Getting to Work: Experimental Evidence on Job Search and Transportation Costs

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Abstract

Do transportation costs constrain job search in urban low wage labor markets? I test this question by providing transit subsidies to randomly selected clients of a non-profit employment agency. The subsidies generate a large, short-run increase in search intensity for a transit subsidy group relative to a control group receiving standard job search services but no transit subsidy. In the first two weeks, individuals assigned to the transit subsidy group apply and interview for 19 percent more jobs than those not receiving subsidies. The subsidies generate the greatest increase in search intensity for individuals living far from employment opportunities. Some suggestive evidence indicates that greater search intensity translates into shorter unemployment durations. These results provide experimental evidence in support of the theory that search costs over space can depress job search intensity, contributing to persistent urban poverty in neighborhoods far from job opportunities.

Keywords: search costs; spatial mismatch; public transit; urban poverty

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1. Introduction

In the urban United States, de-facto residential segregation results in many minority, poor individuals living in areas with few available jobs. Kain (1968) and Wilson (1997) have argued that a lack of geographic access to jobs contributes to adverse labor market outcomes for these individuals. In this paper, I use an experiment of randomly provided public transit subsidies to test one mechanism that could drive such spatial mismatch effects. I randomly assign clients of a non-profit employment agency to a control group receiving only standard job search assistance or a treatment group also receiving subsidized public transit. Individuals in the sample are disproportionately African-Americans from economically disadvantaged neighborhoods. I find strong evidence that those assigned to receive subsidized transit experience a large, temporary increase in search intensity relative to the control group. During the first two weeks after receiving subsidies, individuals assigned to the treatment group search more intensely than the control group, completing 19 percent more job applications and interviews. After two weeks, the uptick in applications and interviews disappears. This is not surprising as it coincides with the treatment group's quick exhaustion of the subsidy.

Transit subsidies have the largest effect on search behavior for those living far from job opportunities. I measure job access as the average travel distance to a set of job vacancies at baseline. Transit subsidies have nearly double the effect on search intensity for applicants 9 miles from job vacancies (90^{th} percentile) versus those 6 miles away (median). While search costs over space matter for the whole sample, they matter most for those living in neighborhoods with limited access to employment. I also find suggestive evidence of decreased unemployment durations, though this estimate is statistically imprecise. Unfortunately, effects on wages and job locations cannot be distinguished from random noise.

The results match the predictions of standard job search theory. I build a simple job search model and explicitly demonstrate what accepted theory would predict. Providing a durable but depletable search-enhancing good (e.g. bus passes) should create an immediate, temporary spike in search intensity, followed after some lag by an increase in the hazard from unemployment. These predictions follow standard theory in which any decrease in the cost of searching, including subsidizing public transit, causes job seekers to search more intensively (Pissarides, 2000). Workers may translate greater search intensity into shorter average unemployment durations, an increased reservation wage, or some combination of the two.[2] In this theoretical framework, transportation subsidies relax a constraint on job search, leading to greater search intensity and improved labor market outcomes, especially for those far from jobs. While the evidence on labor market outcomes is limited, the fact that search intensity increases especially for those far from employment centers confirms a central mechanism of the spatial mismatch hypothesis, that transportation costs hinder job search for the urban poor.

A well-established empirical literature uses randomized experiments to measure how active labor market policies affect job search and labor market outcomes. Woodbury and Spiegelman (1987) find that providing job-finding bonuses to those receiving unemployment insurance (UI) leads to faster exits from UI. Meyer (1995) reports on a series of follow-up experiments in which a combination of job search assistance and enforcement cut the length of UI receipt. Such results have been replicated in other contexts (Dolton and O'Neill, 1996; Dolton and O'Neill, 2002; Black, et. al. (2003); Card, Kluve, and Weber, 2010). While many experiments have tested how variants of information, coaching, or enforcement may affect job search outcomes, to my knowledge no study has examined the importance of search costs over

[2] If the job seeker becomes much more selective, unemployment durations may actually rise, though this is not typical (van den Berg, 1994).

space using a field experiment. Thus, in its most narrow interpretation, the present study provides new evidence regarding the effect of adding public transit subsidies to a standard job search assistance package for the urban poor.

More broadly, the results indicate that search costs over space matter most for low-wage job seekers living in neighborhoods with the least access to employment. The spatial mismatch hypothesis seeks to explain the existence of geographically concentrated poverty by focusing on spatial access of urban workers to available jobs (Kain, 1968; Wilson, 1997). This theory can be generated by many mechanisms (Gobillon, Selod, and Zenou, 2007), but search costs over space in general (Colson, Laing, and Wang, 2001; Wasmer and Zenou, 2002; Wasmer and Zenou, 2006) and transportation costs in particular (Gautier and Zenou, 2010) can lead to disparities in search behavior, labor market outcomes, and the spatial concentration of poverty. These theories rely on the assumption that search becomes less effective over space due to worse information or higher transportation costs. As a result, search costs dampen search intensity most for individuals living in distant, spatially-mismatched neighborhoods, and this leads to poor labor market outcomes. The results show that transportation costs affect search intensity particularly for those living far from jobs. This result confirms a vital assumption of the theory. Of course, the present results apply to the population of low-wage, urban, minority workers included in this experiment, but this is the group proves relevant for the spatial mismatch literature.

A large empirical literature has used observational data and natural experiments to test the theory of spatial mismatch, with most of the literature confirming that better transportation improves employment outcomes (Raphael and Stoll, 2000; Holzer, Quigley, and Raphael, 2003; Holzer, Ihlanfeldt, and Sjoquist, 1994; Raphael and Rice, 2002) and suburbanization of jobs harms employment outcomes of urban minorities (Zax and Kain, 1996). Recent empirical work on urban job search also supports this idea. Job applicants in the UK (Manning and Petrongolo, 2013) and in the US (Marinescu and Rathelot, 2013) apply to nearby jobs much more intensely than distant jobs, even within the same metropolitan area. Gobillon, Rupert, and Wasmer (2014) demonstrate in a search and matching model that commuting costs can explain a significant part of racial unemployment disparities in both France and the US. However, no experiments have successfully confirmed that alleviating transportation costs can improve labor market outcomes. Whether by directly lowering transportation costs through a shared van ride system (Roder and Scrivner, 2005) or by providing housing vouchers in lower poverty neighborhoods (Kling, et. al. 2007; Ludwig, et. al. 2012), previous attempts to reduce spatial mismatch have generally shown little improvement in labor market outcomes. Thus, the present study provides the first experimental evidence on one potential mechanism of spatial mismatch: search costs over space. I find that transportation costs can reduce job search intensity of the urban poor, particularly those living far from existing job vacancies. Thus, this study provides the first experimental evidence that transportation costs constrain job search of the urban poor in a manner that could contribute to observed patterns of concentrated poverty.

The next section provides context on the spatial distribution of employment in Washington, DC; section three presents a simplified job search model to help define outcomes of interest and provide benchmarks against which to compare measured results; the fourth and fifth sections describe the operation of the experiment and the data; the sixth section presents results of the experiment; and a final section concludes.

2. Context

Washington, DC provides a prime example of how de-facto residential segregation can lead to spatial mismatch of workers from available jobs. Low-wage jobs in the DC metro area tend to be located downtown and in the western suburbs of the city while low-wage workers tend to live far from these jobs in the Southeast part of the city and its eastern suburbs. On net, individuals from far Southeast DC must look elsewhere to find employment. Figure 1 demonstrates this fact by mapping US Census Longitudinal Employer-Household Dynamics data on the ratio of low-wage jobs to low-wage residents across DC zip codes. Low-wage jobs are abundant relative to the supply of workers downtown, in the more affluent western part of the city, and in the northern and western suburbs. However, jobs are scarce in Southeast DC. In the three zip codes east of the Anacostia River (furthest southeast but still within the red border of the District), there are 4 low-wage workers for each low-wage job.

Baseline data for individuals participating in the experiment (described below) match these qualities of the census data. As shown in Figure 2, at the time of recruitment 83 percent of the sample lived inside the boundaries of the District, almost entirely in the eastern half of the city. The remainder largely reside in the Maryland suburbs east of the city. Jubilee Jobs, an organization with which I partner in the experiment, collects information on available job vacancies that are appropriate for their clients. Figure 3 maps administrative data on these vacancies from April 2010 to April 2011. Jubilee Jobs targets vacancies somewhat closer to the homes of their applicants than the average job, but even vacancies tailored to their applicants tend to be in the western half of the city. The individuals participating in this experiment face a situation in which job vacancies exist but are often far from their residences. If search costs help sustain pockets of concentrated poverty, reducing the effective distance between potential workers and vacancies should affect their job search behavior and labor market outcomes.

