The Impact of Subsidized Ride-Hailing on Employment: Results from an RCT

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Abstract

In models of labor supply, a reduction in commute cost increases labor force participation. To test this prediction, we implement an RCT in a population with tenuous labor force attachment: women who have dependent children and have no reliable access to a car. Study participants are assigned to receive $200 in monthly ride-hailing credits for one month (control group) or six months (treatment group). We estimate the effect on employment during a three-month period in which the treatment group, but not the control group, receives the credits. The estimated treatment effect is 0.054 (SE 0.027) on a base of 0.597.

Keywords: Labor Supply; Transportation and Employment; Commuting Costs; Randomized controlled trial

JEL: J22, R48

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1 Introduction

An elementary implication of standard models of labor supply is that high monetary and time commuting costs deter labor force participation (Cogan, 1981). However, there is little compelling direct evidence on this prediction due, we suspect, to challenges in implementing robust research designs with available observational data.

We present some of the first experimental evidence on the impact of reducing commute costs on labor force participation. Our study involves a randomized controlled trial aimed at economically disadvantaged women with dependent children who lack regular access to a car—a group for which even modest commuting costs can pose a significant barrier to employment. Participants are randomly assigned to receive \$200 in monthly ride-hailing credits for one month (control group) or six months (treatment group). We evaluate the impact on employment using quarterly unemployment insurance (UI) records, focusing on the three-month period during which only the treatment group continued to receive credits.

The monthly \$200 subsidy provided the possibility of a meaningful reduction in commute costs. On average, the employed women in our sample worked about 13 days a month and usually commuted by public transit. The \$200 in credits was sufficient to cover round-trip ride-hailing commutes for approximately six of those days. On days when women used ride-hailing rather than public transit for their commute, recipients would have saved the transit fare and, more importantly, about 90 minutes in commute time.

We find that, as predicted, the offer of the more generous six-month stream of ride-hailing credits increases employment. The estimated treatment effect is 0.054 (SE 0.027) on a base employment rate of 0.597 in the control group.

Our results are related to a literature that estimates the impact of access to a car on employment, inasmuch as our experiment provides an option of commuting by car (via ride-hailing, on at least some work days) to a group of individuals who would otherwise have commuted by some other transportation mode (typically bus). To facilitate a comparison of our study with the results reported in that literature—in which authors typically report the local average treatment effect (LATE) of car access on employment—we estimate the LATE of ride-hailing take-up on employment. Our estimated LATE, 0.098 (SE 0.049), is moderately lower than the LATE of car access on women's employment reported in the pioneering work of Raphael and Rice (2002).

Our experiment provides a cleanly identified test of the prediction that a reduction in commute costs increases employment.[1] Our results also offer a useful input for potential public policy interventions, and more generally, our work contributes to the large literature on transportation-related barriers to employment among economically vulnerable populations (Kain, 1968).

2 Background

We begin by setting out a simple version of the standard labor supply model. The model demonstrates that a reduction in commute costs increases employment. It also provides a structure for interpreting our experiment, and in addition allows us to highlight the difficulty of identifying the causal effect of commute costs on employment when using non-experimental methods.

2.1 A Simple Theoretical Structure

Consider an individual who is endowed with L_0 hours of time and R dollars of non-labor income per day, and who has human capital h, which allows her to earn an hourly wage $w(h)$ in the labor market. She derives utility from y, dollars available for consumption, and "leisure" $L \leq L_0$, which are hours devoted to activities other than commuting and working. For the sake of clarity, we assume utility takes a quasi-linear form:[2]

$$U(y, L) = y + \theta u(L) - \varepsilon_c, \tag{1}$$

where $\theta > 0$, $u'(L) > 0$ and $u''(L) < 0$, and ε_c is a money-metric utility cost component (positive or negative) associated with commuting.[3] Let H be the number of hours worked if the individual is employed. Then (1) is maximized subject to the constraints,

$$\begin{array}{lll} y = w(h)H + R - p_c \;\text{ and }\; L = L_0 - t_c - H & \text{if employed, and} \\ y = R \;\text{ and }\; L = L_0 & \text{if not employed,} \end{array} \tag{2}$$

[1]Heckman (1979) famously argues that for women participation in the labor market (the extensive margin of labor supply) is far more important than the number of hours worked by those who are employed (the intensive margin). The predicted effect of commute time on the intensive margin is ambiguous, but we also evaluate the effect of our treatment on earnings, which are affected by both margins.

[2]This makes it easy to illustrate how the labor supply decision is driven by the preference for leisure (relatively high-θ individuals have a stronger preference for leisure), and also simplifies analysis by ruling out income effects. This simplification, relative to a more general utility function, does not change predictions about the effect of commute costs on the labor force participation decision.

[3]In the textbook model, $\varepsilon_c = 0$, which implicitly assumes the only costs to commuting are the monetary cost and the opportunity costs of time (priced at the market wage). We generalize slightly by allowing for additional variation in individual preferences for commuting (e.g., some may enjoy driving to work while others find it stressful).

where p_c is the monetary cost of commuting and t_c is the corresponding time cost. If the individual chooses to work, her indirect utility is

$$V^{\mathrm{W}} = w(h)H^* + R - P_c + \theta u(L_0 - t_c - H^*), \tag{3}$$

where $H^*(h, \theta, t_c)$ is the optimal number of work hours and $P_c \equiv p_c + \varepsilon_c$ is the full money-metric utility cost of commuting.[4] V^{W} is compared to the utility achieved by not working,

$$V^{\mathrm{NW}} = R + \theta u(L_0). \tag{4}$$

We can characterize the labor force participation decision as follows: Let $\hat{w}$ be the "reservation wage," i.e., the wage at which the individual is indifferent between working and not working, $V^{\mathrm{W}} = V^{\mathrm{NW}}$. The individual then chooses to work if her market wage $w(h)$ exceeds the reservation.

The effect of commute costs on employment: Comparative statics with respect to commute costs, in elasticity form, are as follows:[5]

$$\tfrac{d\hat{w}}{dP_c}\tfrac{P_c}{\hat{w}} = \tfrac{P_c}{\hat{w}H^*(\cdot)} > 0, \quad \text{and} \quad \tfrac{d\hat{w}}{dt_c}\tfrac{t_c}{\hat{w}} = \tfrac{w(h)t_c}{\hat{w}H^*(\cdot)} > 0. \tag{5}$$

A reduction in either the time or the monetary cost of commuting reduces the reservation wage, thereby increasing participation in the labor force at the margin. A simple numerical example illustrates. Suppose we have a population of workers who pay a daily commute cost of $P_c = \$8$. Each individual works 8 hours per day and has a reservation wage $\hat{w} = \$10$ (and therefore earns $w(h) \geq \$10$). Equation (5) shows that the elasticity of $\hat{w}$ with respect to P_c is the ratio of the daily commute cost to the daily income earned at the reservation wage, so this elasticity is 0.10. Thus, if P_c were to decrease by 50%, the reservation wage decreases by 5%, inducing workers with $\$9.50 \leq w(h) < \$10$ to enter the labor market.

