# Financial Access and Labor Market Outcomes: Evidence from Credit Lotteries*

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**Abstract**

We assess the employment and income effects of access to credit dedicated to investment in individual mobility by exploiting time-series variation in access to credit through random lotteries for participants in a group-lending mechanism in Brazil. We find that access to credit for investment in individual mobility increases formal employment rates and salaries, yielding an annual rate of return of 12 percent. Consistent with a geographically broader job search, individuals transition to jobs farther from home and public transportation. Our results suggest that accessing distant labor markets through credit for investment in individual mobility yields high and persistent returns.

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# 1 Introduction

Various interventions have been proposed to overcome hurdles to economic development for low-income households. Much hope has been placed in the transformative power of financial access as the marginal return on capital should be largest for the most capital-constrained individuals. Yet randomized control trials across a diverse set of settings and countries document modest or no effects of extending credit to low-income households (Banerjee et al., 2015; Crepon et al., 2015; Angelucci, Karlan, and Zinman, 2015; Attanasio et al., 2015; Augsburg et al., 2015). These findings raise the question of whether the return on capital is generally lower than expected for credit-constrained households or whether interventions require better targeting of populations and investments that generate higher returns.

We contribute to this debate by documenting that facilitating access to credit for investment in individual mobility yields high and persistent returns. We exploit data on participants in a group-lending mechanism in Brazil, which generates time-series variation in access to credit tied to the purchase of a motorcycle. We find that formal employment rates increase by 16 percent and salaries are 8 percent higher 5 years after obtaining credit for investment in individual mobility. Access to credit for investment in individual mobility yields an annual real rate of return of 12 percent. Consistent with the ability to engage in a geographically broader job search, we find that individuals transition to jobs farther from home or public transportation. The effects are larger for lower-income individuals and in areas with less developed public transportation and sparse local labor markets.

Consorcios are a widespread group-lending mechanism for financing durable goods in Brazil, with more than 6.7 million participants in a given year. We focus on motorcycle groups, which tend to comprise credit-constrained individuals seeking to invest in individual mobility.[1] Every month, participants in a consorcio make identical contributions, which are then allocated to a subset of participants as credit designated for motorcycle purchase. Recipients of credit are determined through lotteries and auctions. When allocating credit through lotteries, consorcios use a contractually specified algorithm to translate the outcome of the national lottery (*Loteria Federal*) into ticket numbers that have been assigned to all participants beforehand.[2] All participants continue their contributions until everybody has been awarded credit. In many ways, consorcios resemble rotating savings and credit associations (ROSCAs) (Besley, Coate, and Loury, 1993; Besley and Coate, 1995). One main difference is that enforcement operates through physical collateral rather than social capital, as participants share no social ties and do not live in geographic proximity.

[1] One-third of motorcycles in Brazil are sold through consorcios (ABAC, 2017). From 2009 to 2016, more than 10 million individuals (6.6 percent of working-age population) participated in a motorcycle consorcio.

[2] This ensures that lotteries are transparent and fair. The algorithm is designed such that ex ante, each participant has the same probability of winning the lottery in a given month (see Section 2.2 for details).

Identifying a causal effect of access to credit for investment in individual mobility on labor market outcomes is challenging. Credit access results from endogenous decisions, which depend on characteristics that may be correlated with other economic variables.

To overcome this challenge, we exploit time-series variation in access to credit in consorcios. Most groups allocate credit through both lotteries and auctions. While we restrict the sample to participants who obtain credit through lotteries, the presence of auctions generates an endogeneity problem with respect to changes in the pool of lottery participants over time. Specifically, individuals who remain in the lottery pool have not obtained credit through auctions, which may reflect characteristics correlated with the labor market outcomes. The design of consorcios allows us to simulate all groups as if credit were allocated only through lotteries. Specifically, since we know the algorithm a group employs to translate the national lottery number into the winning ticket number, we can identify who would have obtained credit through a lottery if the group held no auctions. We use the simulated lottery winners as an instrument to predict actual lottery winners. We then employ the instrument in a staggered difference-in-differences methodology comparing outcomes for participants who are predicted to win a credit lottery with outcomes for participants who have not yet been predicted to win a lottery within the same group. Since the instrument is based on random lotteries, it is orthogonal to individuals' characteristics and satisfies the exclusion restriction.[3]

