Freedman and Phillips' (2022; 2025) study took place before COVID-19, whereas the current study takes place after. At the onset of the pandemic, fare collection in King County was paused to limit interactions between riders and transit employees and reduce cash handling. During that period, individuals may have become accustomed to not paying fares. Even once fare collection was reinstated, enforcement was limited. New norms may have therefore been established. If many individuals are evading fares, the effects of receiving a transit subsidy like the annual pass may have limited impacts on mobility, as the perceived financial cost of riding transit was zero regardless.

We can estimate fare evasion rates by comparing ORCA transit card use reported in surveys to that observed in administrative records, across study groups. Survey and administrative records may differ for many reasons, including fare evasion but also other things such as recall error (e.g., over-reporting trips that happened before the survey reference period). This appears in the data. Even though the treatment group has no incentive to evade fares, they report paying for 1.63 transit trips with an ORCA card for every record of an ORCA card boarding on the day prior to the survey (0.57 trips for 0.35 taps, in Table 2). The control group, which does have an incentive to evade fares, has a higher ratio of 2.00. If we credit the difference in those ratios to fare evasion, it implies that 23% ((2/1.63)–1) of trips in the control group involve evading fares.

Recent observational data from King County Metro also suggests that fare evasion is more common post-pandemic. In late 2024, Metro sampled payment rates and methods among boarding riders on selected bus and train routes via direct observation. They found that, on average, individuals evade fares for upwards of one-third of all trips – in line with our estimate above. By comparison, a 2010 study of fare evasion found that only 4.8% of adult riders in the county paid no or only partial fares for their transit trips (King County Department of Transportation, 2010). Both the 2024 and 2010 data collection efforts revealed heterogeneity by route, but both overall levels of evasion and disparities in evasion rates across routes were substantially higher post-pandemic.

The COVID-19 pandemic exacerbated fare evasion on transit systems around the world (Buehler et al., 2025). However, the relatively high rate of evasion in King County may also help to explain the differences in results with other experiments on the effects of free transit among similar populations that were also conducted post-COVID. For example, Chizeck and Mbonu (2025) estimate based on a post-endline survey that 11.5% of adult riders in their sample evaded fares – an arguably high rate of evasion, but substantially lower than in King County. To the extent that riders are comfortable evading fares, it effectively reduces their sensitivity to posted prices, as a substantial share pay zero regardless. This blunts the effectiveness of fare policy as a tool to improve access to employment opportunities and other urban amenities relative to, for example, expanding the transit network, increasing service frequency, improving reliability, or enhancing safety. However, in recognition of widespread evasion and its implications for horizontal equity on the system as well as fare box revenues, Metro has recently begun ramping up enforcement (after our sample period).[21] If Metro is successful in reducing evasion, fare policy could once again play a more important role in shaping travel patterns, particularly of individuals with low incomes.

7 Conclusion

We examine the effects King County, Washington's annual pass program, which provides free public transportation to individuals with low incomes. We leverage an RCT in which we compare mobility outcomes as well as downstream health, employment, and other outcomes for a group of individuals that met income but not other requirements for receiving the annual

[21]See, for example, https://www.seattletimes.com/seattle-news/transportation/metro-to-resume-fare-inspections-aboard-buses/.

pass. We supplement this analysis with a quasi-experimental approach that incorporates a group of individuals who met standard eligibility requirements and enrolled in a subsidized annual pass after being introduced to it through the study survey.

Using both administrative and survey data, we find that providing free transit has very limited effects on mobility or downstream outcomes related to health and well-being for the targeted population. This finding holds both in the RCT and in the quasi-experimental design.

The null effects we find contrast with the results of other recent RCTs on the impacts of means-tested free transit programs, including one studying a similar intervention in the same setting but prior to the COVID-19 pandemic. We rule out differences in the nature of the treatment, the study populations, and take-up rates as explanations for the differences in results. Changes in the external environment post-pandemic, including changes in norms around fare evasion, represent the leading explanation for the differences. These changes conspired to make demand for travel by transit among individuals with low incomes less price elastic during the study period.

Our results have critical policy implications. When fare evasion is common, fare policy adjustments will naturally have limited impacts on travel behavior. To equalize access to employment centers and other urban amenities in such an environment, transit authorities may instead need to invest in other aspects of their systems, such as more frequent or reliable service and improved safety. However, fare evasion presents fiscal challenges to transit systems. Already, some transit agencies have responded via heightened enforcement of fare payment to avoid continual declines in fare box revenues.[22] But this raises the additional challenge of ramping up enforcement in a way that does not have disparate impacts on disadvantaged communities.

Going forward, more research is needed to inform optimal transit fare policy after the COVID-19 pandemic. Policy decisions depend on whether key behavioral parameters, like

[22] For example, transit agencies in New York City, Boston, and Washington DC stepped up fare enforcement in the second half of 2024 in efforts to curb evasion (Ley and Bayya, 2024; Larson, 2024; Weiner, 2024).

the price-elasticity of transit, are persistently affected by COVID-era customs, revert back to pre-pandemic norms, or continue to evolve in a new way. This study highlights how quickly such parameters can change, even within the same location.

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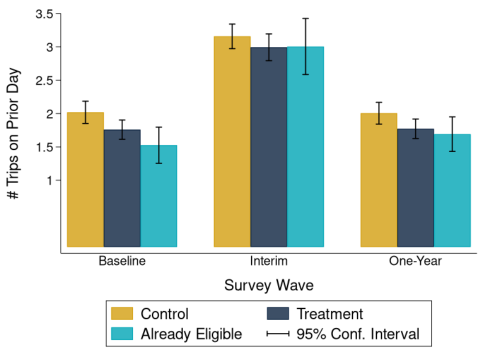
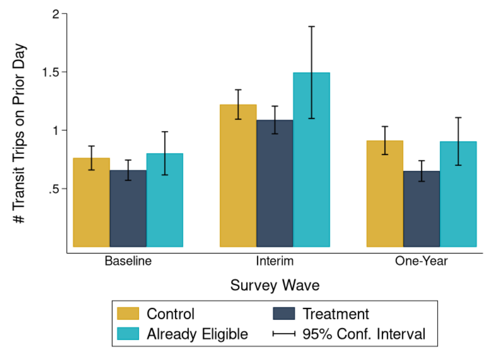
8 Tables and Figures