3. Stylized Model

In this paper, I present empirical results from a field experiment. However, a simple, stylized model provides a theoretical structure for thinking about the nature of the experimental treatment, defines outcomes to be measured, and predicts changes in those outcomes that can be compared to the observed results. Thus, I make use of a simplified and slightly modified version of the standard job search model (e.g. Pissarides, 2000)

Consider an individual with utility function $u(\cdot)$ and discount rate $\delta \in (0,1)$ living in an infinite horizon, discrete time world. While unemployed she receives income y_L. Next period, she has $\lambda(s_t)$ probability of being employed and otherwise remains unemployed. If employed, she receives income $y_H > y_L$ forever.[3] She has access to k_t units of a durable, depletable, job search-enhancing good (e.g. bus passes) that can be used to increase search intensity, s_t, and thus the probability of finding a job, $\lambda(s_t)$, which is increasing, concave, and differentiable. For simplicity, she cannot replenish the initial stock of k_t. This durable good evolves according to:

$$k_{t+1} = k_t - s_t \quad (1)$$

Given this environment, the job seeker's choice at any point in time can be characterized by two value functions, one for being employed (E) and one for being unemployed (U):

$$V(E, k_t) = V(E) = \frac{u(y_H)}{1-\delta} \quad (2)$$

$$V(U, k_t) = \max_{k_{t+1}} \left\{ u(y_L) + \delta\left[\lambda(k_t - k_{t+1})V(E) + \left(1 - \lambda(k_t - k_{t+1})\right)V(U, k_{t+1})\right]\right\} \quad (3)$$

Provided standard conditions at the limits and non-negativity constraints do not bind, the optimal choice of search intensity can be characterized by the following:

[3] I abstract from the idea of a reservation wage for simplicity and because statistical noise prevents precisely measuring the effect of treatment on wages in the experiment. Standard random search models predict a higher reservation wage and thus higher observed wages when search is subsidized. Any such increases can offset reductions in unemployment (van den Berg, 1994). Applicants may also require higher wages if the new search leads to job offers from more distant jobs with higher commuting costs. This would also reduce effects on unemployment.

$$\frac{\lambda'(s_t)}{\lambda'(s_{t+1})} = \frac{u(y_L) + \delta V(E) - V(U, k_{t+1})}{V(E) - V(U, k_{t+1})} \quad (4)$$

It is straightforward to show that the definition of $V(E)$, the concavity of $\lambda(\cdot)$ and equation (4) together imply that search intensity is greatest at time zero, decreasing monotonically afterward.[4]

The public transit subsidy described in the present study can be thought of as providing a certain number of bus or train rides, which corresponds to the initial stock k_0 of the durable search input. A comparative static illustrates the predictions of this simple model for the present context. Suppose that the job seeker goes from having no initial stock of the search input (no free bus passes) to having some positive amount k_0. Initially, search intensity would be at a constant level (normalized to zero) leading to a constant job finding rate $\lambda(0)$. If the person were to receive a treatment providing transit subsidies, search intensity would increase. The greatest increase would occur immediately, and the effect would monotonically decrease over time until hardly distinguishable from zero. The observed job finding rate would follow a similar path, but with a lag. It would be unaffected at time 0, spike at time 1, and then subsequently decrease. Thus, subsidizing public transit costs should cause increased search intensity, particularly at first, followed by an improved job finding rate after some lag.

This simple model does not include an explicit spatial dimension. However, an increase in search intensity s_t must come from either more intense search nearby or search at new, more distant locations. To the extent that the latter occurs, standard models would predict a larger search area and longer commute distances, on average. Such spatial elements have been formally incorporated in equilibrium search and matching models by assuming discrete, separate neighborhoods with higher search costs in the other neighborhood (Colson, Laing, and Wang, 2001) or costs of search intensity that increase continuously with distance (Wasmer and Zenou,

[4] See Appendix A.3. for details .

2002; Wasmer and Zenou, 2006). Increased cost of searching over space discourages search, particularly far from home. Alleviating such costs will increase search intensity at existing search locations and expand the maximum distance of search, leading to lower unemployment rates and/or higher wages. Thus, the simplified model here naturally fits within search models that explicitly include spatial frictions.

4. Experimental Design

4.1. Treatment

In this experiment, treated subjects received a fee-reducing fare card and an in-kind subsidy to ride Washington Metropolitan Area Transit Authority (WMATA) buses and trains, which cover the entire DC metropolitan area. In particular, they received a reusable SmarTrip card with a free $25 balance. When this balance had been depleted, clients who were still looking for work with the partner agency could return the empty card for a second card with a fresh $25 balance. Members of the treatment group were made aware of the availability of receiving two cards from the beginning. A control group received no transit subsidies, only receiving standard job search assistance. Cards were tracked using unique serial numbers, and in all but one case the returned card matched the correct serial number. Given that the card itself costs $5, the total cost of the package is $60. The balance functions similarly to a gift card; the balance is debited with each trip and ceases to work after the balance is negative. The $50 subsidy represents about 33 bus trips ($1.50 each) or 10 to 31 train trips ($1.60 to $5.00 each), depending on distance.

In addition to the $50 card balance, the SmarTrip cards also provide price subsidies for subsequent trips relative to using cash because WMATA charges different base prices to users of SmarTrip cards and those who pay with cash. Bus trips receive a 20 cent discount and train trips receive a 25 cent discount relative to riders using cash. Finally, the SmarTrip card also allows passengers to transfer between buses for free or between bus and rail at a 50 cent discount so long as the rides are within two hours of each other. This service is not available to passengers using cash and can represent a considerable subsidy to job-seekers who can make multiple successive bus trips in one day on a single fare.

Altogether, treatment provides a significant package of transportation subsidies, and I will not attempt to disentangle the effects of different parts of the package. What matters is that this package should significantly reduce the cost of search for the sample of individuals participating in this study. Using labor market histories of individuals in the sample, I estimate that in a typical year an average member of the sample would have a monthly income of about \$552.[5] Even this average is likely a significant overestimate of the sample's current available funds given that nearly the entire sample is unemployed at baseline. Though in the ideal, we would compare the size of the subsidy to estimated total commuting costs, this data is not available. It seems reasonable, though, to expect that a transit subsidy equal to at least 10% of monthly income is unlikely inframarginal and thus truly reduces the cost of search.

Finally, control subjects did not receive transportation assistance but continued to receive standard job search assistance from the partnering organization. Thus, I will measure the impact of transportation contingent on also receiving job search assistance.

4.2 Study Design

I cooperated with a local, non-profit, private job placement assistance organization, Jubilee Jobs, to implement this experiment. Jubilee Jobs has two sites located in Washington, DC with one in Anacostia, a predominantly low-income and African-American area in southeast Washington, and one in Adams Morgan, a racially and economically diverse area in north-

[5] See summary statistics below. An average individual is employed for 30 hours per week at \$10 per hour and is only employed 46 percent of the time.

central DC. They provide job skills training and job placement assistance free of charge to all interested individuals, but Jubilee focuses on low-wage job placements. As a result they typically assist low-income individuals who are often receiving public assistance, re-entering the workforce after incarceration, or recovering from substance abuse.

Applicants come through Jubilee Jobs in orientation cycles. Anyone is eligible to use their services free of charge, but they only assist in finding entry-level low-wage jobs. In practice, most people in our sample report arriving at Jubilee Jobs after being recommended by someone in their social network ("hearing through the grapevine"). A particular cohort begins with an orientation session, followed by a week of job skills workshops. Then, applicants search for employment with assistance from a job counselor who actively markets them to prospective employers and meets with them regularly to set-up interviews.[6] Every two weeks, a new cohort begins this process. Due to the timing of services provided by Jubilee Jobs, the experiment also runs in a sequential manner. Potential subjects were not informed about the experiment until after they had completed Jubilee's initial week of workshops; this was meant to screen out anyone who might sign up for job search services solely to obtain transit subsidies (though we cannot rule that some may have learned of the program informally). At the end of the final workshop, potential subjects were introduced to the study as a group and invited to participate in

[6] Due to the logistics of distributing the transit cards, job counselors were aware of participants' treatment status. In any case, close collaboration between the counselors and job-seekers suggests that an attempt to blind staff to treatment status would have failed. Note that job counselors being aware of treatment does not inherently represent a problem as applicants may optimally wish to adjust the actions of one job search input (e.g. job counselor assistance) in response to subsidy of another input (e.g. transit subsidies). However, job counselor behavioral change could, in principle, lead to overestimates of the effect of treatment if staff members shift effort toward those receiving treatment at the expense of others. However, the data indicate that spillovers are if anything *positive.* If anything, members of the sample search more intensely and get jobs more quickly when those sharing the same job counselor in the same cohort are treated. Thus, the stated results are if anything a lower bound. Results available upon request.

the study. Those interested in participating reviewed and signed an informed consent form. Both parts of this process described the transit subsidies and the process of allocating subsidies to some individuals by lottery. Of course, awareness of treatment can lead to placebo effects; however, as discussed below this explanation of the observed effects seems unlikely given the evidence that treatment matters most for those living far from employment opportunities or the fact that spillovers appear to be, if anything, positive. Finally, the recruitment process was repeated roughly every two weeks from November 2010 through June 2011.