We observe a 2.37 percentage point (5.8 percent) relative increase in formal employment rates in the first year after individuals win a credit lottery, which increases to 6.64 percentage points (16 percent) 5 years after winning. For salaries, we observe a relative increase by 3.12 percent in the first year after winning a credit lottery, which increases to 8 percent 5 years after winning. Our data also allow us to compute two metrics to capture commuting patterns: the distance between where an individual works and where they live (henceforth, commuting distance), and the distance between where they work and the nearest public transportation stop (henceforth, transportation distance). For commuting distance, we observe a relative increase of about 3.22 percent in the first year after winning a credit lottery, which increases to 15.25 percent 5 years after winning. For transportation distance, we observe a relative increase of 1.81 percent in the first year after winning a credit lottery, which increases to 5.88 percent 5 years after winning.[4]

In the cross-section, we observe heterogeneity in treatment effects that vary with individual- and location-specific characteristics. Individuals who live in areas with less developed public

[3]Since participants in a consorcio do not share social ties and do not live in geographic proximity, concerns about multiplier or other general equilibrium effects that may differentially affect treated and untreated individuals are limited (Cai and Szeidl, 2022; Breza and Kinnan, 2021).

[4]Individuals may also generate income from a motorcycle as a production factor. This type of activity is mostly confined to the informal sector, which prevents us from observing it in the data.

transportation and fewer local employment opportunities and younger individuals with lower salaries experience greater increases in employment, salaries, and commuting distance. These findings suggest that investment in individual mobility can be a substitute for public transportation, and that returns to credit for investment in individual mobility are higher for young, low-income individuals who live in areas with sparse local labor markets.

We complement our analysis with additional tests to tighten the interpretation of the results. We document that our results are robust to systematic heterogeneous treatment effects across individuals over time by applying the estimation in Sun and Abraham (2021), which is robust to such heterogeneity. We find that the estimates are strikingly similar with and without application of the Sun and Abraham (2021) methodology, which suggests that our estimates are not biased as a result of heterogeneous treatment effects. In additon, we show that hours worked in the formal sector do not change after obtaining access to a motorcycle, which mitigates concerns that our salary results could reflect the consolidation of smaller jobs in the formal and informal sectors into a larger formal sector job. Finally, our results are very similar for municipalities with high and low levels of labor market informality, which suggests that they are not driven by the presence of informal labor markets.

Our paper relates to an active debate on returns to facilitating access to capital for capital-constrained individuals. While much hope has been placed in a transformative impact of financial access, a number of recent studies suggest that returns are modest at best (Karlan and Zinman, 2011; Attanasio et al., 2015; Augsburg et al., 2015; Banerjee et al., 2015; Crepon et al., 2015; Tarozzi, Desai, and Johnson, 2015; Meager, 2019; Gertler, Green, and Wolfram, 2021).[5] However, recent evidence suggests that while the return on access to capital is low on average, it may be high for specific groups. For example, Banerjee et al. (2021) and Meager (2022) find positive effects of easier access to credit only for entrepreneurs with entrepreneurial experience, which suggests that credit expansion may be more productive at the intensive rather than the extensive margin. Karlan and Zinman (2011) find that returns on investment are larger for higher-income male entrepreneurs.

The results in our paper contribute to this debate by showing that access to credit tied to investment in mobility can yield high and persistent returns. Many programs and RCTs extend credit or cash grants to entrepreneurs, based on the insight that credit is essential for starting a new business, which requires a large upfront investment. In contrast, participation in labor markets is typically assumed not to require an upfront investment. Our results question this assumption by showing that individuals may be unable to freely

[5] Credit constraints are not exclusive to mid- or low-income countries. The evidence for developed countries is mixed. While some studies find positive effects of extending credit to low-income households (Zinman, 2010; Morse, 2011; Morgan, Strain, and Seblani, 2012), others document negative effects (Melzer, 2011; Campbell, Martinez-Jerez, and Tufano, 2012; Carrell and Zinman, 2014; Skiba and Tobacman, 2019).

participate in labor markets due to spatial mobility constraints. Overcoming these may require a large upfront investment and therefore access to credit. This insight is consistent with recent evidence in Banerjee, Duflo, and Sharma (2021) that persistent long-term effects on labor income from a cash grant program in West Bengal are tied to migration to more distant urban centers. Studies that report which type of investment individuals undertake after credit constraints are relaxed repeatedly identify investment in mobility as one of the highest priorities (Karlan and Zinman, 2010; Kaboski and Townsend, 2012).