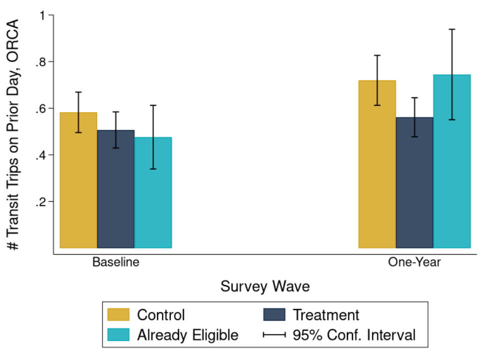
Figure 1: Mean Travel Outcomes



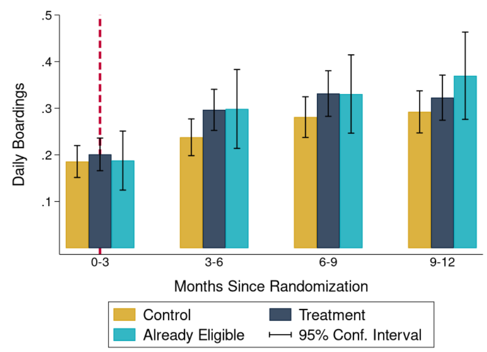
(a) All Trips - Survey



(b) Transit Trips - Survey



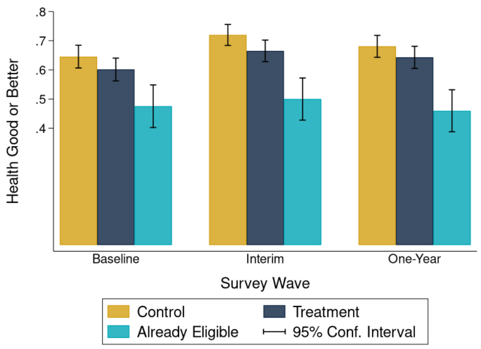
(c) ORCA Transit Trips - Survey



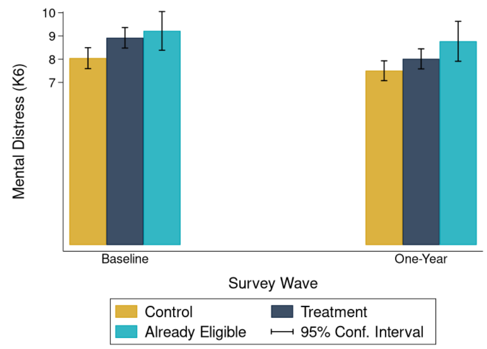
(d) ORCA Transit Trips - Admin

Notes: These figures display means of (a) all trips taken in the prior day, (b) all transit trips taken in the prior day, (c) transit trips paid by ORCA measured from survey data, and (d) transit trips paid by ORCA measured from administrative data. Means for the control group are shown as gold bars. Means for the treatment group are shown as navy bars. Means for the already eligible group are shown in turquoise bars with 95% confidence intervals shown in black vertical lines for each group. Figures (a), (b), and (c) display mean travel outcomes collected at baseline and during the interim and final survey waves. Figure (d) displays the average number of ORCA card taps from the King County Metro administrative data over the course of one year since enrollment. The red vertical line denotes the approximate time of enrollment for each study participant.

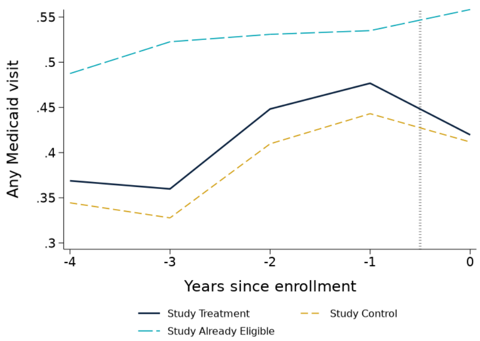
Figure 2: Downstream Outcomes



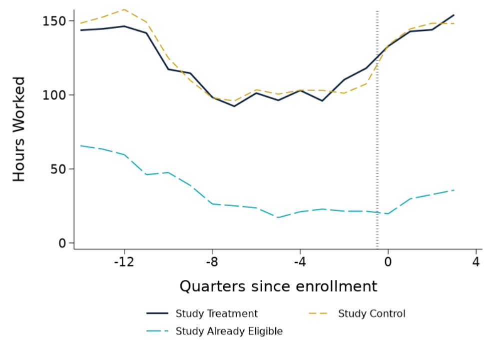
(a) Good or Excellent Health - Survey



(b) Mental Distress (K6) - Survey



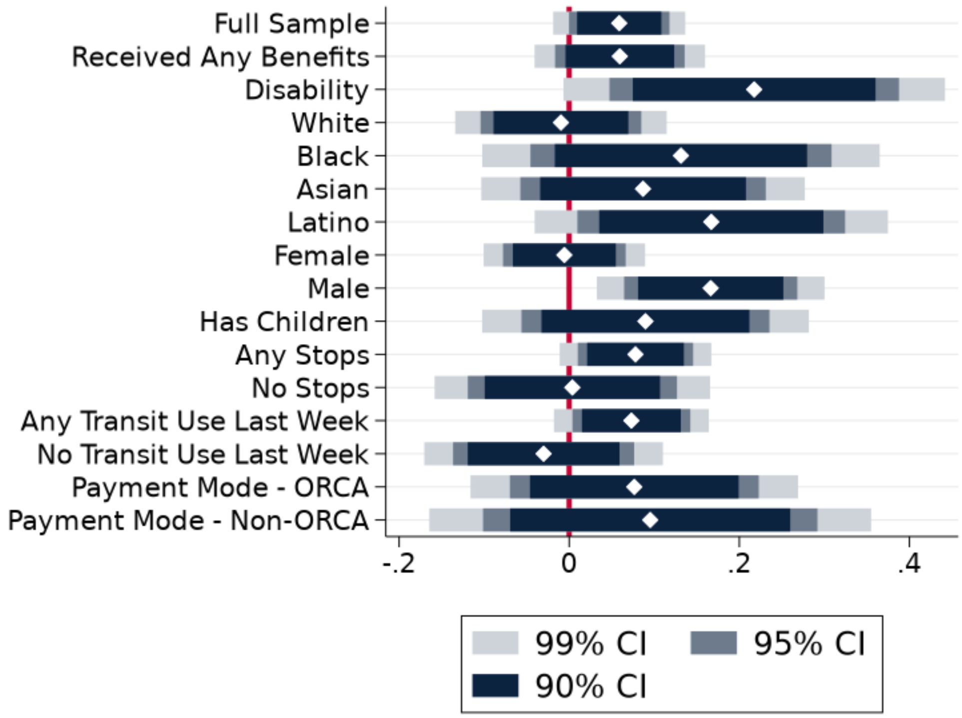
(c) Any Medicaid Visit - Admin



(d) Paid Hours Worked - Admin

Notes: The top two figures present downstream outcomes collected from surveys, displaying (a) the proportions of individuals who rated their health good or better and (b) means of the Kessler 6 (K6) index of mental distress. Means for the control group are shown as gold bars. Means for the treatment group are shown as navy bars. Means for the already eligible group are shown in turquoise bars with 95% confidence intervals shown in black vertical lines for each group. The bottom two figures display trends in (c) the proportion of individuals with any Medicaid visit and (d) mean of paid hours worked among those who matched with the DSHS RDA data. Means for the control group are shown in gold dashed lines. Means for the treatment group are shown in solid navy lines. Means for the already eligible group are shown in turquoise dashed lines. Any Medicaid visit (Health Care Authority) is measured at an annual frequency and paid hours worked (Washington State UI records) are measured at a quarterly frequency, with the vertical dashed line denoting the approximate time of enrollment.