In short, a reduction in the monetary commute cost increases employment, and the magnitude of the effect depends on the fraction of the population whose market wages are sufficiently close to their reservation wage. The same logic holds for the time cost of the commute.

Alternative commute transportation modes: In our experiment, assignment to treatment does not simply lower the price of the usual transportation mode for commutes (typically bus), but instead provides an *alternative* transportation mode, ride-hailing, which

[4] $H^*(h, \theta, t_c)$ solves $w(h) = \theta u'(L_0 - t_c - H^*)$, i.e., is chosen so as to equate the wage and the marginal rate of substitution between income and leisure.

[5] To establish these results, we note that the reservation wage $\hat{w}$ equates (3) and (4), and use the implicit function theorem: $\frac{d\hat{w}}{dP_c} = \frac{1}{H^*(\cdot)} > 0$ and $\frac{d\hat{w}}{dt_c} = \frac{w(h)}{H^*(\cdot)} > 0$.

for many individuals saves both time and money. Our expectation, in line with the above theory, is that this treatment will increase employment among those who use the alternative transportation mode.

To fix this idea, we extend our theoretical approach to include multiple transportation modes. We now slightly modify the notation: let the transportation mode M have time cost t_c^{M}, and a full money-metric utility cost P_c^{M}. Suppose, for instance, that an individual can commute by bus (B) or car (C). Then the choice of commute mode boils down to a comparison of $[w(h)t_c^{\mathrm{C}} + P_c^{\mathrm{C}}]$ and $[w(h)t_c^{\mathrm{B}} + P_c^{\mathrm{B}}]$. As a conceptual matter, the employment decision is made by first deducing the optimal commute mode (if one chooses to work) and then making the utility-maximizing employment choice as described above.

Figure 1 illustrates this. Consider first panel (a), which depicts the maximization problem for an individual with relatively low human capital h.[6] For the person depicted, the daily time cost of commuting by car is lower than commuting by bus ($t_c^{\mathrm{C}} < t_c^{\mathrm{B}}$), but the full monetary cost is higher ($P_c^{\mathrm{C}} > P_c^{\mathrm{B}}$). Taking both costs into account, she chooses to take the bus to work; the budget constraint for transportation mode B lies above the budget constraint for transportation mode C. Note that in this example, the individual is indifferent between working, providing $H^* = L_0 - t_c^{\mathrm{B}} - L^*$ hours, and not working.

Now consider people who, similar to the individual depicted in panel (a), are close to indifferent between working and not working, and commute by bus if they do work. And suppose that these people are randomized into the treatment arm of our experiment. The treatment provides a potentially cheaper transportation mode C (transport by car via ride-hailing). For many, though not necessarily all, of these individuals, the relevant budget constraint lies above the budget constraint they faced when commuting by bus.[7] For such individuals, commute costs have declined, leading some individuals to now strictly prefer employment to non-employment. Put another way, our treatment assigns individuals a new budget constraint that has the same slope as their old budget constraint, but that typically lies above the old budget constraint, as would be the case with any treatment that effectively reduces the time and/or monetary cost of commuting.

In Figure 1, panel (b) shows an individual who has the same preferences as the individual in panel (a) and faces the same time and monetary commute costs, but who has a higher level of human capital h and therefore has a higher market wage $w(h)$. Notice that this

[6]Her human capital is clearly lower than her counterpart in panel (b) as she has a flatter budget constraint, which has slope $-w(h)$.

[7]Why might the new budget constraint *not* lie above the old budget constraint? Recall that the cost of commuting by ride-hailing includes a money-metric utility cost term that differs between individuals and might be large for some people, so an individual might be so averse to ride-hailing that she genearlly prefers to commute by bus even when ride-hailing is free.

high-h individual commutes by car,[8] and the utility she achieves by participating in the labor market far exceeds her utility from nonparticipation.

We draw attention to the contrast between panels (a) and (b) to make an important point: we generally expect to observe a positive correlation between car access and employment in the labor market, simply because individuals with relatively high human capital disproportionately choose to work *and* tend to use more time-efficient modes of transportation, which in most locations means commuting by car. This characteristic of the labor market greatly complicates efforts to infer a causal effect of access to a car on employment.

Income effects: Ride-hailing services provided to study participants can be used for commuting to work *or* for any other purpose. Consider an individual in the treatment group who uses ride-hailing strictly *for purposes other than commuting to work.* For this person, treatment conceptually increases non-labor income from R (the pre-treatment level) to $R + \Delta$, where Δ is the money-metric consumption value of the free ride-hailing service. Now, if preferences are quasi-linear, as assumed in our model above, the incremental increase in V^{W} and V^{NW} are identical, so the reservation wage is unchanged. Treatment therefore has no effect on labor force participation. However, in a more general case, leisure is assumed to be a normal good (Cogan, 1981). If leisure is indeed normal, our treatment, which provides a valuable unconditional benefit, potentially increases V^{NW} by more than V^{W}, thereby reducing the incentive to participate in the labor market.

We suspect that income effects, arising from the consumption value of treatment, are negligible in our context.[9] However, if income effects are relevant to some study participants, they work *against* our hypothesis that treatment increases labor force participation. In this sense, our estimated treatment effect is conservative; it represents a lower bound estimate of the effect of a ride-hailing benefit that is restricted to work commutes.

Expected effects of treatment on earnings: Our treatment is expected to increase labor supply on the extensive margin, which naturally leads to the expectation that treatment will increase average earnings. However, in a general model such as Cogan (1981), the effects of a reduction in commute costs on the intensive margin (for example, work hours per day) are ambiguous and therefore the effects on earnings are also ambiguous. In our empirical analysis, we examine the effect of our treatment on quarterly earnings according

[8] Although the time costs of commuting by car or bus are the same for the individuals in panel (a) and panel (b), the opportunity costs differ because the individual in panel (b) has a higher market wage.