Participants in motorcycle consorcios are individuals who self-select into credit for investment in mobility. This matters. In a recent study, Beaman et al. (forthcoming) document that cash grants only yield positive returns to individuals who have previously sought access to credit. This suggests that targeting a population that actively seeks access to capital is important. Typically, endogeneity problems prevent researchers from drawing firm conclusions from non-experimental variation in access to credit, since selection into loan contracts is likely to be endogenously related to other economic variables. In this respect, consorcios are unique as an institutional setting, since all participants select into the credit product and variation in the timing of access to credit is determined through random lotteries. This allows us to provide evidence on the effects of access to credit for a population that selects into the credit market in a non-experimental setting.

Our results also relate to the literature on mobility and labor markets more broadly. The idea of spatial mismatch between where individuals live and job opportunities has been around for a while in the urban economics literature (Kain, 1968; Ihlanfeldt and Sjoquist, 1998). While theoretically plausible, establishing the prevalence of spatial mismatch in the data is challenging. Individuals and firms may optimize their location such that spatial mismatch is not a first-order concern. For example, Marinescu and Rathelot (2018) argue that spatial mismatch explains only 5.3 percent of U.S. unemployment, since job seekers live close to potential vacancies, whereas Heise and Porzio (2022) suggest that removing spatial frictions in Germany leads to aggregate productivity gains. We find that both the extensive margin effects (employment rates) and the intensive margin effects (salaries) of mitigating spatial constraints are economically large, and individuals with access to individual mobility earn significantly higher salaries.[6]

# 2 Institutional Background

This section provides a detailed description of consorcio groups and how they allocate credit designated for the purchase of durable goods and describes and discusses potential market

[6] Bryan, Chowdhury, and Mobarak (2014) find positive consumption effects of cash grants for migration.

frictions in Brazil that may give rise to spatial mismatch in the labor market.

## 2.1 Consorcios

First, we describe how consorcios are organized, how they allocate credit, and provide some aggregate statistics on consorcios in Brazil.

**Basic Features** Consorcios are a financial product in which participants pool funds to save toward the purchase of durable goods. Consorcios are typically administered by the finance division of a manufacturer who provides the good, a bank, or a specialty finance company. The administrator is in charge of marketing the consorcio, selecting participants, managing payments, and enforcing contracts. The administrator is compensated for these services through an administrative fee levied on all participants. Screening of applicants is virtually nonexistent, and it is easy for anyone with a social security number in Brazil to sign up and participate.

Prospective participants are provided with several pieces of information when selecting a group. They know the identity of the administrator, the price of the good, the number of months for which the group will operate, and the target number of participants. All participants contribute identical predetermined payments at regular intervals, typically monthly. These payments are adjusted for inflation. Monthly payments also cover the administrative fee and establish a guarantee fund that covers losses from defaults of individual participants.[7] All participants are required to continue their monthly contributions, including those who have received credit. The group continues until all participants have won a lottery.

Due to the organization of the group through a central and independent administrator, personal connections between consorcio participants are uncommon and participants in the same group are not known to each other. Enforcement relies on physical collateral generated by the durable good purchased through the group.

**Credit Allocation** All participants start out as savers making equal contributions to the group. Every month some participants receive credit from the group. Which members receive credit in a given month is decided using two mechanisms: lotteries and auctions. The relative numbers of lotteries and auctions vary by group. By law, at least one good has to be allocated through a lottery each period.

Lotteries are based on the national lottery in Brazil (Loteria Federal), which is broadcast on TV. Each participant receives a ticket number at the start of the group. Based on an

[7] In most groups a fraction of the payments insures the collateral value of the good.

algorithm, which is contractually specific, the national lottery number is translated into a ticket number and the participant holding the respective ticket number is declared the winner of the lottery. Each algorithm is designed such that at the beginning of the group each participant has the same unconditional probability of winning the lottery at any point in time. A detailed description of such an algorithm is provided in Appendix A.1.