4.3 Recruitment and Sample Demographics

Table 1 summarizes participation in the experiment. Of the potential participants, 60 percent consented to participate in the study. Table 2 reports baseline characteristics of individuals participating in the experiment and compares them to average characteristics of those who did not participate. The first column reports mean baseline characteristics for those who were recruited. Minority job-seekers compose almost the entire sample, and they face severe disadvantages in the labor market including low educational attainment, long on-going unemployment durations, and low earnings in the past. The sample faces major transportation issues: only nine percent of the sample have access to a car, yet they live on average 6.7 miles from the vacancies displayed in Figure 3. The second column of Table 2 shows characteristics of those who were invited to participate but did not.[7] The third column compares the first two columns, testing for selection into the experiment. Many dimensions (age, gender, educational attainment, ex-offender status, immigrant status, ability to drive, or residential location) display no evidence of selection. Applicants do appear to select into the experiment based on

[7] See Appendix for details on calculating characteristics of non-participants using aggregated data. Unlike other baseline characteristics, the labor market history variables and distance variables require reviewing paper files that cannot be accessed and summarized without informed consent.

transportation need, race, and existing receipt of public assistance. It is worth noting, though, that access to vehicles is quite low in both groups, and regardless of this selection the experiment would be measuring the effect of transit subsidies for a population reliant on public transit. For more detail on this comparison, see Appendix A.2. In a comparison with Current Population Survey data, I find that the experimental sample has a similar gender breakdown but is somewhat older and somewhat less educated than the typical black, unemployed, urban respondent to the CPS (see Appendix A.2). Altogether, the experimental sample can be most accurately described as disadvantaged, unemployed, urban, minority, mid-career individuals who rely on public transit. Though this is clearly not a representative sample of the US population, these individuals are the group of interest when considering the effects of spatial mismatch. The experiment will be able to test whether transportation costs matters for disadvantaged, urban job seekers who rely on public transit.

4.4 Compliance with Treatment

Applicants who decided to participate are randomly assigned to treatment and control groups using a random number generator with treatment stratified by cohort, Jubilee Jobs site (two separate sites), and ex-offender status. In an initial phase from November to January, the treatment probability was one quarter. Starting in February, as more funding became available, half of all subjects were offered treatment. As Table 1 documents, this results in slightly less than half of the sample being treated with 208 of 468 individuals assigned to treatment.[8]

Treatment group individuals were provided with a transit card (WMATA SmarTrip). Each selected applicant was offered the card from Jubilee Jobs staff prior to being sent out on

[8] Since the probability of treatment varies over time, I will always control for a dummy for cohorts in the initial phase. If I did not, then a trending outcome would cause bias in measured treatment effects. In any case, this choice does not affect the main results.

their first interview and could obtain a second card if the first was exhausted prior to finding employment and they continued to work with Jubilee Jobs.[9] Table 1 describes compliance of individuals with treatment, i.e. take-up of the SmarTrip cards. Take-up is high with 89 percent of the treatment group picking up at least one SmarTrip card and half receiving two cards.[10] The control group, meanwhile, did not receive any transportation assistance from Jubilee Jobs, as indicated in the table. Altogether, this points to high but imperfect compliance with treatment. Less than full take-up of the second card mainly occurred if individuals found employment prior to obtaining a second card or dropped out of Jubilee's program. Imperfect take-up of the first card occurred if individuals found employment or dropped out of the program very quickly. To deal with imperfect compliance, all of the analysis will focus on Intent-to-Treat effects based on original treatment assignment.

5. Data and Follow-Up

5.1. Data on Outcomes

Data come from three sources: Jubilee Jobs administrative records, a phone survey, and the WMATA SmarTrip on-line card manager. Using these data sources I can measure job search activity (number, timing, and location of job applications and interviews) and labor market outcomes (time of finding employment, wage rates, and job locations) for up to 3 months after enrollment in the study. Attrition at three months is only 9 percent and is balanced across the treatment and control groups. I track public transit use electronically using the provided transit cards, though only for the treatment group. For further description of the data, see the Appendix.

5.2. Baseline Data and Randomization Test

[9] This exchange helped protect against re-sale of the cards and connected the cards more closely with job search. Serial numbers of returned cards matched the originally issued card in all but one instance.

[10] Three individuals were able to obtain 3 cards due to administrative errors.

Jubilee Jobs collects background and demographic information on all clients in intake interviews prior to randomization. This data provides baseline information on demographics, labor market history, ex-offender status, educational attainment, and more. Above, I make use of these data to characterize the baseline characteristics of the sample in Table 2. As an additional check to ensure that selection into attrition does not introduce bias into the estimates for employment characteristics, I also test baseline balance for only those individuals with follow-up employment data, either in the administrative records or the phone survey. Table 3 reports these results, with the first two columns reporting mean baseline characteristics for treatment and control and the third column reporting the coefficient on a treatment dummy in a regression of baseline characteristics on treatment and an indicator for the initial phase. The results indicate no statistical difference in baseline characteristics between the treatment and control groups. It appears that the randomization was valid and not violated by attrition from the employment data.

6. Results

6.1. Transit Card Usage

As noted above, the vast majority of individuals assigned to treatment receives at least one free transit card and most receive a second; however, many individuals do not receive the full transit subsidy because they find employment or drop out of Jubilee Jobs' program prior to exhausting the subsidy. I use electronic data on usage of the cards to more precisely measure uptake of treatment. For instance, individuals ride the bus rather than the train for 63 percent of all transactions. The transaction data shed some light on whether the subsidies are used for job search or other activities. Figure 4 shows the time of day when members of the treatment group use their SmarTrip cards. Usage clearly peaks during the middle of regular business hours for both rail and bus trips with with 39% of all rail trips and 44% of all bus trips taken between 9:30

a.m. and 3:00 p.m. (84% and 86%, respectively, before 7:00 p.m.). This contrasts sharply with the average WMATA rider, whose bi-modal distribution of ride times follows the morning and evening commute. Additionally, members of the treatment group are not significantly more likely than the average rider to take the bus or train late in the evening or, as shown in Figure 5, on the weekend. These facts together indicate that card usage occurs disproportionally during prime job search hours, rather than either commuting or entertainment hours. The timing of subsidized travel, along with the policy of tying the cards to job search of one individual through exchanging uniquely identifiable used cards for new ones, indicates that the transit subsidies are likely being used for job search. The lack of data for the control group prevents this conclusion from being definitive, but these findings indicate that observed card usage is consistent with transit subsidies being used for job search activities.

I can also calculate total spending on transit using the cards and deposits by individuals onto the cards. Figure 6 depicts three variables over time: total spending, total deposits, and spending net of deposits. Usage of the cards is extensive, with average spending surpassing $50 within two months of first using the card. Also of interest, many individuals receiving the cards do not simply use the cards for the $50 subsidy but continue depositing their own money onto them. Deposits are slow at first as the subsidy is used up, but within four months applicants have added an additional $50 to the balance of their card(s) on average. Spending net of these deposits rises quickly and flattens out within the first 50 days.[11] These results indicate a pair of important facts about treatment. First, applicants make use of the lump sum subsidy relatively quickly, with individuals exhausting the subsidized balance with the first 50 days. This suggests that we should expect any effect of treatment on search intensity to appear and disappear within this time frame.

[11] The average subsidized balance is less than $50 because of imperfect compliance with treatment. Also, deposits are quite skewed with the median deposit amount close to $10.

Second, applicants demonstrate a revealed preference for using the transit card even after the subsidized balance has been exhausted. This suggests that the value of treatment is not exclusively due to the balance but also because of the fare reductions that the card provides.

6.2. Search Intensity

Theory indicates that a decrease in transportation costs and the resulting increase in transit usage just documented should affect labor market outcomes via an increase in search intensity. I measure search outcomes using administrative records from Jubilee Jobs on jobs applied to and interviewed for by each individual. Given non-random selection of individuals out of job search (into employment) and likely poor recall by individuals of job application histories, I use only administrative data for these outcomes. I also combine applications and interviews (generically, "search actions") due to ambiguity in the criteria for dividing them in the data.

Figure 7 plots a local polynomial smoothing of the number of search actions undertaken by the treatment and control groups over time. The most notable feature of the data is that the subsidy group experiences a sharp increase in search intensity in the first two weeks after recruitment, relative to the control group. This difference diminishes over time with the two groups searching with similar intensities between days 15 and 60. Toward the very end of the sample period the two groups diverge again. However, the sample of individuals searching for employment becomes both smaller and increasingly selected over time. Thus, in the final month treatment effects cannot be distinguished from sample selection bias and statistical noise. Overall, the data indicate that receiving a transit card leads to a large but short-lived increase in search intensity. This pattern matches the type of treatment effects that we would expect given what we know about usage of the free transit cards. As shown in Figure 6, individuals quickly exhaust the transit subsidy, and this is matched by the declining difference in search intensity.

I quantify the effect of treatment on search intensity using a regression framework:

$$A_{itd} = \alpha + \beta T_i + \phi P_t + \delta Z_i + \eta_d + \epsilon_{itd} \qquad (5)$$

where A_{itd} is the number of search actions taken by person i in cohort t on the dth day since recruitment; T_i is an intent-to-treat dummy; P_t is a dummy for the initial phase;[12] Z_i is a vector of baseline characteristics; and η_d is a days-since-recruitment fixed effect. For this regression, observations are at the individual-day level, and observations for an individual-day are included only if the individual has not yet found employment at that time. Table 4 present the results of this analysis. In the first two columns, I limit the sample to the first fourteen days after recruitment. The first two weeks have the benefit of being relatively free of selection bias and focus attention on a time in which the subsidy would likely still be in effect. The first column includes only intent-to-treat and initial phase dummies, while the second also includes baseline demographic controls and days-since-recruitment fixed effects. Both specifications indicate that treatment causes an increase in search intensity during the first two weeks that is statistically significant at the 5% level. The coefficient of 0.031 indicates that applicants assigned to receive public transit subsidies undertake 0.031 more search actions per day, or 0.43 of a search action more over the two-week period. This represents a 19% increase in search intensity relative to the control group.