[9] The idea that an unconditional transfer decreases labor force participation is studied in the remarkable work of Vivalt et al. (2024), who evaluate the impact on labor force participation of a $1,000 per month cash transfer allotted to low-income families. This treatment was found to reduce labor market participation by about 2.0 percentage points. Of course, the monthly value of the treatment in our experiment is far lower; it is at most $200 per month, and as we show below it is less than $100 for most study participants (i.e., less than one tenth as large as in their experiment).

to UI records. However, we do not have data on hours worked, so we cannot evaluate the intensive margin in more detail.

2.2 Extant evidence on commute costs and employment

The broad theoretical predictions of our model align well with the characteristics of real-world labor markets. Individuals with high human capital tend to have higher labor force participation than those with low human capital, high-human capital workers disproportionately commute by car, and individuals who live in places with longer average commute times have relatively lower labor force participation. These latter patterns doubtlessly reflect the endogeneity of commute patterns with respect to individuals' human capital and to their residential location choices.[10]

The prediction that commute costs deter employment, specifically, has received a lot of attention. For example, Bastiaanssen et al. (2020) systematically review 93 studies, all of them nonexperimental, that examine commute costs and employment, including 42 studies of the effect of car access on employment. Bastiaanssen et al. (2020) characterize the body of results as generally "mixed" and "not conclusive or consistent." Mixed findings in this literature are due, in part, to challenges in implementing strong research designs with available data. There are few naturally occurring sources of exogenous variation in commute costs suitable for cleanly identifying causal effects on labor force participation.

Consider, in particular, studies of the effect of car access on employment. As noted above, our model highlights the inherent difficulties facing researchers who seek to estimate this effect. A positive association between car access and employment is anticipated simply because people with higher levels of human capital are more likely to participate in the labor market *and* generally favor faster modes of transportation, most commonly commuting by car.[11] As shown in the important work of Raphael and Rice (2002), it may still be possible to tease out the effect of car access on employment with an instrumental variables (IV) strategy. Using local car insurance rates and gas taxes as instruments for car access, they estimate an economically meaningful and statistically significant effect of car ownership for women (but not for men). Other studies adopt this same approach, including Bansak et al. (2010), Ong (2002), Baum (2009), and Bee (2009), all of which find that car access increases employment. However, there are well-known limitations to the IV approach: the

[10]The finding that labor force participation is lower in places with longer commutes comes from Black et al. (2014), who study the participation of the labor force among married women. The authors acknowledge that causality probably goes two ways: long commute times likely deter labor force participation *and* families that plan to have one parent stay out of the labor force (often the mother) likely sort into neighborhoods with longer commute times, which typically have lower-priced housing.

[11]Complicating matters further, these same factors will shape where people choose to reside and work.

assumptions necessary for IV estimation are strong and generally untestable, and the LATE may not be the treatment effect of primary interest.

Our experimental study builds on the existing quasi-experimental literature. To date, there appear to be few comparable studies that have randomly varied access to a car. Several RCT's have tested the employment effects of public transportation subsidies. In the first such study, Phillips (2014) randomly provided low-income job seekers in Washington D.C. with a $50 prepaid transit card, finding a temporary increase in job search intensity. In Brough et al. (2025), transit fares were reduced from $1.50 to free for a sample of public benefits recipients in Seattle. They find this price intervention to have little effect on labor outcomes.[12] In contrast, Chizeck and Mbonu (2025) run a similar fare discount experiment and find an increase in employment and earnings among participants who were unemployed at the start of the treatment.[13]

3 The Experiment

3.1 Study Design and Sample Characteristics

Our experiment takes place in Allegheny County, Pennsylvania, which contains Pittsburgh and surrounding suburbs. Study participants were recruited primarily by text messages sent to recipients of social services from the Allegheny County Department of Human Services (ACDHS). We recruited the study sample this way in order to target disadvantaged mothers. We focus on this population because of its policy relevance and because the literature suggests that labor force participation among economically-disadvantaged mothers is particularly sensitive to transportation costs.

Enrollment took place on a rolling basis between September 2019 and June 2022. Prospective participants were directed to a study website, and those who were interested signed a consent form and answered a short set of questions to establish eligibility. Applicants were screened out if they were not female, did not have at least one dependent minor-aged child, or if they self-reported having reliable access to a car. When possible, participant eligibility was checked using ACDHS administrative records.

Eligible applicants were randomly assigned to treatment or control immediately upon submitting their application, and were notified of their randomization outcome. Participants

[12]However, Brough et al. (2025) report "suggestive evidence that transit subsidies improve measures of financial and physical health," which serves as an important reminder that improved transportation access can improves welfare for reasons other than increased access to the labor market.

[13]Christensen and Osman (2025) conduct an experiment that randomly assigns ride-hailing price discounts to riders in Cairo, Egypt. They find a strong demand response to price reductions among female riders, but do not examine the effects on labor outcomes.

then completed a second consent form that contained the standard terms of service for using the ride-hailing app. Finally, the participants completed an optional baseline survey with questions about demographics, employment status, and transportation use.

Participants also received instructions to download a smartphone app that contained a GPS tracking function to collect data on their spatial mobility, a platform for administering optional follow-up study surveys, and a list of resources for job-seekers (e.g., contact information for local employment and training programs). All participants received $200 in ride-hailing credits for use over the course of a month. Those assigned to the treatment arm received $200 per month in credits for five additional months. The monthly allotments could not be carried over to a subsequent month.

We enrolled 1,022 study participants. Among those individuals, we were able to link 902 to Pennsylvania UI wage records.[14] Participants who could not be matched to UI records are excluded from the analyses in this paper. Table 1 describes the baseline characteristics of our 902 UI-linked study participants. Participants are quite young and have relatively low levels of education. About 90% receive Medicaid and 80% receive SNAP benefits. These high rates of public assistance are as expected, given our channel for recruiting participants. The treatment and control groups are well-balanced on key characteristics, including on the pre-treatment measures of our focal labor market outcomes.

Fewer than 60% of study participants have any paid employment in the calendar quarter prior to enrolling in the study. Those who were employed in this quarter have relatively low earnings, averaging $4,558 ($1,519 per month). As shown in Panel C, among the survey respondents who reported commuting to work, most spend more than 30 minutes per day commuting and many spend more than an hour. The most commonly reported transportation mode is public transit (67.4%). Over half (58.4%) of participants reported using ride-hailing apps at least occasionally.