In credit auctions, participants bid a fraction of the total value of the good. Rather than a higher aggregate payment, bids move payments forward, akin to making a higher down payment on the loan given by the group. Future contributions are adjusted accordingly. For example, if the value of a good is $5,000 with monthly contributions of $100 and a participant bids 40 percent, they would pay $2,000 immediately and would cease making monthly payments 20 months before the end of the group.

**Defaults** After obtaining credit from either a lottery or an auction, the good is purchased and becomes the property of the group. The good serves as physical collateral and can be seized if payments are late.[8] Participants cannot sell the good to somebody else without the approval of the administrator to ensure that the good is not transferred to someone who is a high credit risk for the group.

If a participant defaults before receiving credit, their past payments are retained by the group until they win a lottery. Once they win a lottery, their funds are released instead of credit being allocated. However, defaulted participants receive only a fraction of their previous payments, since default carries a contractually specified penalty of, on average, about 40 percent of the payments for motorcycle groups.[9]

Because of this contractual design, defaults of participants before receiving credit do not affect the required payments of other participants. However, defaults after receiving credit impose costs on the group if the collateral value of the good is not sufficient to recover the full amount of credit owed to the group. The resulting losses are first covered by the guarantee fund, which is designed to be sufficiently generous to make the collapse of the group highly unlikely. If losses exceed the capacity of the guarantee fund, administrators usually absorb the losses. In practice, losses from defaults virtually never exceed the capacity of the guarantee fund. At the termination date of the group, any remaining funds in the reserve fund are split equally and repaid to participants.

[8]Consorcios register all real estate and vehicle collateral under the fiduciary lien (Alienação fiduciária), which allows for out-of-court settlement in the event of default. As a consequence, collateral can be recovered quickly upon default.

[9]In groups started before 2009, participants who defaulted before receiving credit had to wait until the end of the group to have their payments returned.

**Aggregate Statistics** Consorcios are widespread in Brazil. In 2015, consorcios had 6,705,673 participants, and about half were in motorcycle consorcios. The 3,407,678 motorcycle consorcio participants are equivalent to 2.25 percent of the working age population or 6.82 percent of formally employed individuals in Brazil. In 2015 alone, 444,636 individuals, or 0.29 percent of the working-age population obtained a motorcycle through a consorcio.

The average motorcycle value across all groups is USD 2,837. Average monthly payments amount to about 3 percent of the value of the motorcycle. These payments cover the costs of the motorcycle, an average administrative fee of 16.84 percent of the value of the motorcycle, and a guarantee fund to cover losses. The average duration of a consorcio is 48.4 months. The share of motorcycles allocated through lotteries is 36.71 percent with the rest allocated through auctions. Consistent with consorcios' not relying on social ties among participants, the average group comprises 284 participants from 153 different municipalities in 17 different states (out of 27). Thus, fewer than two participants in the same group are from a given municipality.

Not all participants eventually obtain a motorcycle. 23.06 percent exit before they obtain credit due to missed payments.[10] Participants who exit before obtaining a motorcycle have their payments returned after a deduction of an average penalty of 40 percent. As described above, these funds are not returned immediately. For groups started before 2009, funds were returned at the end of the group. For groups started in 2009 or later, participants' funds are returned when they are drawn in a lottery. An additional 16.42 percent of participants default after receiving credit, in which case the motorcycle may be seized by the group to cover outstanding payments. If the liquidation value of the motorcycle is higher than the outstanding payments, non-defaulted participants keep the difference.

## 2.2 Market Frictions and Spatial Mismatch

To highlight the importance of being able to engage in a geographically broad job search, in Table 1, we list the numbers of firms, jobs, and distinct occupations that are accessible at varying commuting distances around individuals' homes. Individuals on average have access to 234 firms, 2,785 jobs, and 33 occupations within a 1 km commuting distance. Access increases to 1,488 firms with 23,387 jobs in 138 different occupations within a 3 km commuting distance, and to 9,227 firms with 179,169 jobs in 554 different occupations within a 20 km commuting distance. These numbers suggest that being able to engage in a geographically broader job search increases the scale and scope of available employment opportunities.

[10]In addition, some individuals sign up but change their mind before making payments, in which case they are excluded from the group.