The next two columns report results over a larger sample including all search days, not just those in the first two weeks. Column (3) demonstrates that the average effect over all 90 days is statistically insignificant, though a positive effect in the first two weeks that fades thereafter would fit in the 95% confidence interval of the 90-day effect. I formalize this idea in

[12] P_t is included in all regressions because the probability of treatment increased at the end of the first stage of the study. Including P_t ensures that the measured treatment effects do not result from differences across cohorts in employment and job search.

column (4), which uses the full sample of search days but now includes an interaction between treatment and a dummy for the first two weeks. The coefficient of 0.038 on the interaction term (statistically significant at the 1-percent level) confirms that the gap in search intensity between treatment and control during the first 14 days is statistically different from the same gap for days 15 and later. The precisely estimated coefficient on the treatment dummy indicates very little effect on search intensity more than 14 days after treatment. Thus, decreased search intensity on later days does not appear to offset the increased search observed in the first 14 days. In the final column, I demonstrate that these results are not an artifact of measuring treatment effects for count data with OLS; a Poisson model generates similar results, indicating 24% greater search intensity in the first two weeks.

The increase in search intensity during the first 14 days is economically significant. On the one hand, I only measure an increase of about one half of one search action spread over two weeks. That is small relative to a subsidy that can support several transit trips. On the other hand, a half search action increase represents a large percentage increase of the control group mean of 2.3 search actions in the first two weeks. The very low level of measured search intensity in the control group likely indicates that applicants under-report search behavior to job counselors. As discussed in the Appendix, I can only record search behavior reported to Jubilee Jobs. This likely misses a significant amount of search behavior. For instance, 41% of those in the sample obtain employment as the result of a job application in which their job counselor was not involved (and thus would likely not be reported if a job had not resulted). This evidence suggests that applicants undertake a large amount of additional search (either unreported applications and interviews or un-measured search activities like information gathering) which will not be reported in my data. Thus, the effects I measure likely underestimate the effect of treatment on the *level* of search intensity because unmeasured search behavior also likely increases sharply in the first two weeks.

6.3. Heterogeneous Effects

The above results indicate that transit subsidies lead to greater search intensity. If these effects result from lower spatial search costs, we would expect that individuals with less geographic access to employment would benefit the most. I use geocoded home address and vacancy data from Jubilee Jobs (see figures 2 and 3) to measure the average great circle distance in miles from each applicant's home address to a random sample of 2500 vacancies. This provides a measure of how "spatially mismatched" each individual is from relevant employment opportunities. Table 5 tests whether the observed effect of transit subsidies on search intensity differs by average distance to job vacancies (a continuous measure of geographic job access). The first column of Table 5 replicates the average effect of being assigned to treatment on search intensity in the first two weeks. Column (2) shows that transit subsidies have a greater effect on those living further from job opportunities. Those in the 10^{th} percentile of distance from job opportunities are on average 5 miles from a vacancy; for this group treatment has nearly no effect ($-0.062 + 5*0.014 = 0.008$). On the other hand, for someone on average 9 miles to a job (90^{th} percentile), the treatment effect is very large, increasing search intensity by about twice the average treatment effect.

Of course, distance could simply proxy for heterogeneity based on some correlated variable. I check for this in column (3). I include interactions of all of the baseline demographic controls as well as census tract poverty rates and median income with the treatment dummy. As can be seen, controlling for these other treatment interactions actually strengthens the case that transit subsidies increase search intensity more for those living far from job vacancies. The interaction increases to 0.017 and the p-value drops to 0.014. The effect strengthens because distance to vacancies correlates positively with tract median income which has a negative point estimate on its interaction term,[13] and correlates negatively with being female, which has a strong positive interaction with treatment. The female-treatment interaction itself is interesting. Column (4) demonstrates that treatment does not induce any additional search by males, and that the entire increase in search intensity is due to females.[14]

The evidence in Table 5 indicates that transit subsidies facilitate greater search intensity for those living in distant neighborhoods with limited access to employment opportunities. This matches the predictions of search theory and the spatial mismatch hypothesis, strengthening the case that being randomly selected for treatment affects search intensity because it lowers the cost of moving across space. It is difficult to reconcile this observed treatment heterogeneity with other possible mechanisms, such as a placebo mechanism involving the psychological response of the treatment group to simply being chosen. Placebo effects would, if anything, have a lesser effect on those far from employment opportunities since a given increase in search effort will translate into fewer additional job interviews for such individuals. Altogether, the evidence indicates that treatment works through lowering search costs because transit subsidies matter most for individuals who live far from job vacancies.

6.4. Labor Market Outcomes

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[13] The interaction of tract median income and treatment is statistically insignificant, though, as are all omitted interaction coefficients. Also, no evidence exists for heterogeneous effects by program site.

[14] It is beyond the main goal of this paper to determine if this difference applies beyond the present experiment or is simply due to some unobserved characteristic correlated with being female. However, a gender differential in spatial mismatch effects could occur if women tend to apply to more spatially dispersed jobs or those requiring more interaction prior to hiring.

According to both standard search theory and the spatial mismatch hypothesis, public transit subsidies that encourage greater job search intensity should translate into different labor market outcomes. In particular, we would predict higher wages and possibly shorter unemployment duration (provided the reservation wage does not increase too much). In a spatial context, we would expect commute distance to increase for those obtaining employment. This could amplify the effect on wages as reservation wages rise with commute distance but simultaneously limit the effect on unemployment duration.

The results for employment suggest that receiving subsidized transit sped up finding employment. Figure 8 plots the Kaplan-Meier survivor functions for the unemployment duration data. These functions non-parametrically describe the probability of remaining unemployed after a given number of days for both treatment and control groups. The survivor function for the treatment group lies below that for the control group, indicating that for nearly all duration lengths the probability of remaining unemployed until that time is lower for the treatment group. A two-sided non-parametric Kolmogorov-Smirnov test cannot reject the null that the distribution of durations for the treatment group and the control group are identical (p-value of 0.157). Thus, the evidence on the effect of treatment on time to employment is suggestive rather than definitive. In any case, Figure 9 shows estimates and confidence intervals for the difference in (unconditional) probability of being employed after zero to ninety days. The first two columns of Table 6 provide examples of these estimates, showing the results at 40 and 90 days. These results formalize the idea suggested by Figure 8 that treatment leads to a large increase in employment probability in the short run with employment rising by a statistically significant 9 percentage points at 40 days. This difference diminishes over time to statistical insignificance after 90 days. Altogether, the unemployment duration results suggest that the burst in search intensity generated by treatment increases the job-finding rate during the first few weeks after treatment and moves forward the time of first employment. However, the noisy statistical results caution against reaching strong conclusions.

Results for earnings and wages are positive but statistically uncertain. Columns (3) and (4) of Table 6 examine the impact of being assigned to treatment on weekly earnings (by definition zero if observed not working) [15] and wage rates conditional on employment. Given selection issues in conditioning on employment, for wage rates I estimate a Tobit model (imposing a cutoff for observable wages at the minimum observed wage of $6.00). The point estimates of the effect on earnings and wage rates are large but imprecisely estimated.[16]

The theory of spatial mismatch suggests that if transportation were less of a barrier applicants would find not only better jobs and sooner but also potentially jobs further from home. The final column of Table 6 examines the distance travelled from home to work[17] using a Tobit model to account for selection into employment. Overall, those assigned to receive transit subsidies travel 5.9 minutes further to work, a 14 percent increase in travel time. However, this effect is not statistically significant. For both wage rates and work travel distances, results from OLS performed on the employed sub-sample are similar to the Tobit results.

7. Conclusion

This paper reports results from a randomized experiment that provided a transportation subsidy package consisting of a fee-reducing public transit card and a $50 in-kind transit subsidy

[15] Actual hours are used when available. For much of the sample, though, they are estimated by assuming 20 hours for part-time employment and 40 hours for full-time employment.

[16] Focusing on the median or other parts of the wage distribution does not provide additional precision; quantile regression estimates are positive but also statistically insignificant.

[17] Travel time is computed as the shortest trip on public transit from the individual's address to the employer's address on Monday at 8:00 a.m. according to WMATA's on-line Trip Planner. For some individuals, such routes do not exist at 8:00 a.m. on Monday. For these individuals I measure the value as close to 8:00 a.m. as possible.

to low-wage job seekers in Washington, DC. Standard theory predicts that this should lead to an immediate but temporary spike in search intensity, followed by improvements in labor market outcomes. The evidence matches the main predictions. Members of the treatment group apply to and interview for more jobs during the first two weeks after treatment, completing 19 percent more of these search actions than a control group receiving only standard job search assistance. After two weeks, the effect disappears. The transit subsidies most benefit those who live far from open job vacancies. I also find suggestive evidence that increased search intensity leads to improved labor market outcomes. While the results for increased search intensity are statistically strong, the results for labor market outcomes are subject to substantial statistical uncertainty.