Figure 3: Heterogeneous Effects



Notes: This figure displays heterogeneous treatment effects across subgroups, with point estimates represented by diamonds and corresponding confidence intervals at the 90% (dark blue), 95% (gray), and 99% (light gray) levels. The red vertical line at zero indicates no effect. Subgroups are defined based on demographic characteristics (e.g., race/ethnicity, gender, disability status), family composition, and baseline transit use behaviors. "Payment Mode – ORCA" and "Payment Mode – Non-ORCA" refer to the method of payment used at baseline. Confidence intervals that do not overlap zero indicate statistically significant subgroup effects at the respective confidence level.

Table 1: Baseline Characteristics, Full Sample

	Control Mean (1)	Treatment Mean (2)	Already Eligible Mean (3)	Treatment vs. Control (4)	Already Eligible vs. Control (5)
Age	39.79	40.46	45.74	0.664	5.950***
				(0.643)	(0.968)
Female	0.59	0.60	0.64	0.012	0.047
				(0.025)	(0.035)
White	0.53	0.53	0.58	0.006	0.051
				(0.025)	(0.036)
Asian	0.21	0.18	0.11	-0.022	-0.099***
				(0.020)	(0.024)
Hispanic or Latino	0.16	0.16	0.11	0.004	-0.053**
				(0.019)	(0.023)
Black or African American	0.15	0.14	0.27	-0.012	0.115***
				(0.018)	(0.031)
Household Size	2.22	2.26	2.11	0.040	-0.105
				(0.077)	(0.117)
Receives Any Benefits	0.61	0.60	0.97	-0.003	0.362***
				(0.025)	(0.021)
Used Transit 1-3 Days/week	0.44	0.43	0.44	-0.007	0.008
				(0.025)	(0.036)
Used Transit 4-7 Days/week	0.38	0.38	0.41	-0.003	0.030
				(0.025)	(0.036)
Want Lower Fares	0.50	0.46	0.46	-0.039	-0.037
				(0.025)	(0.037)
Trips in Prior Day	2.00	1.76	1.57	-0.242**	-0.437***
				(0.101)	(0.138)
Transit Trips in Prior Day	0.77	0.68	0.85	-0.088	0.082
				(0.061)	(0.095)
Trips Paid by ORCA	0.56	0.53	0.52	-0.026	-0.034
				(0.053)	(0.075)
Mental Distress (K6)	8.33	8.88	9.28	0.544*	0.947**
				(0.288)	(0.418)
Good or Better Health	0.63	0.60	0.46	-0.030	-0.173***
				(0.025)	(0.036)
Disability	0.18	0.16	0.40	-0.018	0.223***
				(0.019)	(0.034)
N	799	800	255	1599	1054

Notes: This table uses survey data collected at baseline during screening and intake to compare demographic characteristics, travel behavior, and mental and physical health among study participants in the treatment, control, and the already eligible group. Race/ethnicity are not mutually exclusive as participants were allowed to select all that apply. Columns (1), (2), (3) report means of the control group, treatment group, and the already eligible group, respectively. Column (4) reports the difference between treatment and control for each characteristic and column (5) reports the difference between already eligible and control. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table 2: Travel Outcomes

	Control Mean (1)	Treatment Mean (2)	Already Eligible Mean (3)	T vs. C w/Ctrls (4)	T vs. C Dif-in-Dif w/Ctrls (5)	A vs. C Dif-in-Dif w/ Ctrls (6)
A. Interim Survey Outcomes						
Trips in Prior Day	3.14	2.96	3.05	-0.140	0.073	0.337
				(0.133)	(0.173)	(0.273)
Transit Trips in Prior Day	1.21	1.08	1.53	-0.102	-0.044	0.294
				(0.079)	(0.104)	(0.223)
N	640	651	198	1291	2580	1676
B. Final Survey Outcomes						
Trips in Prior Day	1.90	1.74	1.66	-0.099	0.071	0.243
				(0.099)	(0.153)	(0.209)
Transit Trips in Prior Day	0.85	0.65	0.90	-0.180***	-0.103	0.015
				(0.066)	(0.095)	(0.146)
Trips Paid by ORCA	0.68	0.57	0.74	-0.092	-0.032	0.137
				(0.061)	(0.086)	(0.133)
No Transit Use Last Week	0.20	0.23	0.16	0.026	0.020	-0.014
				(0.020)	(0.031)	(0.042)
Used Transit 1-3 Days Last Week	0.44	0.43	0.47	-0.000	0.006	0.033
				(0.026)	(0.039)	(0.056)
Used Transit 4-7 Days Last Week	0.36	0.34	0.36	-0.026	-0.026	-0.020
				(0.023)	(0.037)	(0.054)
N	672	701	217	1373	2648	1716
C. Administrative Travel Outcomes						
Card Taps 3-6 months (Full)	0.24	0.30	0.30	0.059*		
				(0.030)		
Card Taps 3-6 months (Survey)	0.24	0.29	0.29	0.046		
				(0.032)		
ORCA Taps on Prior Day	0.34	0.35	0.48	0.001		
				(0.063)		
No Transit Use Last Week	0.74	0.73	0.69	-0.008		
				(0.024)		
Used Transit 1-3 Days Last Week	0.17	0.17	0.16	0.004		
				(0.020)		
Used Transit 4-7 Days Last Week	0.09	0.10	0.15	0.004		
				(0.016)		
N	674	704	212	1378		

Notes: This table reports treatment effects on daily transit use for the study sample. The first panel reports information collected from the interim survey, which was administered approximately 6 months after enrollment. The second panel reports information collected from the final survey, which was administered 1 year after enrollment. We only have payment method and transit use frequency information for the participants that took the final survey. Column 4 reports the difference between treatment and control for each outcome. Individual controls include age and age squared, gender, race and ethnicity dummies, days of transit use reported at baseline, and the value of the outcome at baseline. The final two columns report difference-in-difference estimates between treatment and control as well as already eligible and control, comparing the difference in means between groups during survey periods and at baseline. The last panel reports the number of trips derived from the King County Metro administrative data tracking transit card taps. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table 3: Health Outcomes from Surveys

	Control Mean (1)	Treatment Mean (2)	Already Eligible Mean (3)	T vs. C w/Ctrls (4)	T vs. C Dif-in-Dif w/Ctrls (5)	A vs. C Dif-in-Dif w/ Ctrls (6)
A. Interim Survey Outcomes						
Fair or Better Health	0.97	0.94	0.86	-0.022**	-0.028	-0.021
				(0.010)	(0.019)	(0.040)
Good or Better Health	0.72	0.67	0.51	-0.033	-0.018	-0.049
				(0.022)	(0.038)	(0.057)
N	640	652	198	1292	2496	1618
B. Final Survey Outcomes						
Fair or Better Health	0.96	0.94	0.87	-0.019*	-0.023	-0.011
				(0.011)	(0.020)	(0.038)
Good or Better Health	0.68	0.65	0.45	-0.010	0.014	-0.049
				(0.022)	(0.038)	(0.057)
Mental Distress (K6)	7.53	8.06	8.80	-0.023	-0.252	0.111
				(0.214)	(0.416)	(0.624)
N	661	680	208	1341	2558	1658