Below, we discuss market frictions in Brazil that may give rise to spatial mismatch between firms and workers: transport infrastructure development, access to individual mobility, and geography.

**Transport Infrastructure** Public transportation and road infrastructure are not well developed in Brazil. According to the World Economic Forum's Global Competitiveness Report (WEF, 2019), Brazil scores poorly on the quality of its transport infrastructure. Of the 141 countries studied, Brazil ranks 116$^{\text{th}}$ in the quality of road infrastructure and 86$^{\text{th}}$ in the efficiency of train services. Thus, access to individual mobility is important as a substitute for public transportation in Brazil. In addition, commuting may be more difficult without access to motorized individual mobility, since other modes of transportation e.g. bikes may be harder to use in case of lower road quality or inefficiently long routes.

**Access to Individual Mobility** While motorcycle ownership is fairly common in Brazil, a significant fraction of individuals do not have access to motorized individual mobility. In a Pew Research Center Study from 2014 (PEW, 2014), 47 percent of Brazilians had access to a car, and 29 percent to a motorcycle or scooter. This compares with 88 percent car ownership and 14 percent motorcycle or scooter ownership in the U.S. Thus, even if we assume no overlap between car ownership and motorcycle ownership, at least 24 percent of Brazilians do not have access to motorized individual mobility. For these individuals, gaining access to a motorcycle constitutes a significant improvement in their access to individual mobility and their ability to commute longer distances. In addition, motorcycles need to be replaced over time. Thus, gaining access to a new motorcycle is important to sustain the ability to access distant labor markets.

**Geography** Brazil is a geographically large country, where physical distances between towns and to bigger cities can be large. Population density is almost one-third lower than in the U.S., which is also a geographically large country. This makes it even more important for individuals to be able to commute longer distances to reach urban centers with the employment opportunities they offer.

# 3 Data

The data for this paper come from two main sources. Data on consorcios is from the Sistema de Administracao de Grupos/Cotas de Consorcio (SAG) database, which is maintained by the Banco Central do Brasil (BCB). Data on labor market outcomes is from the Relacao

Anual de Informacoes Sociais (RAIS), an employer-employee matched database that includes employment information and wages for all formally employed workers in Brazil.

The database on consorcios provides information on the administrator, all participants, the good being allocated, and the dates when credit is awarded to participants. The BCB has been collecting data on all consorcio groups since October 15, 2008, including consorcios that started earlier but were still active. The earliest starting date for a consorcio in our sample is January 2006 and the sample ends in December 2015. For our empirical analysis, information about the algorithms through which consorcios translate the national lottery draw into a number that matches the ticket number of a participant is required, but is not readily available in the database. We hand-collect data from as many administrators as possible and verify the algorithms in the data.

The consorcio database provides the social security number of all participants. This allows us to match them to the RAIS database. The RAIS database records information on all formally employed workers and is maintained by the Ministry of Economics. All formally registered firms in Brazil are legally required to report annual information on each worker the firm employs. RAIS includes detailed information on the employer (tax number, sector of activity, establishment size, geographic location), the employee (social security number, age, gender, education), and the employment relationship (salary, tenure, type of employment, hiring date, layoff date, reason for layoff, etc.). Consistent with the sample period for consorcios, we use data from RAIS for the period from 2003 to 2015. By the end of 2015, the database covers about 50 million formal employees.

The final sample for our analysis comprises all groups for which we can collect the algorithm used to translate the national lottery number into a number that matches the ticket number of a participant, and our algorithm correctly predicts at least one lottery winner. Our sample comprises all lottery winners for each of these groups. Table 2 provides descriptive statistics for our sample. Panel A provides descriptive statistics for consorcios and Panel B reports pre-treatment characteristics for consorcio participants as well as for the working-age population and all formally employed workers.

The data contain 8,777 consorcios. The sample of members of these groups who win a lottery leaves us with an average group size of 56 participants, with a median of 43 participants. The average group lasts for 43 months, with a median of 36 months. The average monthly salary across all participants in motorcycle consorcios is BRL 1,160, compared with the average salary of BRL 1,456 in the working-age population. The most notable difference between consorcio participants and the working-age population is gender, with consorcio participants being 17 percentage points more likely to be male.