In its most narrow interpretation, the evidence indicates that substantial benefits may result from incorporating transportation assistance into urban employment assistance programs for the poor. A simple transit subsidy induced large effects on search behavior and potentially large effects on labor market outcomes. Even if there were full compliance, the cost of treatment would not exceed $60 per person. Even a generous accounting of staff time suggests that administrative costs would be no more than $80 per person. If one takes the point estimates literally, the benefits to individuals receiving the transit subsidy easily exceed the combined $140 cost. A simple Cox hazard model can be used to measure a point estimate for the effect of treatment on the hazard rate of leaving unemployment. Using this point estimate and accounting for censoring by assuming an exponential distribution of unemployment durations, treatment causes the average unemployment duration to drop by about 13 days. Making the conservative assumption that treatment leads to no increase in wages, individuals assigned to treatment earn $575 more than those assigned to the control group. Obviously, this back-of-the-envelope calculation includes substantial statistical uncertainty and strong parametric assumptions, but the results indicate that the benefits of transit subsidies likely swamp the costs in this context. These small-sample results suggest that transportation subsidies for clients of employment agencies should be investigated with larger samples and in greater detail.

Of course, this rough cost-benefit calculation only includes the effects on those enrolled in the program. While this may be a reasonable approximation for a small-scale pilot program, any public policy involving transit subsidies would need to consider effects on non-participants (Heckman, Lochner, and Taber, 1998). The classic search and matching model predicts that untreated applicants applying to the same jobs may be harmed by the increased competition but also perhaps helped as firms create new vacancies to take advantage of the easier hiring situation.[18] The most skeptical perspective would be to assume that treatment generates no vacancy creation and that any positive effects on the treated simply displace others applying to the same jobs. Crépon, et. al., (2013) demonstrate that this is possible (at least for men) with a large-scale experiment varying the intensity of a public job search assistance program across localities. However, in a spatial search model negative displacement effects of lowering transit costs should most affect residents of job-rich areas (e.g. Manning and Petrongolo, 2013). If public policy aims to reduce spatial disparities in employment, then this negative effect may be the acceptable consequence of improving the job search abilities of those living in concentrated poverty. However, a policymaker motivated mainly by efficiency would need further information about the extent of spillover effects that is beyond the reach of the present study.

More broadly, this study provides the first experimental evidence that search costs over space matter for the urban poor. The strongest empirical results are (i.) that individuals receiving

[18] These offsetting externalities can lead to either inefficiently low or inefficiently high amounts of search (Hosios, 1990). A similar result holds true in models that also include spatial frictions (Wasmer and Zenou, 2002).

transit subsidies apply and interview for more jobs and (ii.) that this matters most for those who live furthest from available jobs. These two facts indicate that transportation costs constrain job search, particularly for those living in neighborhoods isolated from employment opportunities. Context and design of the intervention may be important in interpreting these results. The particular sample of individuals studied matters. Transit subsidies were provided to individuals who were both significantly disadvantaged and already looking for work. A modest transit subsidy would not likely have similar effects for people with significant amounts of wealth or those totally detached from the labor force. Also, subsidizing access to an existing public transit provides fewer logistical challenges than creating totally new transportation systems (e.g. Roder and Scrivner, 2005). The extensive public transit and relatively compact layout of Washington, DC allow me to focus on a simple subsidy for an existing public transit system, which may have allowed for successful intervention. But while context matters, the present study does generate some new facts. Transit subsidies can generate greater search intensity among the urban poor, and this is particularly true for those living far from job centers. Transportation costs can hinder job search for those living far from employment opportunities, perpetuating geographically concentrated poverty.

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Table 1: Recruitment and Compliance

			Number	Proportion
Total			775	1.00
Recruited			468	0.60
	Treatment		208	0.44
		Cards Received		
		0	21	0.10
		1	80	0.38
		2	104	0.50
		3	3	0.01
	Control		260	0.56
		Cards Received		
		0	260	1.00
Not Recruited			307	0.40
	Not Eligible		17	0.06
	Refused		290	0.94

Notes: Treatment provides up to two public transit cards each with a $25 in-kind subsidy for bus and train trips. The card also provides reduced per-trip fares.

Table 2: Baseline Characteristics, Recruited and Non-Recruited

	Recruited		Not Recruited		Difference	p-value
	Mean	SD	Mean	SD		
Black	0.98	0.13	0.96	0.16	0.02	0.02**
Age	40.47	11.5	40.28	11.02	0.19	0.78
Male	0.58	0.49	0.61	0.45	-0.03	0.31
No HS Diploma	0.20	0.40	0.18	0.36	0.02	0.38
HS Diploma	0.56	0.5	0.54	0.45	0.02	0.57
Some College	0.19	0.39	0.16	0.34	0.03	0.24
College Graduate	0.05	0.21	0.05	0.20	0.00	0.76
Ex-Offender	0.50	0.50	0.46	0.45	0.04	0.16
Public Assistance	0.66	0.47	0.49	0.46	0.17	0.00***
Employed	0.11	0.31	--	--	--	--
Duration of Most Recent Job	2.24	3.24	--	--	--	--
(Median)	(1.03)					
Current Unemployment Duration	1.76	3.16	--	--	--	--
(Median)	(0.90)					
Pct of Last Five Years Employed	0.46	0.30	--	--	--	--
Most Recent Wage	10.04	5.44	--	--	--	--
Most Recent Wage Missing	0.10	0.30	--	--	--	--
Immigrant	0.10	0.30	0.10	0.27	0.00	0.96
Driver's License	0.45	0.50	0.45	0.45	0.00	0.99
Access to a Car	0.09	0.29	0.14	0.30	-0.05	0.01***
Maryland Residence	0.16	0.37	0.14	0.32	0.02	0.30
Virginia Residence	0.02	0.14	0.02	0.14	0.00	0.71
Average Distance to Vacancies	6.70	2.43	--	--	--	--
Sample Size	468		650			

Source: Baseline administrative data. Difference is an un-weighted comparison of means. 1, 5, and 10 percent statistical significance are denoted by ***, **, and * respectively. "Recruited" refers to those participating in the experiment. "Not Recruited" refers to those who did not participate but used Jubilee Jobs services during the experimental time period. Sample sizes for non-recruited differ between Table 1 and Table 2 because of those who exit Jubilee services during an initial week of (pre-experiment) workshops. Table 1 covers only those who complete a week of workshops and Table 2 measures anyone who completes an intake interview before the week of workshops.

Table 3: Randomization Test for Analysis Sample Only

	Treatment		Control		Treatment-Control	
	Mean	SD	Mean	SD	Difference	p-value
Age	40.22	11.08	41.20	11.42	-0.94	0.41
Male	0.57	0.50	0.57	0.50	0.01	0.90
No HS Diploma	0.19	0.40	0.19	0.39	0.01	0.74
HS Diploma	0.55	0.50	0.57	0.50	-0.03	0.59
Some College	0.19	0.40	0.20	0.40	-0.01	0.85
College Graduate	0.06	0.24	0.04	0.19	0.02	0.33
Ex-Offender	0.50	0.50	0.47	0.50	0.02	0.72
Public Assistance	0.65	0.48	0.67	0.47	-0.02	0.71
Employed	0.11	0.31	0.11	0.32	-0.01	0.85
Duration of Most Recent Job	2.26	3.18	2.20	3.24	0.08	0.81
(Median)	(1.08)		(1.00)			
Current Unemployment Duration	1.92	3.50	1.73	3.06	0.18	0.58
(Median)	(0.80)		(0.89)			
Pct of Last Five Years Employed	0.49	0.30	0.48	0.29	0.00	0.95
Most Recent Wage	11.14	5.71	10.27	4.46	0.62	0.25
Most Recent Wage Missing	0.06	0.23	0.04	0.19	0.04	0.17
Immigrant	0.13	0.33	0.08	0.28	0.05	0.11
Driver's License	0.44	0.50	0.50	0.50	-0.07	0.18
Access to a Car	0.08	0.28	0.11	0.32	-0.05	0.11
Maryland Residence	0.15	0.36	0.17	0.37	-0.02	0.55
Virginia Residence	0.01	0.10	0.03	0.16	-0.01	0.28
Average Distance to Vacancies	6.84	2.75	6.66	2.25	0.07	0.77
Sample Size	190		237			

Source: Baseline administrative data. Difference is from a regression of the baseline characteristic on a treatment dummy and a dummy controlling for the initial phase. 1, 5, and 10 percent statistical significance are denoted by ***, **, and * respectively. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Table 4: Search Intensity, Regression Results