Notes: This table reports treatment effects on overall health rating and mental distress for the study sample. The top panel reports information collected from the interim survey, which was and the bottom panel reports information collected from the final survey. The Kessler 6 measure of mental distress was only asked in the final survey. Column 4 reports the difference between treatment and control for each outcome. Individual controls include age and age squared, gender, race and ethnicity dummies, days of transit use reported at baseline, and the value of the outcome at baseline. The final two columns report difference-in-difference estimates between treatment and control as well as already eligible and control, comparing the difference in means between groups during survey periods and at baseline. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

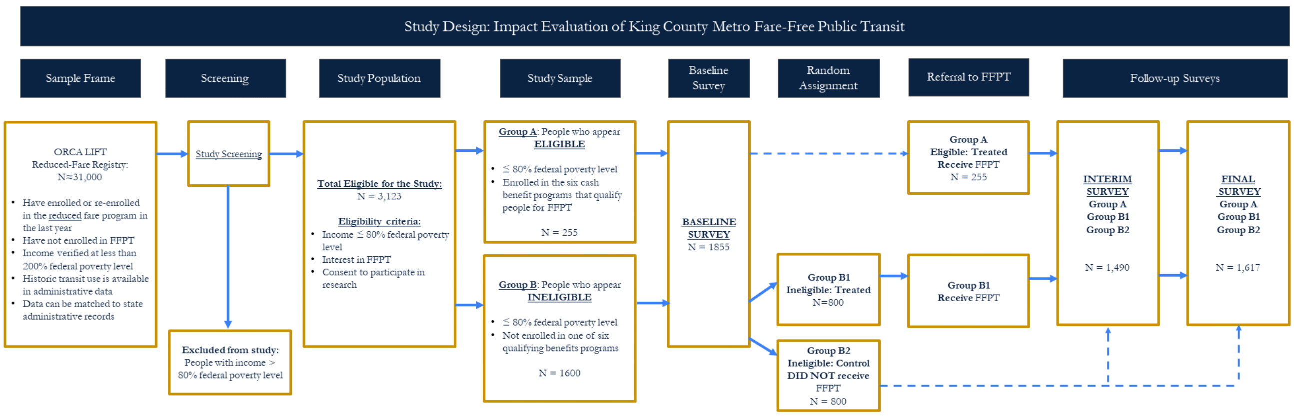
Table 4: Downstream Outcomes from Administrative Records

	Post				Pre - Post	
	Ctrl Mean	Treat Mean	Already Eligible Mean	T vs. C w/Ctrls	T vs. C Dif-in-Dif w/Ctrls	A vs. C Dif-in-Dif w/Ctrls
	(1)	(2)	(3)	(4)	(5)	(6)
A. Public Assistance, monthly, t = -9 to 9						
Any food or cash benefits	0.515	0.506	0.942	0.006	0.007	0.078***
				(0.015)	(0.016)	(0.016)
SNAP	0.460	0.462	0.924	0.014	0.014	0.084***
				(0.016)	(0.017)	(0.016)
TANF	0.021	0.010	0.176	-0.003	0.000	0.006
				(0.005)	(0.005)	(0.009)
Other	0.031	0.028	0.244	-0.004	-0.003	0.046***
				(0.006)	(0.006)	(0.016)
N	6590	6670	2430	13260	25194	17138
B. Employment, quarterly, t = -3 to 3						
Hours Worked	143.712	143.480	29.505	-3.079	-4.444	-32.231***
				(7.774)	(7.860)	(7.158)
Any Employment	0.484	0.470	0.170	-0.021	-0.025	-0.014
				(0.018)	(0.020)	(0.023)
Total Earnings	3915.784	3783.324	661.280	-115.337	-166.641	-1217.828***
				(246.141)	(241.876)	(206.001)
N	2636	2668	972	5304	9282	6314
C. Criminal Justice, monthly, t = -9 to 9						
Total Arrests	0.035	0.018	0.041	-0.018	-0.024*	-0.041*
				(0.013)	(0.014)	(0.023)
–Misdemeanor	0.002	0.000	0.004	-0.002	0.006	0.003
				(0.002)	(0.008)	(0.012)
–Gross Misdemeanor	0.020	0.013	0.025	-0.006	-0.011	-0.035
				(0.010)	(0.012)	(0.024)
–Felony	0.014	0.001	0.012	-0.011	-0.009	-0.019
				(0.009)	(0.009)	(0.018)
N	659	667	243	1326	2652	1804
D. Healthcare, annual, t = -1 to 0						
Cost to Medicaid ($)	1137.740	938.552	2003.454	-225.920	-278.010	-131.234
				(249.085)	(255.903)	(401.112)
Any Medicaid visit	0.412	0.420	0.558	0.009	-0.026	0.054
				(0.028)	(0.029)	(0.037)
Total preventative visits	1.240	1.292	1.750	0.044	-0.012	0.073
				(0.051)	(0.035)	(0.050)
Total prescriptions	7.492	7.626	17.879	0.052	-0.535	-0.494
				(0.902)	(0.544)	(1.015)
N	612	610	240	1222	2548	1754

Notes: This table presents means of downstream outcomes measured approximately one year after study enrollment among control, treatment, and already eligible groups in columns (1), (2), and (3), respectively, and regression-adjusted differences in means between the treatment and control groups in column (4), adjusting for individual controls and the relevant outcome 12 months prior to enrollment. Columns (5) and (6) report difference-in-difference estimates comparing mean outcomes between groups in the balanced period before and after random assignment. Public assistance receipt comes from Washington State Economic Services Administration records and is measured 10 months after random assignment. Employment information comes from the Washington State Employment Securities Department and is measured 4 quarters after random assignment. Criminal justice contact measures come from Washington State Patrol records and are measured cumulatively between random assignment and 10 months later. Healthcare information comes from Washington State Health Care Authority records on Medicaid claims and is also measured cumulatively between random assignment and 12 months later; cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Heteroskedasticity-robust standard errors are reported in parentheses.