3.2 Results

Table 2 shows participants' use of the offered ride-hailing credits. Take-up of the ride-hailing opportunity was quite low—about 68% for the treatment group and 59% for the control group.[15] As expected, those in the treatment group (who received six months' worth of ride-hailing credits) took about six times as many rides and spent about six times as

[14] The linkage to UI records is based on social security number. The 120 failed matches were primarily due to the individual not having a social security number on file with ACDHS.

[15] These relatively low take-up rates likely stem in part from the administrative steps that participants had to complete before gaining access to their credits. These steps included completing multiple consent forms, downloading an app, and enabling various settings on their phone to allow the study app to collect GPS location histories.

many ride-hailing credits as those in the control group (who received only one month of ride-hailing credits). Due to administrative error, 11.7% of control group members were allowed to take more than $200 worth of trips, with 2.6% taking more than $300 worth of trips.

The first row of Panel A in Table 3 presents our estimates of the treatment effect on employment during the first full calendar quarter after the quarter in which the woman enrolled in the study.[16] The estimate in column (1) is from a regression in which the dependent variable is an indicator variable for employment status (1 if UI records show any employment in the quarter, 0 otherwise), and independent variables are an indicator for treatment status (1 if treated, 0 if in control) and quarter-of-enrollment indicator variables, which capture time-varying labor market conditions over the duration of the study. In regression (2), our preferred specification, we additionally control for the individual's employment status in the quarter prior to enrollment, which should increase the precision of our estimates. The offer of the longer-duration ride-hailing subsidies increases employment by 5.4 percentage points, a meaningful increase relative to the control group's employment rate of 59.7 percent.

We also estimate the effect of the treatment on average quarterly earnings in the first quarter after enrollment. Given a treatment effect on employment of 0.054, a back-of-the-envelope calculation suggests that average earnings might increase by approximately $300.[17] As it turns out, estimated treatment effects on earnings are imprecise. Confidence intervals include $300, but also include 0. Our experiment is underpowered for the purpose of estimating the effects on earnings, and therefore we do not perform additional analyses of these treatment effects.

The theory in Section 2.1 shows that the employment response to subsidized ride-hailing will be concentrated among individuals who are close to indifferent between working and not working. With this in mind, Panel B reports a *post hoc* analysis in which we estimate treatment effects for three sample subgroups defined by their employment status during

[16] In UI records, employment outcomes are measured at the quarterly level. The treatment group received $200 worth of ride-hailing credits per month for six months. Thus, if a woman assigned to treatment enrolled in January and received her first monthly allocation of credits in February, her last month of treatment would be July. The first complete calendar quarter after enrollment for this woman is April through June. A comparable woman in the control group would not have credits available during that quarter. However, there are some control participants who received limited credits during the quarter in which we measure employment status. For example, a control individual who enrolled in March might receive some credits during April. Also, as noted above, a small number of individuals in the control group erroneously received credits for more than one month (and one individual received credits for six months). Such cases create a slight downward bias in our estimates of the treatment effect.

[17] If the earnings of women drawn into employment by treatment were the same as the women in the control group, average earnings would increase from $3,353 to $\frac{0.597+0.054}{0.597} \times \$3,353 = \$3,656$, i.e., increase by about $300. However, theoretical considerations lead us to suspect that marginal women will have relatively lower human capital and therefore lower earnings than infra-marginal workers.

the four quarters prior to enrolling in the study: those continuously employed (4 quarters), those intermittently employed (employed 1–3 quarters) and those not employed (employed 0 quarters). Women who were intermittently employed prior to enrollment seem most likely to be close to indifferent between working and not working, so it is not surprising to see that the treatment effect is concentrated in this group.[18]

As we have discussed, much of the literature on the effect of car access on employment uses a LATE framework. With this in mind, we estimate a LATE in our context that is at least somewhat comparable: we estimate the LATE of "free ride-hailing use" on employment, where the randomized assignment in the experiment is the instrument for free ride-hailing use (i.e., ride-hailing *take-up*). Our benchmark definition of take-up is that the person took at least $300 worth of subsidized trips during the entire active subsidy period.[19]

Angrist (2006) and Angrist et al. (2012) show how the LATE approach can be useful in the evaluation of experiments. Individuals conceptually fall into three groups: (1) regardless of their experimental assignment, *never takers* do not take up the offered treatment, in our case, ride-hailing; (2) *compliers* take up ride-hailing if and only if they are assigned to the treatment arm; and (3) regardless of assignment, *always takers* find a way to take up ride-hailing.[20] The LATE identifies the impact of treatment on compliers.

Table 4 presents our analysis. Columns (1) and (2) report a regression in which employment status is regressed on treatment take-up. There is no causal interpretation to these regressions; they simply report associations, roughly comparable to the descriptive regressions of employment on car access. Columns (3) and (4) report our LATE estimates. Treatment status is, of course, a strong predictor of take-up; we have very high first stage F statistics. The LATE estimates indicate that, among compliers, car access increases employment by roughly 10 percentage points.[21]

As with the "intent to treat" estimates reported in Table 3, the positive LATE impacts on employment are concentrated among participants who had intermittent labor force attachment in the four quarters prior to joining the study (Panel B in Table 4). The estimated LATE for this group of women is around 0.19.

[18]The subgroups analyzed are mutually exclusive. Sample sizes do not add up to the total sample size (in panel A) because we lack full pretreatment UI records for 29 participants.

[19]Notice that by this definition, a person in the treatment group is said to "take up" the treatment if they average $50 in rides per month—a fairly low level. As we demonstrate below, our estimates are not sensitive to this (rather arbitrary) definition of take-up.

[20]Because of administrative error, we have a small number of always takers in our experiment. These are individuals assigned to the control group who nonetheless managed to recieve ride-hailing credits that extend beyond one month—usually one additional month, but in rare cases even more.

[21]The LATE estimates are robust to several alternative definitions of take-up, including: taking at least six rides in the first quarter after enrollment; taking at least 12 rides during the study; and taking at least $150 worth of rides during the first full calendar quarter after enrollment (Appendix Table A1).

By way of comparison, the LATE of car ownership on women's employment probability in Raphael and Rice (2002) is 0.166 (SE 0.086). The measures of car access in two studies are very different—they have a measure of car ownership and ours is ride-hailing access that at most covers a portion of commutes made by a typical study participant.[22] Also, we study rather different populations, inasmuch as we focus on disadvantaged mothers while they study women more generally. Nonetheless, it turns out that our LATE estimate of the effect of car access is only moderately lower than the quasi-experimental benchmark in Raphael and Rice (2002).