	(1)	(2)	(3)	(4)	(6)
Sample	First 14 Days Only	First 14 Days Only	All Days Prior to Employment	All Days Prior to Employment	All Days Prior to Employment
Unit of Observation	Individual-Day	Individual-Day	Individual-Day	Individual-Day	Individual-Day
Dependent Variable	Number of Search Actions	Number of Search Actions	Number of Search Actions	Number of Search Actions	Number of Search Actions; Poisson Model
Treatment	0.031**	0.031**	0.004	-0.007	-0.078
	(0.015)	(0.014)	(0.009)	(0.009)	(0.105)
	[0.037]	[0.030]	[0.64]	[0.42]	[0.45]
First 14 Days X Treatment				0.038***	0.243**
				(0.014)	(0.107)
				[0.008]	[0.024]
Dummy for Initial Phase	YES	YES	YES	YES	YES
Day Fixed Effects	NO	YES	YES	YES	YES
Demographic Controls	NO	YES	YES	YES	YES
Control Group Mean	0.164	0.164	0.104	0.104	--
Obs	5,879	5,879	20,962	20,962	20,962
R^2	0.001	0.04	0.04	0.04	--

Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Demographic controls include gender, age, age squared, ex-offender status, education dummies, and labor market history variables. Individual-day observations are only included if the day is prior to the individual finding employment, and standard errors are clustered at the individual level and in parentheses. Selected p-values are in brackets. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Table 5: Search Intensity, Heterogeneous Effects

	(1)	(2)	(3)	(4)
Sample	First 14 Days Only	First 14 Days Only	First 14 Days Only	First 14 Days Only
Unit of Observation	Individual-Day	Individual-Day	Individual-Day	Individual-Day
Dependent Variable	Number of Search Actions	Number of Search Actions	Number of Search Actions	Number of Search Actions
Treatment	0.031**	-0.062	-0.366	0.002
	(0.014)	(0.049)	(0.724)	(0.019)
Average Distance to Vacancies		-0.004	-0.009**	
		(0.004)	(0.004)	
Treat X Distance to Vacancies		0.014*	0.017**	
		(0.007)	(0.007)	
		[0.052]	[0.014]	
Treat X Female			0.071**	0.069**
			(0.030)	(0.029)
			[0.018]	[0.020]
Dummy for Initial Phase	YES	YES	YES	YES
Day Fixed Effects	YES	YES	YES	YES
Demographic Controls	YES	YES	YES	YES
Demographic Controls X Treat	NO	NO	YES	NO
Control Group Mean	0.164	0.164	0.164	0.164
Obs	5879	5878	5878	5879

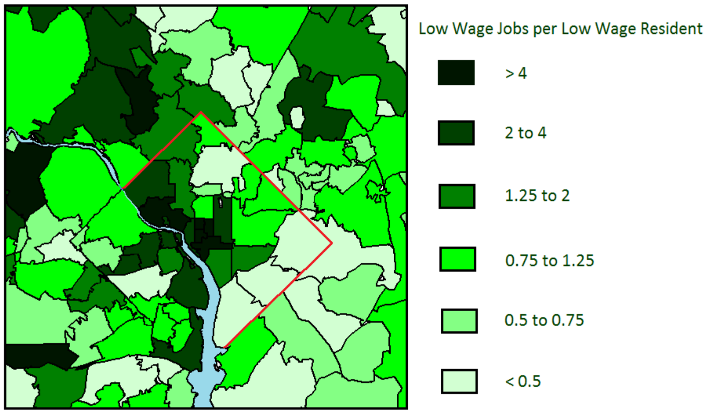
Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Demographic controls include gender, age, age squared, ex-offender status, education dummies, and labor market history variables. Column (3) also includes census tract level poverty rates and median income. Individual-day observations are only included if the day is prior to the individual finding employment, and standard errors are clustered at the individual level and are in parentheses. Selected p-values are in brackets. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Table 6: Employment Outcomes, Regression Results

Model	(1) OLS	(2) OLS	(3) OLS	(4) Tobit	(5) Tobit
Dependent Variable	Employed in 40 Days	Employed in 90 Days	Weekly Earnings	Wage Rate	Minutes to Travel to Work
Treatment	0.09**	0.04	15	0.57	5.9
	(0.05)	(0.05)	(18)	(0.50)	(5.8)
	[0.05]	[0.46]	[0.23]	[0.26]	[0.31]
Initial Phase Dummy and Demographic Controls	YES	YES	YES	YES	YES
Control Group Mean	0.26	0.46	135	9.61	42.3
Obs	440	427	424	427	422
R^2	0.05	0.04	0.06	--	--

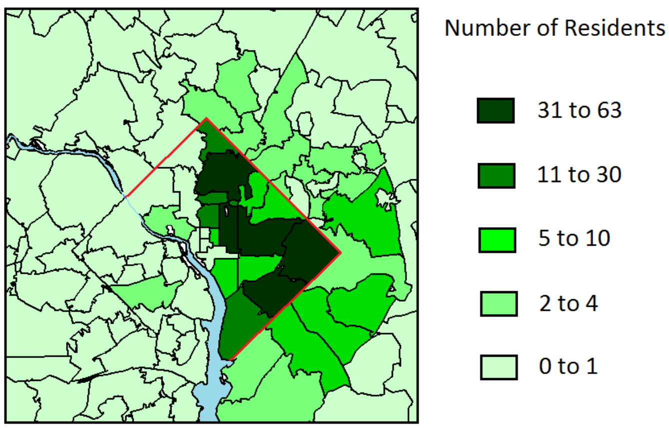
Statistical significance at the 1, 5, and 10 percent levels is denoted by ***, **, and * respectively. Demographic controls include gender, age, age squared, ex-offender status, education dummies, and labor market history variables. Heteroskedasticity-robust standard errors are in parentheses. Selected p-values are in brackets. The wage rate Tobit model assumes wages as unobserved if they are less than the lowest observed wage of $6.00; the commute distance Tobit has a cutoff at zero. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Figure 1: Ratio of Low-Wage Jobs to Low-Wage Residents, Across Zip Codes



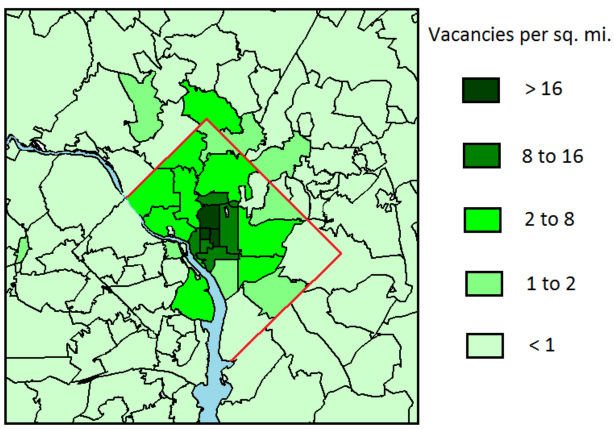
Source: 2010 Longitudinal Employer-Household Dynamics data. Low-wage jobs are those with workers earning $1,250 per month or less.

Figure 2: Residence of Experimental Sample, Across Zip Codes



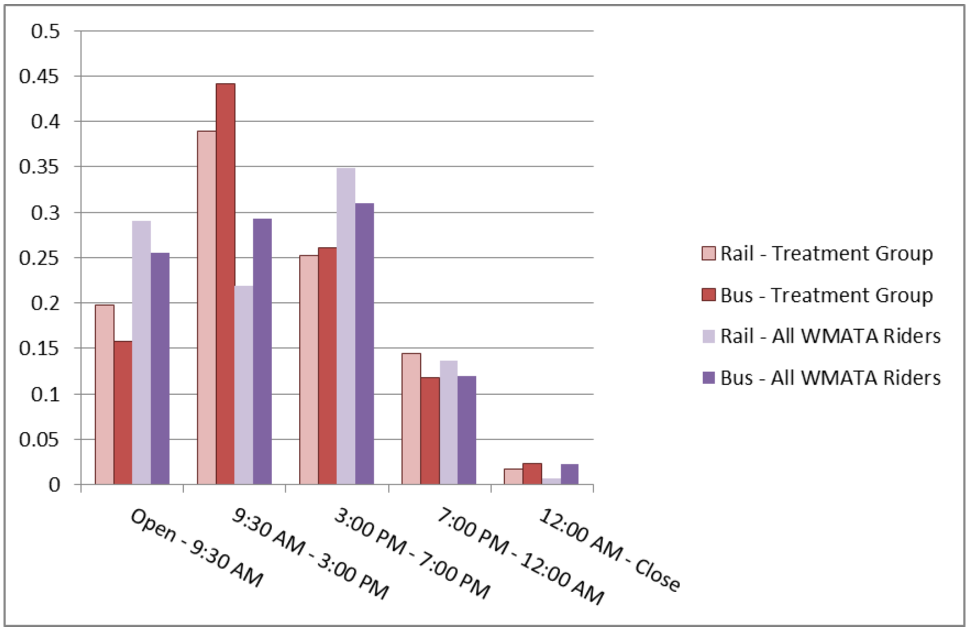
Source: Jubilee Jobs Administrative Data