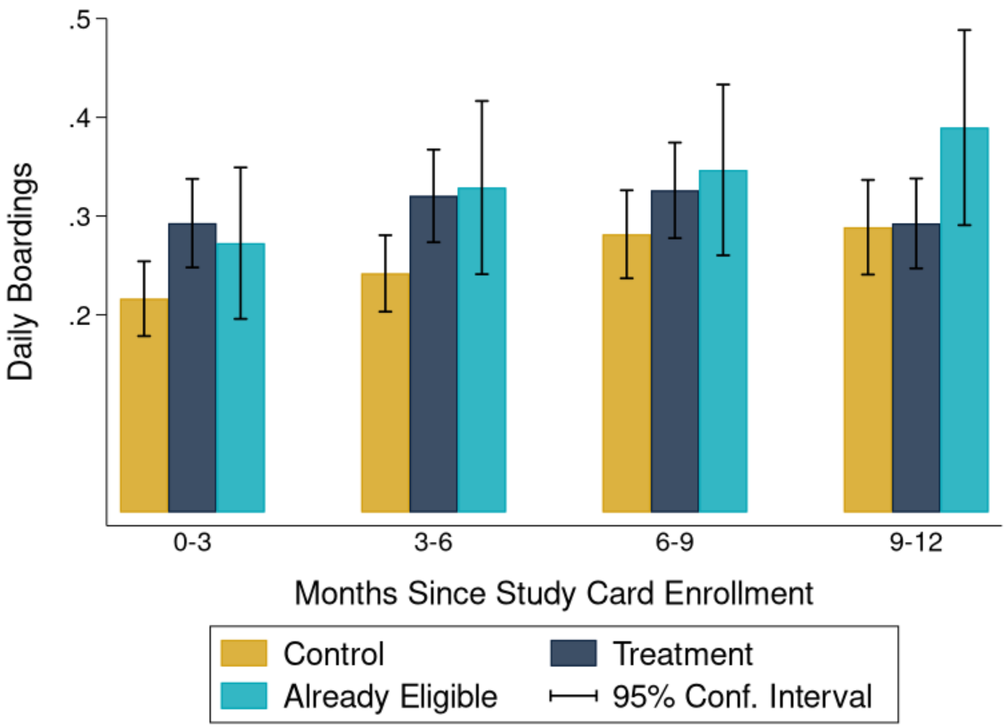
Appendix

Figure A1: Study Design and Enrollment



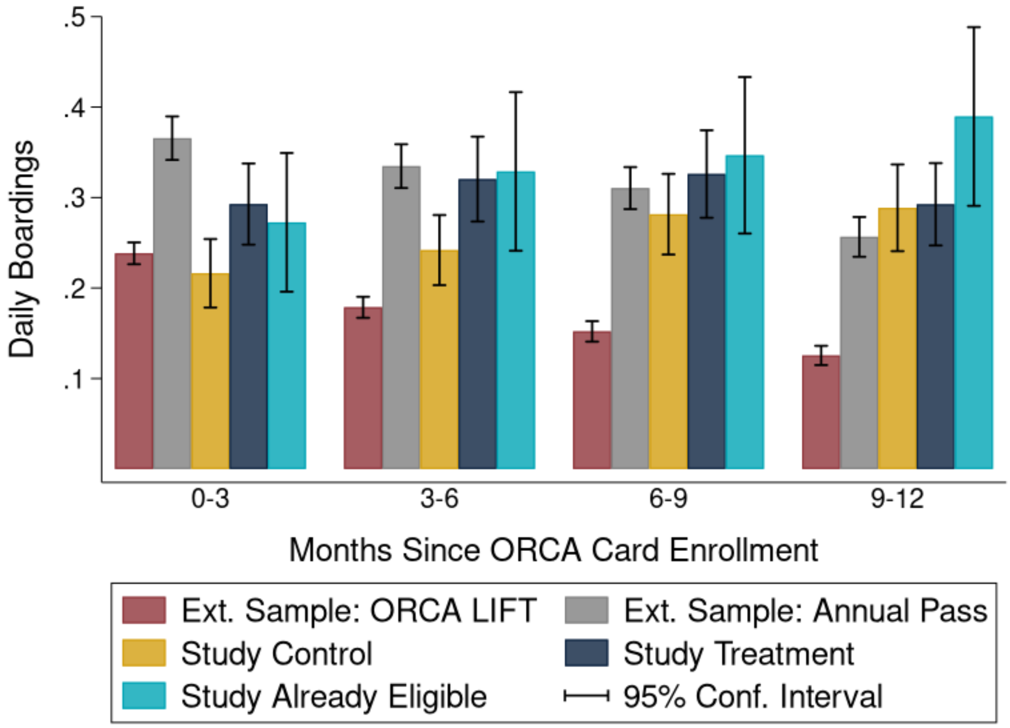
Notes: This figure outlines the enrollment and randomization process for the impact evaluation of King County Metro's Fare-Free Public Transit (FFPT) program. Sample sizes for each group is denoted along with the eligibility criteria.

Figure A2: Starting Date of Card Enrollment



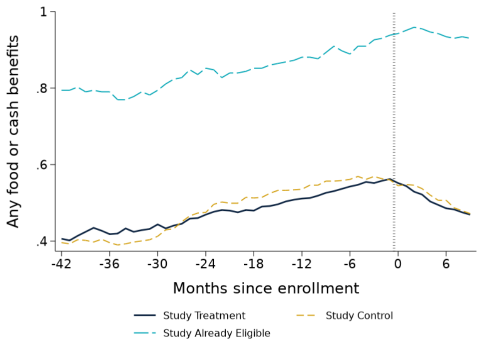
Notes: This figure displays the average number of daily boardings by study participants using ORCA card data from King County Metro, measured across four three-month intervals since study card enrollment. Means for the control group are shown as gold bars. Means for the treatment group are shown as navy bars. Means for the already eligible group are shown as turquoise bars. Black vertical lines represent 95% confidence intervals for each group.

Figure A3: Starting Date of Card Enrollment, Including Extended Samples

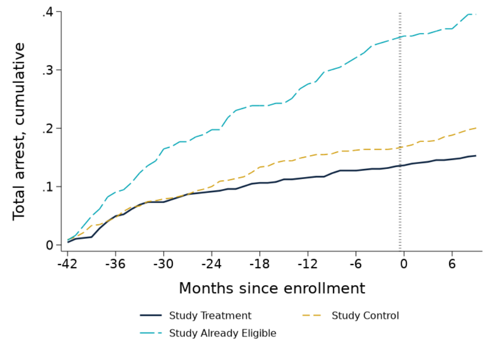


Notes: This figure displays the average number of daily boardings by study participant and the extended sample using ORCA card data from King County Metro, measured across four three-month intervals since study card enrollment. Means for the extended sample with an ORCA LIFT card are shown as dark red bars. Means for the extended sample with the subsidized annual pass are shown in gray bars. Means for the control group are shown as gold bars. Means for the treatment group are shown as navy bars. Means for the already eligible group are shown as turquoise bars. Black vertical lines represent 95% confidence intervals for each group.