We note that labor market disruptions due to the COVID-19 pandemic may have affected the impact of car access on employment. While only 91 of the 902 study participants received their ride-hailing subsidies during the peak period of economic disruption in March through December of 2020, we still cannot rule out the possibility that the effects of subsidized ride-hailing trips would have differed had we conducted our experiment under more typical economic conditions. More broadly, further experimental research is required to gauge the generalizability of our results to other populations and locations.

4 Conclusion

This study provides one of the first experimental tests of the link between commute costs and employment. We find, as predicted by standard theory, that a reduction in commute costs in the form of subsidized ride-hailing credits causes an increase in employment that is both economically and statistically significant.

The increase in employment, 5.4 percentage points, comes at a high cost. Among those in the treatment arm, the monthly cost is \$101 per person during the three-month period when the experimental contrast is evaluated. The primary reason for this high cost is that treated individuals were able to use the allocated ride-hailing credits however they liked—for commuting or for any other purpose. The per-person cost of treatment would likely have been much lower if ride-hailing usage had been restricted to work commutes. After all, those who were employed in the quarter used \$107 per month in credits, but those without

[22] The full \$200 monthly ride-hailing subsidy constitutes about half of the level of car access (at least for commuting purposes) that one gets from owning a personal vehicle. The participants' ride-hailing trips cost an average of \$2.76 per mile, and it seems that employed participants had a mean daily round-trip commuting distance of about 11.5 miles (on the basis of estimates using smartphone GPS data from 323 participants for whom we were able to identify their home and work locations). A \$200 monthly ride-hailing subsidy therefore covers the average participant's commute for about 6.3 days per month. Participants who were employed in the quarter prior to joining the study had mean earnings of \$4,558 in that quarter, i.e., \$1,519 per month. If these individuals work 8 hours per day and earn \$15/hour, they work $\$1{,}519/(15 \times 8) = 12.66$ days per month. For someone who uses the entire subsidy for commuting, the subsidy thus covers 6.3/12.66 work days, i.e., about half of work days.

paid employment (who obviously used no credits to commute to formal employment) used \$91 per month in credits. Furthermore, even among those who had paid for work, many ride-hailing credits were probably used for non-commuting purposes.[23] A program that allots ride-hailing commute credits (e.g., a company benefit providing limited ride-hailing for eligible employees) might increase employment at a much lower cost.

We were surprised that the use of ride-hailing for commuting was generally low among those who were employed, but upon reflection that outcome seems sensible. Some workers likely had a fine usual mode of commuting, perhaps a short walk or a convenient bus ride, and simply preferred to use the ride-hailing credits for other purposes. Even those with a less convenient commute necessarily had to plan the use of ride-hailing credits as a *backup* for their regular transportation mode and may also have found a high value of the credits for other uses.

It appears that our treatment increased employment because it provided a critical backup means of transport, which is plausibly important on days when there are unexpected problems (bad weather, a late child sitter or an overcrowded bus). Our experiment demonstrates that for economically disadvantaged women with children and no access to a car, the availability of ride-hailing for occasional use was sufficient to move a significant number of women from non-employment into the labor market.

Ride-hailing services, both traditional and autonomous, will likely play an increasingly important role in the future of commuting. Urban planners envision that as autonomous technology and smart infrastructure mature, ride-hailing will be both a key standalone service and a connector in the broader transit network (which might include, for example, a system of autonomous shuttles). Continued empirical inquiry will be necessary to fully understand the broad societal impacts of the resulting changes in transportation access. Our work takes a first step by showing that the availability of flexible on-demand transport at a low cost can improve employment prospects for those who are marginally connected to the labor market.

[23] Evidence comes from an analysis of a rather small subset of employed workers in the treatment group, for whom we can track rides using GPS data, and for whom we know the location of employment. For this group, we can identify rides that originated or ended at the worker's place of employment. We confirm only about 20% of subsidized rides as commutes to or from work. See Appendix Section B for details. Even if we are underestimating the use of subsidized rides for commuting by a factor of 3, among those who have paid employment reported in UI records, only one half of the ride-hailing credits are used for commuting.

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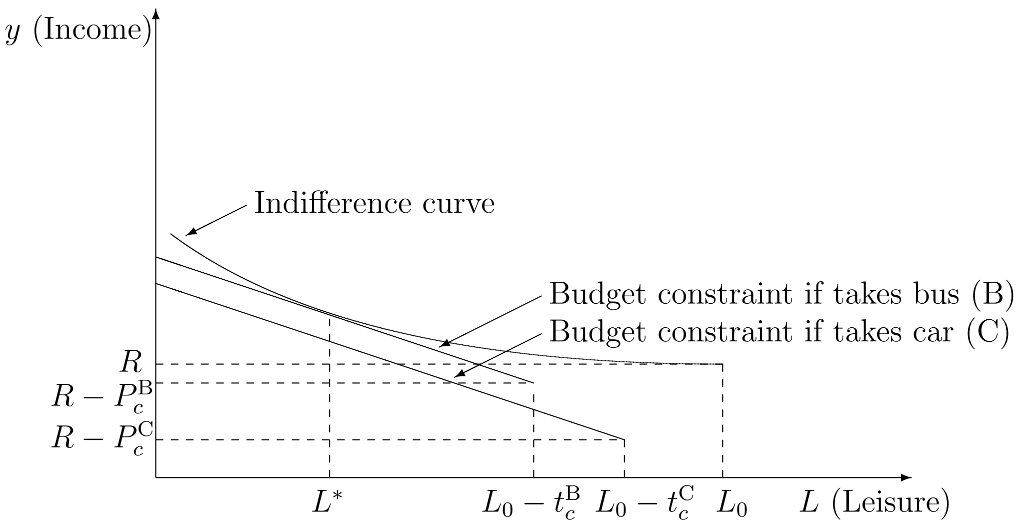
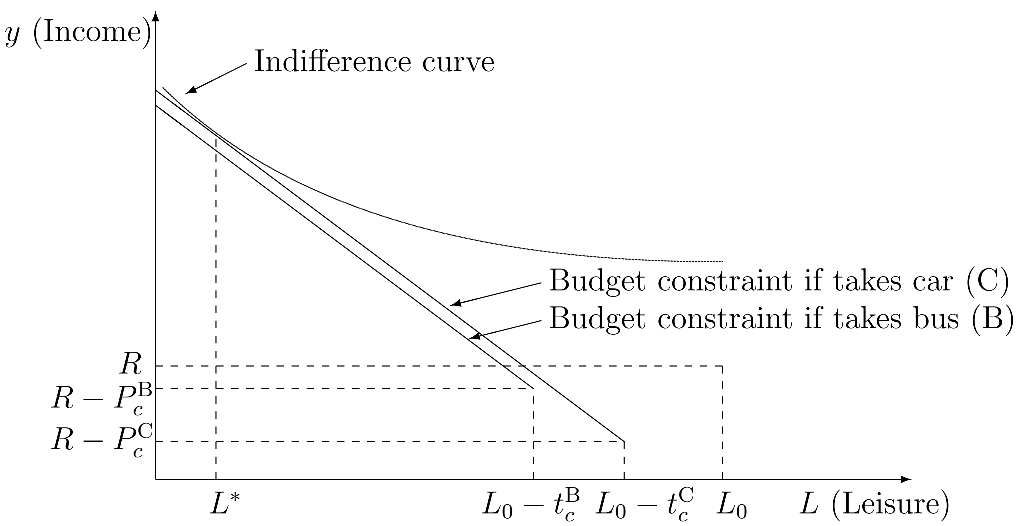
Figures