Figure 3: Available Vacancies, Across Zip Codes



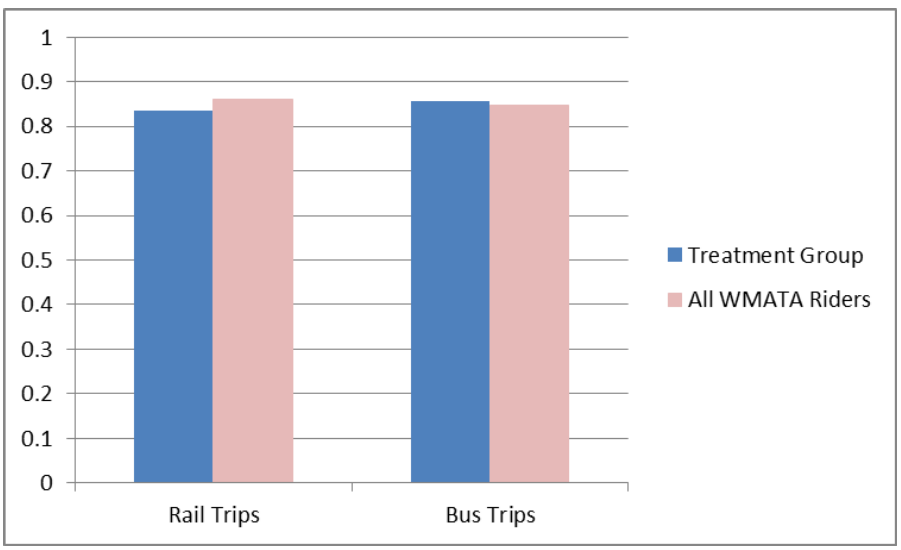
Source: Jubilee Jobs Administrative Data

Figure 4: Proportion of Bus and Rail Trips by Time of Day



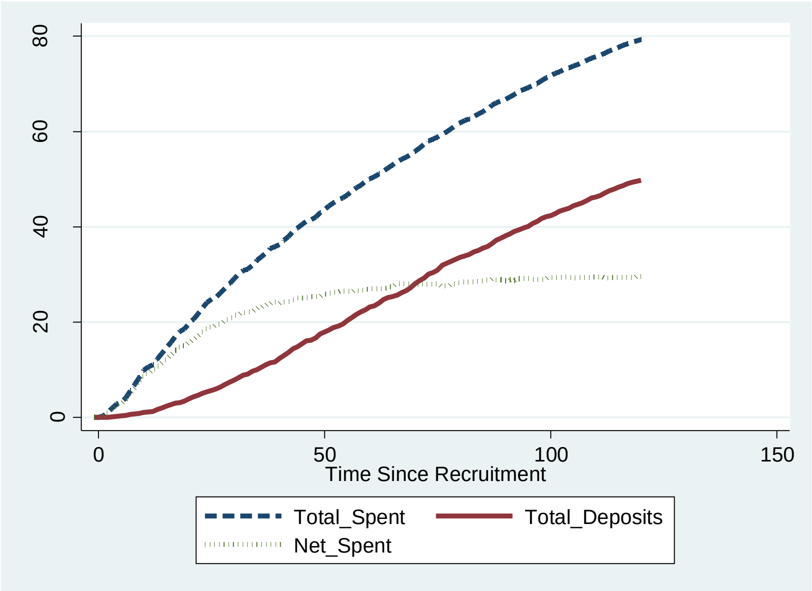
Source: Experimental treatment group is in red with dark outlines; all WMATA riders in purple with no outline; bus has dark interiors and rail has light interiors. SmarTrip card data for rail trips released for May 2012 by WMATA PlanItMetro. Metrobus data for July-September 2010 courtesy of WMATA public records request 11-0025. Data for the treatment group is from electronic records from SmarTrip cards provided to the treatment group. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Figure 5: Proportion of Bus and Rail Trips on Weekdays



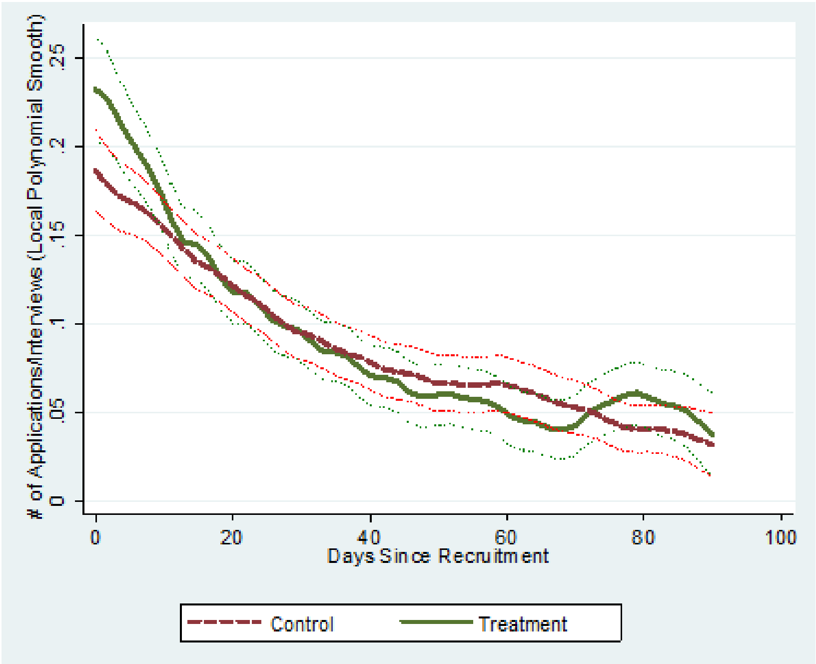
Source: SmarTrip card data for rail trips released for May 2012 by WMATA PlanItMetro. Metrobus data for July-September 2010 courtesy of WMATA public records request 11-0025. Data for the treatment group is from electronic records from SmarTrip cards provided to the treatment group. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Figure 6: Gross and Net Usage of SmarTrip Cards



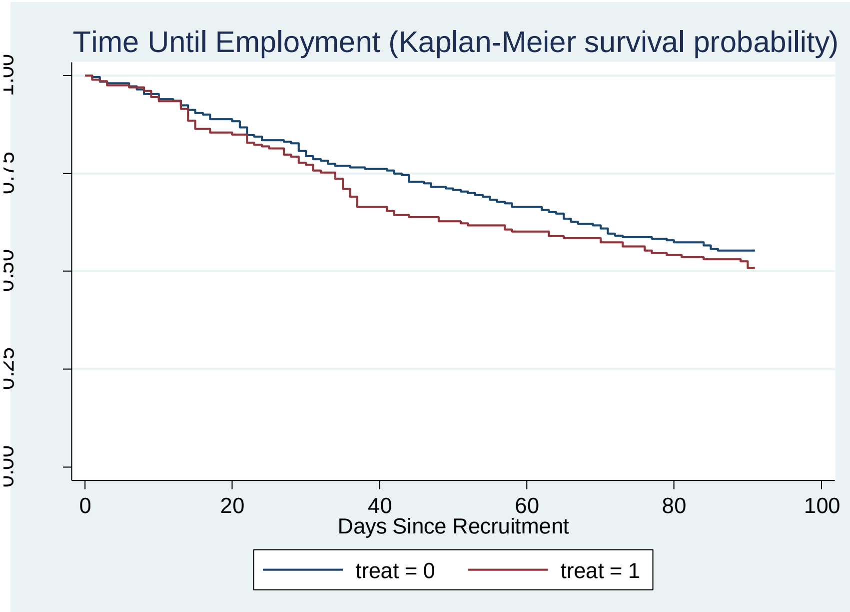
Notes: Simple means by days since recruitment. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Figure 7: Effect of Treatment on Search Intensity



Notes: Local polynomial smooth of applications/interviews per day; solid line is the treatment group; dashed line is the control group; dotted lines are 95% confidence intervals; treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares

Figure 8: Effect of Treatment on Unemployment Duration



Notes: Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares

Figure 9: Effect of Treatment on Unemployment Duration, By Duration Length

Notes: The confidence interval for the difference in probability of employment after X days is generated by a regression of a dummy for whether an individual is employed after X days on a treatment dummy. The regression includes an initial phase dummy and uses robust standard errors. Treatment provides public transit cards with up to a $50 in-kind subsidy for bus and train trips as well as reduced per-trip fares.

Online Appendix

A.1. Data Sources, Follow-Up, and Attrition

I focus on two types of outcomes, those regarding employment and those regarding aspects of the search process. Administrative records from Jubilee Jobs track job search and employment outcomes for most of the sample. These records allow me to measure wages, unemployment duration, and an application/interview history that can be used to measure search intensity. These data also include home addresses and job locations that can be used to measure distance to jobs, interviews, and applications. I collect these records 3 months after enrollment of an individual in the study.

For all individuals, I measure search activity including the number, timing, and location of job applications. For these outcomes I use only Jubilee Jobs administrative data on the application and interview history of the individual as recorded by their job counselor. This data includes only those actions reported to an individual's job counselor and thus likely underestimate search behavior. Individuals in the experiment may apply or interview for jobs without reporting the activity to the job counselor. More broadly, increased search intensity may also entail activities such as information gathering that I do not measure. As a result, any estimated treatment effects are likely underestimates. Attrition in this data is due to missing paper records and is limited to 3 out of 468 records. Selection out of this dataset occurs either when the individual finds employment or loses contact with Jubilee Jobs. While I will use the entire application history at times, to prevent bias from selection out of job search, I focus on search outcomes from the first two weeks after randomization. Up to that point employment rates are under 10 percent and the difference between treatment and control is less than 1 percent.