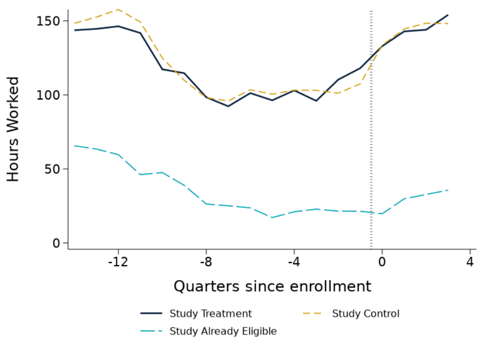
Figure A4: Downstream Outcomes



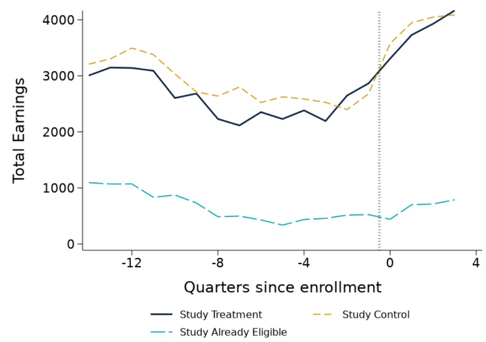
(a) Any Public Assistance



(b) Total Arrests, Cumulative



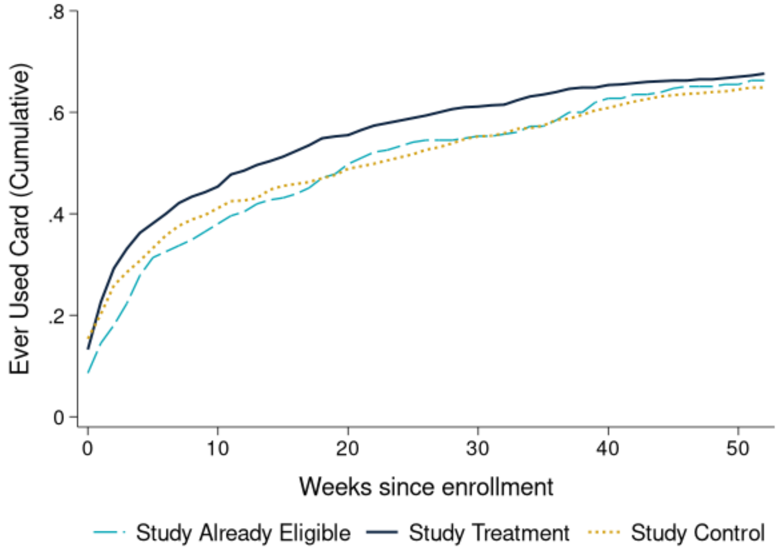
(c) Paid Hours Worked



(d) Total Earnings

Notes: The figures present downstream outcomes collected from administrative data, displaying (a) public assistance receipt (b) total arrests, (c) paid hours worked, and (d) total earnings. Means for the control group are shown in gold dashed lines. Means for the treatment group are shown in solid navy lines. Means for the already eligible group are shown in turquoise dashed lines.

Figure A5: Fraction Ever Using Transit Card, By Study Group



Notes: This figure displays the cumulative fraction of study participants who have ever tapped their transit card, by group assignment and weeks since enrolling the card. Outcomes measured using administrative records from King County Metro.

Table A1: Baseline Characteristics, Full Sample

	N	Control Mean	Treatment Mean	Already Eligible Mean	Treatment vs. Control	Already Eligible vs. Control
	(1)	(2)	(3)	(4)	(5)	(6)
Age	1854	39.79	40.46	45.74	0.664	5.950***
					(0.643)	(0.968)
Female	1838	0.59	0.60	0.64	0.012	0.047
					(0.025)	(0.035)
White	1842	0.53	0.53	0.58	0.006	0.051
					(0.025)	(0.036)
Asian	1842	0.21	0.18	0.11	-0.022	-0.099***
					(0.020)	(0.024)
Hispanic or Latino	1842	0.16	0.16	0.11	0.004	-0.053**
					(0.019)	(0.023)
Black or African American	1842	0.15	0.14	0.27	-0.012	0.115***
					(0.018)	(0.031)
Household Size	1845	2.22	2.26	2.11	0.040	-0.105
					(0.077)	(0.117)
Receives Any Benefits	1832	0.61	0.60	0.97	-0.003	0.362***
					(0.025)	(0.021)
Used Transit 1-3 Days/week	1781	0.44	0.43	0.44	-0.007	0.008
					(0.025)	(0.036)
Used Transit 4-7 Days/week	1781	0.38	0.38	0.41	-0.003	0.030
					(0.025)	(0.036)
Want Lower Fares	1780	0.50	0.46	0.46	-0.039	-0.037
					(0.025)	(0.037)
Trips in Prior Day	1852	2.00	1.76	1.57	-0.242**	-0.437***
					(0.101)	(0.138)
Transit Trips in Prior Day	1852	0.77	0.68	0.85	-0.088	0.082
					(0.061)	(0.095)
Trips Paid by ORCA	1852	0.56	0.53	0.52	-0.026	-0.034
					(0.053)	(0.075)
Mental Distress (K6)	1753	8.33	8.88	9.28	0.544*	0.947**
					(0.288)	(0.418)
Good or Better Health	1781	0.63	0.60	0.46	-0.030	-0.173***
					(0.025)	(0.036)
Disability	1756	0.18	0.16	0.40	-0.018	0.223***
					(0.019)	(0.034)

Notes: This table compares baseline characteristics of all study participants measured at intake. Column (1) reports the overall sample size across the three groups. Variation in sample sizes can be attributed to questions being skipped, especially when surveys were registered on paper. Columns (2), (3), and (4) report mean characteristics for the control, treatment, and already eligible group, respectively. Column (5) reports the difference between treatment and control for each characteristic and column (6) reports the difference between already eligible and control. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table A2: Baseline Characteristics, Restricted to Sample With Interim Survey

	N	Control Mean	Treatment Mean	Already Eligible Mean	Treatment vs. Control	Already Eligible vs. Control
	(1)	(2)	(3)	(4)	(5)	(6)
Age	1490	40.03	40.70	45.63	0.670	5.597***
					(0.718)	(1.112)
Female	1484	0.59	0.60	0.61	0.008	0.022
					(0.027)	(0.040)
White	1487	0.54	0.54	0.61	0.002	0.068*
					(0.028)	(0.040)
Asian	1487	0.21	0.19	0.11	-0.023	-0.106***
					(0.022)	(0.027)
Hispanic or Latino	1487	0.15	0.16	0.11	0.008	-0.038
					(0.020)	(0.026)
Black or African American	1487	0.15	0.15	0.25	-0.006	0.095***
					(0.020)	(0.034)
Household Size	1489	2.17	2.17	2.12	-0.001	-0.051
					(0.081)	(0.136)
Receives Any Benefits	1479	0.61	0.59	0.97	-0.022	0.360***
					(0.027)	(0.023)
Used Transit 1-3 Days/week	1441	0.45	0.44	0.49	-0.008	0.040
					(0.028)	(0.041)
Used Transit 4-7 Days/week	1441	0.37	0.36	0.39	-0.002	0.022
					(0.027)	(0.040)
Want Lower Fares	1441	0.50	0.48	0.44	-0.018	-0.052
					(0.028)	(0.041)
Trips in Prior Day	1489	1.99	1.74	1.54	-0.254**	-0.448***
					(0.109)	(0.157)
Transit Trips in Prior Day	1489	0.75	0.66	0.82	-0.092	0.075
					(0.066)	(0.106)
Trips Paid by ORCA	1489	0.57	0.51	0.48	-0.064	-0.089
					(0.057)	(0.081)
Mental Distress (K6)	1421	8.17	8.80	9.29	0.629**	1.117**
					(0.315)	(0.469)
Good or Better Health	1441	0.65	0.61	0.47	-0.037	-0.180***
					(0.027)	(0.041)
Disability	1427	0.17	0.16	0.41	-0.014	0.235***
					(0.021)	(0.039)