Figure 1: Commute Costs, Commute Mode, and Labor Force Participation

(a) Individual with Low Human Capital h



(b) Individual with High Human Capital h



Notes: Individuals depicted face the same full monetary cost P_c^{M} and time cost t_c^{M} for commute mode M, and have the same preferences. The lower-human capital individual, panel (a), commutes by bus, and is indifferent between employment and non-employment. The higher-human capital individual, panel (b), commutes by car and strictly prefers employment.

Tables

Table 1: Sample Characteristics

	Means		
	Treatment	Control	Difference (SE)
A. Demographics and public benefit receipt ($n_T = 406$, $n_C = 496$)			
Age (years)	30.10	29.16	0.935 (0.527)
Black	0.704	0.742	-0.038 (0.030)
Highest education			
Less than high school	0.033	0.086	-0.053 (0.023)
High school	0.412	0.444	-0.033 (0.042)
More than high school	0.250	0.250	<0.001 (0.037)
Public benefits received in quarter before enrollment			
Medicaid	0.916	0.917	-0.001 (0.019)
Supplemental Nutrition Assistance Program (SNAP)	0.808	0.793	0.014 (0.027)
Temporary Assistance for Needy Families (TANF)	0.152	0.118	0.034 (0.023)
Supplemental Security Income (SSI)	0.051	0.058	-0.007 (0.015)
B. Employment in quarter prior to enrollment ($n_T = 406$, $n_C = 496$)			
Any paid employment	0.552	0.565	-0.013 (0.033)
Earnings among those employed ($)	4,586	4,536	49.06 (413)
Received UI benefits	0.067	0.048	0.018 (0.016)
C. Commuting, reported in baseline survey ($n_T = 154$, $n_C = 168$)			
Total daily commute time last week, for those who commute			
Less than 30 min	0.307	0.247	0.060 (0.052)
30 to 60 min	0.386	0.457	-0.071 (0.057)
1 to 2 hours	0.243	0.179	0.064 (0.047)
More than 2 hours	0.064	0.117	-0.053 (0.033)
Mode of commuting in past 4 weeks, for those who commute			
Drive self	0.032	0.030	0.003 (0.019)
Public transit	0.675	0.661	0.015 (0.053)
Carpool	0.052	0.065	-0.014 (0.026)
Bicycle	0.006	0.006	0.001 (0.009)
Walk	0.234	0.190	0.043 (0.046)
Ride-hailing	0.643	0.530	0.113 (0.055)

Notes: Demographic characteristics in Panel A are from Allegheny County Department of Human Services (ACDHS) administrative records and from our baseline survey responses. Information on public benefit receipt are from ACDHS records. Characteristics in Panel B are from Pennsylvania UI records. Characteristics in Panel C are from the baseline survey. Sample sizes vary due to differing baseline survey item response rates and differing rates of administrative data availability. We exclude the 102 study participants who could not be matched to UI administrative records. Robust standard errors are in parentheses.

Table 2: Use of Subsidized Ride-Hailing during the Study Period

	Means		
($n_T = 406$, $n_C = 496$)	Treatment	Control	Difference (SE)
Took at least one ride-hailing trip	0.682	0.591	0.092 (0.032)
Number of trips (N)	35.84	6.04	29.79 (1.65)
Dollar value of trips ($)	627.5	101.8	525.8 (27.42)
Used $150 or more in trips	0.633	0.419	0.214 (0.033)
Used $300 or more in trips	0.591	0.026	0.565 (0.025)
Used $450 or more in trips	0.571	0.006	0.565 (0.025)
Used $600 or more in trips	0.542	0.002	0.540 (0.025)
Used $750 or more in trips	0.510	0.002	0.508 (0.025)
Used $900 or more in trips	0.441	0.002	0.439 (0.025)

Notes: Data are from the ride-hailing platform administrative records. Robust standard errors are in parentheses.

Table 3: Estimated Treatment Effects

	Control mean	Estimates (1)	(2)
A. Estimated treatment effects on "any paid employment" and "earnings," full sample ($n_T = 406$, $n_C = 496$)			
Dependent variable: any paid employment	0.597	0.058 (0.032)	0.054 (0.027)
Dependent variable: earnings ($)	3,353	220.9 (321.3)	106.5 (211.0)
B. Estimated treatment effects on "any paid employment," by employment status 4 quarters before enrollment			
Employed in all 4 quarters ($n_T = 127$, $n_C = 160$)	0.919	0.025 (0.032)	–
Employed in 1 to 3 quarters ($n_T = 154$, $n_C = 200$)	0.580	0.110 (0.050)	–
Employed in 0 quarters ($n_T = 110$, $n_C = 122$)	0.205	0.028 (0.058)	–
Includes quarter-of-enrollment indicators?		yes	yes
Includes pre-enrollment outcome (quarter before enrollment)?			yes

Notes: Data are from Pennsylvania unemployment insurance (UI) administrative records. Estimates are from a regression of the outcome on an indicator for treatment status. Column (1) controls for the calendar quarter in which the person enrolled in the study. Column (2) additionally controls for the outcome measured in the quarter prior to the quarter of enrollment. Robust standard errors are in parentheses.