Meanwhile, dropout rates are similarly low. Sample selection bias will be less of a concern during this time period.

For employment outcomes, I combine administrative data with a phone survey. After 3 months, individuals fall into three groups: those who have reported a job placement to Jubilee Jobs, those who continue to search for work with Jubilee, and those who have lost contact with Jubilee Jobs. The first group has complete data on employment outcomes in the administrative data. For the second group, durations are censored at 90 days and employment characteristics cannot be observed. I take account of this censoring in analyzing durations. When considering earnings, one might be concerned that selection into employment biases estimates of the treatment effect on wages and earnings. To account for this, I examine both wage rates conditional on being employed at 90 days (using a Tobit censoring model) and weekly earnings, where earnings are observed as zero for those who are unemployed. For outcomes regarding the location of employment, I analyze distance to work using a Tobit model. The final data follow-up group, those who drop out of Jubilee's program and thus the administrative data, provide a more important problem. About 40 percent of the sample drops out of the administrative data between baseline and follow-up; however, there is no statistical difference between attrition in the treatment and control groups. Nonetheless, I attempt to complete the missing employment data using a phone survey of all individuals who drop out of the administrative data. The survey asks questions on employment and job characteristics. As listed in Appendix Table 1, attrition is reduced to 9 percent by the phone survey, and differential attrition between treatment and control remains negligible. For the small number of individuals who cannot be tracked in administrative data or contacted by phone no data on wage rates, earnings, or commute distances are available and they are excluded from these analyses. For unemployment duration, most (all but 9) have at least a date of last contact with Jubilee Jobs. In these cases, observations are considered unemployed until this date and then censored afterward. Altogether, the combination of administrative and phone survey records provide labor market outcomes data with relatively low attrition.

Finally, I also collect data on usage of the transit cards. Electronic tracking of the cards provides time-stamped data on card transactions and balances. While I only have this information for the treatment group, these data assist in confirming that applicants do in fact use the cards and also provide some details on whether applicants appear to be using the cards for job search or for other purposes.

A.2. Recruitment and Representativeness of the Sample

Lack of informed consent prevents me from directly collecting data on reasons for non-participation; however, observation and volunteered information point to lack of time for informed consent as the main reason. As noted above, potential subjects were not warned about the experiment beforehand, and many had other immediate obligations that could not accommodate the sometimes significant time required to sign up (e.g. family plans, parole officer meeting). For some individual baseline characteristics, Jubilee Jobs was able to provide summary statistics on all individuals who were enrolled in their program at the time of recruitment for the experiment. Combining these summary statistics with baseline data on individuals who participate, I am able to calculate summary statistics for the non-participants in a manner that does not require obtaining their individual-level data.

As summarized above, Table 2 reports baseline characteristics of individuals participating in the experiment and compares them to average characteristics of those who did not participate. The first column reports mean baseline characteristics for those who were recruited. Nearly the entire sample, 98 percent, identifies as black or African-American and the average age is 40 years. The baseline data confirm that these low-wage job seekers face substantial disadvantages in the labor market. Only 24 percent of the sample attended any schooling after high school, with only 5 percent completing college and 20 percent not completing high school. Half of the sample is listed by their job counselors as having a criminal history and two thirds are receiving some type of public assistance (e.g. food stamps, TANF, SSI). Only 11 percent of the sample is employed at baseline.[19] Strikingly, the average individual in the sample has been unemployed for 1.8 years (with a median of just under 1 year) and been employed for only 46 percent of the past 5 years. When employed, the most recent wage was about 10 dollars per hour, and turnover was high with median job duration of 1 year. While nearly everyone in the sample identifies as black, 10 percent also identify as immigrants, over half of whom are from Ethiopia. The baseline data identify the sample as a group of people reliant on public transit. More than half of the sample lacks a valid driver's license, and only 9 percent have access to a car. Finally, 82 percent of the sample resides in Washington, DC with most of the remainder living in the Maryland suburbs east of the city. As covered in more detail above, the sample inside DC disproportionately resides in Wards 7 and 8 in the Southeast part of the city, with very few applicants coming from the more affluent western part of the city. As a result, members of the sample would travel on average 6.7 miles to the vacancies displayed in Figure 3.

For most of these characteristics, summary statistics can also be computed for those who did not participate. These are reported in the second column of Table 2. Differences between mean characteristics for recruited and non-recruited individuals, along with p-values testing if

[19] Most of these individuals are employed part-time, and in the analysis that follows I analyze outcomes related to finding a new job.

the two group means differ, are reported in the final two columns. For the most part, individuals recruited into the experiment are similar to those who did not participate with no average differences in age, gender, educational attainment, ex-offender status, immigrant status, ability to drive, or residential location. However, individuals do appear to have selected into the experiment on a couple dimensions. The experimental sample is 17 percentage points more likely to be receiving public assistance and 5 percentage points less likely to have access to a car. Those recruited are also more likely to be black, though this difference is relatively small. Altogether, this baseline comparison indicates that individuals who selected to participate in the experiment had less access to transportation and a lower level of assets at baseline relative to individuals who decided not to participate. The baseline data indicate that my results will apply to populations with heavy participation in public assistance programs and limited access to private vehicles. In any case, a scaled-up policy of transit subsidies for urban job-seekers would likely attract people with similar characteristics, making this a relevant sample in which to test the efficacy of transit subsidies for low-wage job seekers.

It will also be useful to demonstrate the ways in which the experimental sample is (or is not) representative of not only the population of Jubilee Jobs applicants but also the broader population. The external validity of the results will depend on how this context compares to other settings. To provide some sense of context, I compare the characteristics of the sample to data on respondents to the Current Population Survey in Table A.2. The first column of the table lists the educational attainment, age distribution, and gender distribution of the experimental sample. The next three columns summarize 2010 CPS data for progressively selective groups. The second column describes all 18-65 year-old respondents; the third column 18-65 year-old unemployed individuals; and the fourth column 18-65 year-old, unemployed, black individuals living in metropolitan areas. The gender composition of the sample does not match the full working-age population but is representative of unemployed blacks in cities. Not surprisingly, the experimental sample has lower education attainment than the average working age person, with only 5 percent achieving at least a Bachelor's degree as opposed to 28 percent of the general population. This gap narrows considerably but does not completely disappear when the sample is compared only to black, unemployed individuals in metro areas, of whom 11 percent completed a Bachelor's degree. There are major differences in age distribution between the experimental sample and a random sample of unemployed, black individuals living in metropolitan areas. While young people age 18-25 make up 31 percent of the CPS sample, only 11 percent of the experimental sample is this young. Instead, the experimental sample has a disproportionate number of middle aged individuals. The sample is most representative of mid-career, unemployed black individuals living in metropolitan areas who are somewhat less educated than other similar individuals. Some caution should be taken in applying the results to the urban unemployed youths, but otherwise they should be applicable to people most affected by spatial mismatch.

A.3. Theory

The first order condition of maximizing (3) yields:

$$\frac{\lambda'(s_t)}{\lambda'(s_{t+1})} = \frac{[1-\lambda(s_t)]\delta[V(E)-V(U,k_{t+2})]}{V(E)-V(U,k_{t+1})}$$

Then, substituting (3) evaluated at the optimum into this yields (4). To show that search intensity is monotonically decreasing, note that:

$$u(y_L) + \delta V(E) - V(U,k_{t+1})$$

$$= u(y_L) + \frac{\delta}{1-\delta} u(y_H) - V(U,k_{t+1})$$

$$= [u(y_L) - u(y_H)] + \frac{1}{1-\delta} u(y_H) - V(U, k_{t+1})$$

$$\leq V(E) - V(U, k_{t+1})$$

Then, concavity of $\lambda(\cdot)$ applied to (4) implies the result.

Appendix Table 1: Attrition

	Whole Sample		Treatment		Control	
Cards	Number	Proportion	Number	Proportion	Number	Proportion
Non-Attriters	427	0.91	190	0.91	237	0.91
-Administrative Data	289	0.62	133	0.64	156	0.60
-Phone Survey	138	0.29	57	0.27	81	0.31
Attriters	41	0.09	18	0.09	23	0.09
Total	468	1.00	208	1.00	260	1.00

Appendix Table 2: Representativeness of Sample

Sample	Experimental Data All	2010 CPS All Working Age	2010 CPS Unemployed	2010 CPS Unemployed, Black Metro Area
Less Than High School	20	13	19	17
HS Diploma	56	30	39	43
Some College	19	29	27	28
Bachelor's or More	5	28	15	11
18-25	11	18	25	31
25-30	11	11	13	16
30-35	13	10	11	11
35-40	9	11	11	10
40-45	14	11	11	10
45-50	18	12	11	9
50-55	13	11	9	7
55-60	7	10	7	4
60+	1	8	4	3
Percent Female	42	51	39	41
N	468	128845	9473	1511

All data are listed in percentages. Data for the first column are from the present study. The remaining columns draw from the 2010 samples of the Current Population Survey.