Notes: This table compares baseline characteristics of study participants that responded to the interim survey approximately 6 months after enrollment. Column (1) reports the overall sample size across the three groups. Variation in sample sizes can be attributed to questions being skipped, especially when surveys were registered on paper. Columns (2), (3), and (4) report mean characteristics for the control, treatment, and already eligible group, respectively. Column (5) reports the difference between treatment and control for each characteristic and column (6) reports the difference between already eligible and control. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table A3: Baseline Characteristics, Restricted to Sample With Final Survey

	N	Control Mean	Treatment Mean	Already Eligible Mean	Treatment vs. Control	Already Eligible vs. Control
	(1)	(2)	(3)	(4)	(5)	(6)
Age	1617	40.20	40.33	45.75	0.136	5.550***
					(0.691)	(1.062)
Female	1613	0.61	0.61	0.64	-0.001	0.033
					(0.026)	(0.037)
White	1616	0.53	0.53	0.60	0.002	0.072*
					(0.027)	(0.038)
Asian	1616	0.21	0.19	0.10	-0.025	-0.106***
					(0.021)	(0.026)
Hispanic or Latino	1616	0.15	0.16	0.11	0.009	-0.038
					(0.019)	(0.025)
Black or African American	1616	0.15	0.14	0.25	-0.010	0.095***
					(0.019)	(0.032)
Household Size	1617	2.20	2.23	2.14	0.038	-0.056
					(0.080)	(0.128)
Receives Any Benefits	1607	0.61	0.60	0.96	-0.017	0.351***
					(0.026)	(0.023)
Used Transit 1-3 Days/week	1558	0.45	0.43	0.45	-0.016	0.004
					(0.027)	(0.039)
Used Transit 4-7 Days/week	1558	0.37	0.38	0.39	0.008	0.025
					(0.026)	(0.038)
Want Lower Fares	1558	0.50	0.46	0.46	-0.042	-0.043
					(0.027)	(0.039)
Trips in Prior Day	1616	2.00	1.78	1.53	-0.226**	-0.470***
					(0.108)	(0.148)
Transit Trips in Prior Day	1616	0.77	0.67	0.79	-0.101	0.023
					(0.065)	(0.099)
Trips Paid by ORCA	1616	0.57	0.52	0.48	-0.051	-0.086
					(0.056)	(0.077)
Mental Distress (K6)	1537	7.99	8.93	9.18	0.934***	1.183***
					(0.305)	(0.441)
Good or Better Health	1559	0.64	0.61	0.46	-0.033	-0.178***
					(0.026)	(0.039)
Disability	1538	0.17	0.16	0.40	-0.015	0.235***
					(0.020)	(0.037)

Notes: This table compares baseline characteristics of study participants that responded to the final survey approximately 12 months after enrollment. Column (1) reports the overall sample size across the three groups. Variation in sample sizes can be attributed to questions being skipped, especially when surveys were registered on paper. Columns (2), (3), and (4) report mean characteristics for the control, treatment, and already eligible group, respectively. Column (5) reports the difference between treatment and control for each characteristic and column (6) reports the difference between already eligible and control. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table A4: Baseline Characteristics, Restricted to Sample Matched with State Admin Data

	Control Mean (1)	Treat Mean (2)	Already Eligible Mean (3)	Treat vs. Ctrl (4)	Already Eligible vs. Ctrl (5)
A. Baseline Characteristics					
White	0.460	0.463	0.449	0.003	-0.011
				(0.027)	(0.037)
Asian	0.161	0.135	0.086	-0.026	-0.074***
				(0.020)	(0.023)
Latino	0.090	0.112	0.049	0.023	-0.040**
				(0.017)	(0.018)
Black	0.112	0.118	0.193	0.006	0.081***
				(0.018)	(0.028)
Female	0.643	0.642	0.654	-0.002	0.011
				(0.026)	(0.036)
Used Transit 1-3 Days Last Week	0.186	0.188	0.143	0.002	-0.042
				(0.022)	(0.028)
Used Transit 1-3 Days Last Week	0.433	0.438	0.443	0.004	0.010
				(0.028)	(0.038)
Used Transit 4-7 Days Last Week	0.381	0.375	0.414	-0.006	0.033
				(0.027)	(0.037)
N	630	640	237	1270	867
B. Secondary Outcomes at Baseline					
Years of Education	10.823	11.074	12.780	0.251	1.957***
				(0.303)	(0.289)
Owns Vehicle	0.215	0.202	0.173	-0.013	-0.043
				(0.022)	(0.029)
Any food or cash benefits	0.560	0.562	0.938	0.002	0.378***
				(0.027)	(0.025)
SNAP	0.516	0.522	0.922	0.006	0.406***
				(0.027)	(0.026)
TANF	0.018	0.004	0.173	-0.014**	0.155***
				(0.006)	(0.025)
Other	0.021	0.022	0.243	0.001	0.222***
				(0.008)	(0.028)
Total arrest, cumulative	0.017	0.021	0.086	0.004	0.004
				(0.008)	(0.008)
–Misdemeanor	0.009	0.001	0.008	-0.008	-0.008
				(0.008)	(0.008)
–Gross misdemeanor	0.014	0.016	0.062	0.003	0.003
				(0.008)	(0.008)
–Felony	0.006	0.000	0.021	-0.006	-0.006
				(0.004)	(0.004)
C. Employment, 1 Quarter Before Enrollment					
Hours Worked	107.469	118.055	21.432	10.587	10.587
				(9.972)	(9.972)
Any Employment	0.439	0.448	0.128	0.010	0.010
				(0.027)	(0.027)
Total Earnings	2681.683	2866.561	523.412	184.878	184.878
				(278.698)	(278.698)
C. Healthcare, 1 Year Before Enrollment					
Months eligible for Medicaid	9.508	9.643	11.247	0.135	0.135
				(0.249)	(0.249)
Cost to Medicaid ($)	1092.616	1169.142	2092.679	76.526	76.526
				(199.003)	(199.003)
Any Medicaid visit	0.443	0.477	0.535	0.034	0.034
				(0.027)	(0.027)
N	659	667	243	1326	1326

Notes: This table reports means of demographic characteristics and secondary outcomes at baseline for study participants in the control, treatment, and already eligible groups in columns (1), (2), and (3), respectively, and report differences between treatment and control in columns (5) differences between control and already eligible in column (6). Panel A reports baseline characteristics of the full survey sample collected at intake. Panel B reports non-time variant characteristics (years of education and owning vehicle) maintained by the Economic Services Administration (ESA) and secondary outcomes of the sample that matched to the Washington State DSHS RDA records. Public benefits use is measured at one month before enrollment and criminal justice outcomes are measured cumulatively over 12 months before enrollment. Panel C summarizes employment outcomes measured one quarter before enrollment. Panel D summarizes healthcare use measured one year before enrollment. Standard errors reported in parentheses are heteroskedasticity robust. Statistically significant at *10%, **5%, and ***1%.