Table 4: Estimated Effects of Ride-Hailing Use on Employment

		OLS Estimates		LATE Estimates	
	Control mean	(1)	(2)	(3)	(4)
A. Estimated effects, full sample ($n_T = 406$, $n_C = 496$)					
Dependent variable: any paid employment	0.597	0.078 (0.036)	0.071 (0.029)	0.105 (0.058)	0.098 (0.049)
First-stage F stat		-	-	538.3	537.7
B. Estimated effects on "any paid employment," by employment status 4 quarters before enrollment					
Employed in all 4 quarters ($n_T = 127$, $n_C = 160$)	0.919	0.003 (0.037)	–	0.047 (0.061)	–
First-stage F stat		–	–	160.8	-
Employed in 1 to 3 quarters ($n_T = 154$, $n_C = 200$)	0.580	0.155 (0.056)	– –	0.193 (0.089)	– –
First-stage F stat		–	–	229.5	–
Employed 0 quarters ($n_T = 110$, $n_C = 122$)	0.205	0.032 (0.063)	–	0.051 (0.104)	–
First-stage F stat		–	–	125.1	–
Includes quarter-of-enrollment indicators?		yes	yes	yes	yes
Includes pre-enrollment outcome (quarter before enrollment)?			yes		yes

Notes: "Ride-hailing use" is defined as the use of \$300 or more in ride-hailing credits. In the first stage of LATE estimation, the instrument is treatment assignment. Employment status data are from Pennsylvania UI records, and ride-hailing usage is from records of the ride-hailing provider. The OLS estimates in columns (1) and (2) are from a regression of the outcome on an indicator for taking at least \$300 worth of rides. The LATE estimates in columns (3) and (4) are from a two-stage least squares regression that uses the person's randomly-assigned treatment status as an instrument for taking at least \$300 worth of rides. Robust standard errors are in parentheses.

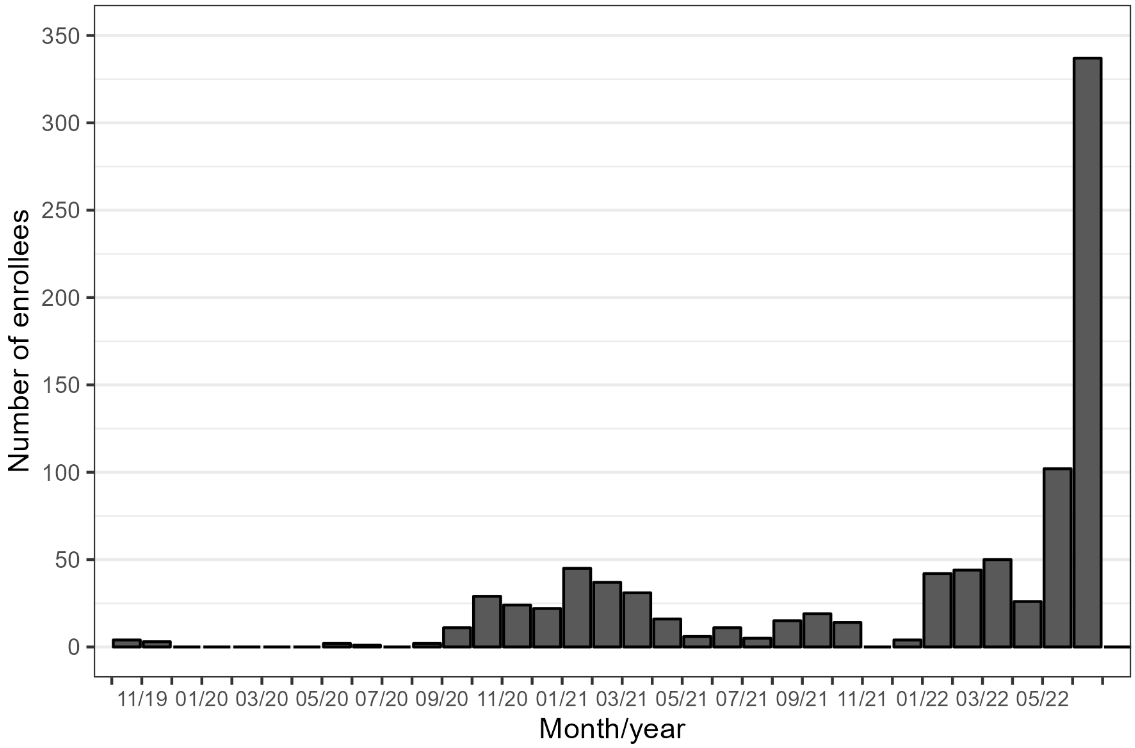
The Impact of Ride-hailing on Employment: Results from an RCT

Online Appendix

Sagar Baviskar, Lee Branstetter, Seth Chizeck, Beibei Li, and Lowell Taylor

A Additional figures and tables

Figure A1: Number of study enrollees over time



Notes: Figure presents the distribution of study enrollees by their month of random assignment.

Table A1: Local average treatment effects on outcomes in first full calendar quarter after enrollment; Defining ride-hailing usage in various ways and using robust vs. non-robust standard errors

	Control mean	(1) Effect	(1) First stage F-stat	(2) Effect	(2) First stage F-stat	(3) Effect	(3) First stage F-stat
A. Had any paid employment							
Took 6+ rides in first full calendar quarter after enrollment	0.597	0.079 (0.076) [0.076]	212.0	0.137 (0.077) [0.077]	202.3	0.128 (0.064) [0.064]	202.1
Took 12 or more rides during study	0.597	0.089 (0.086) [0.086]	156.5	0.155 (0.088) [0.087]	146.3	0.145 (0.073) [0.072]	146.1
Took $150 of rides in first full calendar quarter after enrollment	0.597	0.078 (0.075) [0.075]	226.5	0.135 (0.076) [0.075]	215.1	0.126 (0.063) [0.063]	214.9
Took $300 of rides during study	0.597	0.060 (0.058) [0.058]	578.8	0.105 (0.059) [0.058]	538.3	0.098 (0.049) [0.049]	537.7
Took $450 of rides during study	0.597	0.060 (0.058) [0.058]	627.2	0.104 (0.058) [0.058]	588.0	0.097 (0.049) [0.048]	587.3
Took $600 of rides during study	0.597	0.063 (0.060) [0.060]	575.3	0.108 (0.061) [0.060]	546.2	0.101 (0.051) [0.050]	545.6
B. Total earnings ($)							
Took 6+ rides in first full calendar quarter after enrollment	3,353	-35.70 (736.9) [743.7]	212.0	523.3 (744.7) [761.6]	202.3	252.7 (495.9) [500.8]	201.8
Took 12 or more rides during study	3,353	-39.97 (824.9) [832.6]	156.5	592.1 (843.9) [862.2]	146.3	285.6 (560.7) [566.0]	146.0
Took $150 of rides in first full calendar quarter after enrollment	3,353	-34.95 (721.3) [728.0]	226.5	514.9 (732.4) [749.1]	215.1	248.6 (487.7) [492.6]	214.6
Took $300 of rides during study	3,353	-26.91 (555.4) [560.5]	578.8	400.2 (569.7) [582.0]	538.3	193.1 (379.1) [382.6]	537.4
Took $450 of rides during study	3,353	-26.89 (554.9) [560.1]	627.2	397.5 (565.9) [578.2]	588.0	191.8 (376.3) [379.9]	586.9
Took $600 of rides during study	3,353	-28.16 (581.1) [586.5]	575.3	412.9 (588.1) [600.7]	546.2	199.1 (390.8) [394.5]	545.4
Includes quarter-of-enrollment indicators				X		X	
Includes outcome in quarter before enrollment						X	