Table A5: UI Earnings Outcomes

	Post				Pre - Post	
	Ctrl Mean	Treat Mean	Already Eligible Mean	T vs. C w/Ctrls	T vs. C Dif-in-Dif w/Ctrls	A vs. C Dif-in-Dif w/Ctrls
	(1)	(2)	(3)	(4)	(5)	(6)
Employment, quarterly, t = -3 to 3						
Hours Worked	143.712	143.480	29.505	-3.079	-4.444	-32.231***
				(7.774)	(7.860)	(7.158)
Any Employment	0.484	0.470	0.170	-0.021	-0.025	-0.014
				(0.018)	(0.020)	(0.023)
Total Earnings	3915.784	3783.324	661.280	-115.337	-166.641	-1217.828***
				(246.141)	(241.876)	(206.001)
Number of Employers	0.604	0.594	0.207	-0.032	-0.037	-0.021
				(0.026)	(0.028)	(0.033)
Number of New Employers	0.151	0.153	0.096	-0.007	-0.023	0.041*
				(0.013)	(0.018)	(0.023)
N	2636	2668	972	5304	9282	6314

Notes: This table presents earnings and employment outcomes from the Washington State Employment Securities Department measured approximately four quarters after study enrollment. Columns (1), (2), and (3) presents means in the post-period among control, treatment, and already eligible groups, respectively, and column (4) presents regression-adjusted differences in means between the treatment and control groups, adjusting for individual controls and the relevant outcome 12 months prior to enrollment. Columns (5) and (6) report difference-in-difference estimates comparing mean outcomes between groups in the balanced period before and after random assignment.

Table A6: Public Benefits

	Post				Pre - Post	
	Ctrl Mean	Treat Mean	Already Eligible Mean	T vs. C w/Ctrls	T vs. C Dif-in-Dif w/Ctrls	A vs. C Dif-in-Dif w/Ctrls
	(1)	(2)	(3)	(4)	(5)	(6)
Public Assistance, monthly, t = -9 to 9						
Any food or cash benefits	0.515	0.506	0.942	0.006	0.007	0.078***
				(0.015)	(0.016)	(0.016)
SNAP	0.460	0.462	0.924	0.014	0.014	0.084***
				(0.016)	(0.017)	(0.016)
TANF	0.021	0.010	0.176	-0.003	0.000	0.006
				(0.005)	(0.005)	(0.009)
Other	0.031	0.028	0.244	-0.004	-0.003	0.046***
				(0.006)	(0.006)	(0.016)
N	6590	6670	2430	13260	25194	17138

Notes: Criminal justice contact measures come from Washington State Patrol records and are measured cumulatively between random assignment and 10 months later. Healthcare information comes from Washington State administrative records on Medicaid claims and is also measured cumulatively between random assignment and 12 months later; cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Heteroskedasticity-robust standard errors are reported in parentheses.

Table A7: Healthcare Outcomes

	Post				Pre - Post	
	Ctrl Mean (1)	Treat Mean (2)	Already Eligible Mean (3)	T vs. C w/Ctrls (4)	T vs. C Dif-in-Dif w/Ctrls (5)	A vs. C Dif-in-Dif w/Ctrls (6)
Healthcare, annual, t = -1 to 0						
Months eligible for Medicaid	9.469	9.510	11.342	0.154	-0.083	0.144
				(0.250)	(0.159)	(0.176)
Eligible for Medicaid/Medicare	0.029	0.048	0.258	0.016	0.002	0.027**
				(0.010)	(0.008)	(0.012)
Cost to Medicaid ($)	1137.740	938.552	2003.454	-225.920	-278.010	-131.234
				(249.085)	(255.903)	(401.112)
Any Medicaid visit	0.412	0.420	0.558	0.009	-0.026	0.054
				(0.028)	(0.029)	(0.037)
Any emergency visits	0.212	0.220	0.338	0.005	-0.015	0.014
				(0.023)	(0.027)	(0.039)
Any non-emergency visits	0.783	0.805	0.950	0.026	-0.008	0.051**
				(0.023)	(0.018)	(0.023)
– Index of non-ER visits (0-4)	1.227	1.261	1.563	0.035	-0.039	0.104*
				(0.047)	(0.045)	(0.063)
Received flu shot	0.064	0.062	0.092	-0.002	0.003	0.025
				(0.014)	(0.021)	(0.031)
Total preventative visits	1.240	1.292	1.750	0.044	-0.012	0.073
				(0.051)	(0.035)	(0.050)
Total prescriptions	7.492	7.626	17.879	0.052	-0.535	-0.494
				(0.902)	(0.544)	(1.015)
Diagnosed for mental illness	0.400	0.402	0.629	0.003	-0.044*	0.003
				(0.027)	(0.025)	(0.035)
Any mental health prescriptions	0.310	0.328	0.388	0.019	-0.012	0.014
				(0.026)	(0.021)	(0.027)
Treatment for mental health	0.288	0.303	0.467	0.020	-0.015	0.052
				(0.026)	(0.024)	(0.034)
Treatment for substance use	0.054	0.046	0.138	-0.007	0.005	0.014
				(0.012)	(0.010)	(0.018)
N	612	610	240	1222	2548	1754

Notes: This table presents earnings and employment outcomes from the Washington State Health Care Authority records on Medicaid Claims measured cumulatively between random assignment and 12 months later. Cost to Medicaid reflects expected costs based on visit type, as in Finkelstein et al. (2012). Columns (1), (2), and (3) presents means in the post-period among control, treatment, and already eligible groups, respectively, and column (4) presents regression-adjusted differences in means between the treatment and control groups, adjusting for individual controls and the relevant outcome 12 months prior to enrollment. Columns (5) and (6) report difference-in-difference estimates comparing mean outcomes between groups in the balanced period before and after random assignment.

Table A8: Criminal Justice Outcomes

	Post				Pre - Post	
	Ctrl Mean	Treat Mean	Already Eligible Mean	T vs. C w/Ctrls	T vs. C Dif-in-Dif w/Ctrls	A vs. C Dif-in-Dif w/Ctrls
	(1)	(2)	(3)	(4)	(5)	(6)
Criminal Justice, monthly, t = -9 to 9						
Total Arrests	0.035	0.018	0.041	-0.018	-0.024*	-0.041*
				(0.013)	(0.014)	(0.023)
–Misdemeanor	0.002	0.000	0.004	-0.002	0.006	0.003
				(0.002)	(0.008)	(0.012)
–Gross Misdemeanor	0.020	0.013	0.025	-0.006	-0.011	-0.035
				(0.010)	(0.012)	(0.024)
–Felony	0.014	0.001	0.012	-0.011	-0.009	-0.019
				(0.009)	(0.009)	(0.018)
N	659	667	243	1326	2652	1804

Notes: This table presents earnings and employment outcomes from the Washington State Patrol measured approximately 10 months after study enrollment. Columns (1), (2), and (3) presents means in the post-period among control, treatment, and already eligible groups, respectively, and column (4) presents regression-adjusted differences in means between the treatment and control groups, adjusting for individual controls and the relevant outcome 12 months prior to enrollment. Columns (5) and (6) report difference-in-difference estimates comparing mean outcomes between groups in the balanced period before and after random assignment.