Notes: Table presents estimates of the local average treatment effect (LATE) on employment outcomes. We define treatment “compliers” in various ways to check the robustness of the results in Table **??**. The employment outcomes are measured in the first full calendar quarter after the person enrolled in the study. Data comes from Pennsylvania unemployment insurance (UI) records and administrative records from the ride-hailing platform. The ‘Earnings ($)’ outcome measures participants’ total earnings in the quarter, including individuals with zero earnings in the quarter. Estimates are from a two-stage least squares regression that uses assigned treatment status as an instrument for treatment compliance. Column (1) does not adjust for any covariates. Column (2) adjusts for the calendar quarter in which the person enrolled in the study. Column (3) additionally adjusts for the outcome measured in the quarter prior to the quarter of enrollment. Regular standard errors are in parentheses, and robust standard errors in are in brackets.

Table A2: Intent-to-treat effects on self-reported outcomes from follow-up surveys

	Control		Treatment		
Outcome	N	Mean	N	Mean	Treatment effect
A. Employment outcomes in past four weeks					
Worked for pay	273	0.561	247	0.603	0.042 (0.042)
Weeks worked (max 4) (N)	187	3.33	172	3.36	0.027 (0.090)
Hours typically worked per week (N)	188	30.78	172	31.96	1.17 (1.25)
Hourly wage ($)	189	14.36	173	14.97	0.614 (0.555)
B. Transportation and travel outcomes					
Round-trip commute time on days worked last week					
Less than 30 minutes	190	0.260	173	0.340	0.080* (0.044)
30 to 60 minutes	190	0.428	173	0.414	-0.014 (0.045)
1 to 2 hours	190	0.215	173	0.182	-0.033 (0.035)
More than 2 hours	190	0.097	173	0.064	-0.033 (0.023)
Commute mode used in past 4 weeks					
Public transportation	190	0.598	173	0.576	-0.022 (0.049)
Drive self	190	0.092	173	0.053	-0.039 (0.025)
Carpool	190	0.031	173	0.037	0.006 (0.015)
Ride-hailing	190	0.619	173	0.828	0.208*** (0.037)
Walk	190	0.184	173	0.205	0.021 (0.038)
Bike	190	0.012	173	0.006	-0.006 (0.006)

Notes: Table presents estimates of the effect of being assigned to the treatment group on various self-reported survey outcomes. Data comes from follow-up surveys that took place between 1 and 12 months after the participant enrolled in the study. Participants were able to complete up to 8 surveys during this time period. Estimates are from a panel regression of the outcome on an indicator for treatment status, with no covariate adjustment. Robust standard errors are in parentheses and are clustered at the participant level. ***p <0.01, **p <0.05, *p <0.1

B Methods of identifying work-related ridehailing trips

We estimate that approximately 16.5% of the subsidized ridehailing trips in the first quarter after random assignment (i.e. the quarter in which we observe our main treatment effects) were used for commuting to work. To derive this estimate, we compared the pickup and drop-off locations from the ridehailing trips with the participant's location of employment according to UI records.

B.1 Identifying ridehailing pickup and drop-off locations

The study participants took a total of 9,114 subsidized ridehailing trips in Q1 after random assignment. Our data from the ridehailing platform reports the pickup and drop-off addresses for each of these trips. We were able to geocode the latitude and longitude of the pickup and drop-off addresses for 6,399 (70.2%) of these trips. We then exclude the 2,715 trips for which we failed to geocode the coordinates of both the pickup and drop-off locations. The geocoding failures were generally due to the pickup or drop-off location having an incomplete street address.

B.2 Identifying UI employer locations

Pennsylvania UI wage data reports the address of an employer if the employer is based at a single physical establishment in the state. Among the 457 unique employers that participants worked for in Q1, 218 of them had an address recorded in the UI data. We geocoded the latitude and longitude of these addresses. We then used Google Maps API to identify all of the establishment locations of these 218 employers in Allegheny County. We did this to account for the possibility that the address in the UI data does not represent the physical location where the employee goes to work.

We further used Google Maps API to identify all of the Allegheny County establishment locations of the 457 - 218 = 239 employers that did not have an address recorded in the UI data. We were able to identify at least one establishment location for 56 of these companies. The most common reason for failing to identify the company's establishment locations in Google Maps was that the company's legal name in the UI data does not correspond to the trade name of a business on Google Maps.

B.3 Estimating work-related trips

Among the 6,399 geocoded ridehailing trips in Q1, 3,641 of them (56.9%) took place in a calendar quarter during which the participant either 1) Was not employed 2) Had missing UI employment information or 3) Worked for at least one employer but none of their employers had a geocoded address. We exclude these 3,641 trips from our commuting analysis because the trip could not possibly be work-related (for case 1), or we do not know whether or not the person was employed at the time they took the trip (for case 2), or we do not know the physical location where the person was working when they took the trip (for case 3).

The remaining 6,399 - 3,641 = 2,758 trips in Q1 took place in a calendar quarter during which the participant worked for at least one employer with a geocoded address. We consider these trips to be commuting-related if either the pickup location or drop-off location is within 0.25 miles of the employer location. 532 of the trips meet this criteria.

Altogether, we estimate that 532 / 2,758 = 19.3% of the Q1 ridehailing trips were used to commute to work during periods of employment. This percentage is 104 / 375 = 27.7% among the control group and 428 / 2383 = 18.0% among the treatment group. These estimates are likely a lower bound because the UI data only covers formal-sector employment.