Our distance measures are created using open-source data from Open Street Maps (Open-

StreetMap contributors, 2017) in cooperation with the BCB. The addresses of individuals and firms are from the population and firm registries at Receita Federal. Addresses are geocoded using an open-source geocoder Photon (https://github.com/komoot/photon), and obtain coordinates for 64 percent of firms and 59 percent of individuals. We also extract the coordinates of each firm's nearest public transit stop. Using these coordinates, we compute the distance between an individual's and her employer's location and between the firm and the nearest public transportation stop. Details of the procedure are described in Appendix A.3.

We are able to compute the distance between home and employer for 49 percent of the data for formally employed workers and the distance between the nearest public transportation stop and employer for 45 percent of the data for formally employed workers. When we compare treated and untreated workers, we observe about 50 percent of commuting distances before and 48 percent after access to a motorcycle. Similarly, we observe about 45 percent of transportation distances before and 44 percent after access to motorcycle financing. In the population registry, we observe the most recent snapshot (i.e., information about an individual's residence as of 2015). This introduces measurement error in our distance measures. As a consequence, we top-code commuting distances at 20 km. About 26.5 percent of commuting distances are truncated above 20km before and about 26.8 percent after access to a motorcycle. In our empirical analysis, we show that the results are robust to top-coding commuting distance at 50 km, 75 km, and 100km. Conditional on individuals in the same group, differences in the availability of data on commuting and transportation distances, and truncation rates for commuting distances are not statistically significant. The average commuting distance before gaining access to credit is 9.58 km, and the median commuting distance is 7.08 km.

# 4 Empirical Analysis

This section presents our empirical analysis to assess the effects of access to credit for investment in individual mobility on commuting patterns, employment rates, and salaries.

## 4.1 Consorcios

Exploiting time-series variation in credit allocation in consorcios implies the following specification to assess the relationship between access to credit for investment in individual mobility and labor market outcomes:

$$Y_{it} = \alpha_i + \alpha_{gt} + \beta \cdot win_{it} + \epsilon_{it}, \tag{1}$$

where $i$ denotes individuals, $g$ denotes consorcio groups, and $t$ denotes time. $Y_{it}$ is the outcome of interest and $win_{it}$ is a dummy variable that takes the value of one for individuals who obtain credit through a lottery in year $t$ or earlier and zero otherwise. Employing a difference-in-differences methodology with individual fixed effects ($\alpha_i$) tracks changes for the same individual and controls for sample composition effects, which are particularly acute in our setting where individuals may transition from informal to formal labor markets. Group-time fixed effects ($\alpha_{gt}$) control for selection by and into a specific group.

As described in Section 2.1, groups allocate credit through a combination of auctions and lotteries. While we restrict the sample to participants who obtain credit through lotteries, the presence of auctions generates an endogeneity problem with respect to changes in the pool of lottery participants over time. Specifically, individuals who remain in the lottery pool have not obtained credit through auctions, which may reflect characteristics correlated with the labor market outcomes. For example, individuals may not bid in auctions because of limited funds as a result of adverse labor market shocks. In this case, staying in the lottery pool may be correlated with bad labor market outcomes. Alternatively, individuals may not bid in auctions because they do not feel like they urgently need access to a motorcycle as a result of positive labor market shocks. In this case, staying in the lottery pool may be correlated with good labor market outcomes. As a result, endogenous selection into the lottery pool over time could bias the estimate of $\beta$ in equation (1) upwards or downwards.

## 4.2 Instrument

To overcome this selection problem, we use an instrument. Our data on participants' ticket numbers and historical national lottery numbers enables us to simulate credit lotteries as if there were no auctions. This allows us to identify which participants would have received credit in a given month if a group allocated all credit though lotteries and there was no selection into the lottery pool over time. For each group, we translate national lottery numbers into ticket numbers based on the group's algorithm. By doing so, period by period, we obtain the schedule of credit allocation as if all credit were allocated through lotteries. For example, consider a group with 150 participants that runs for 50 months and allocates credit to three individuals each period – two based on auctions and one based on lotteries. By applying the algorithm to the national lottery number each month, we can replicate the allocation of credit as if one lottery were held each month and no auction. This provides us with a group of 50 predicted lottery winners, determined by the outcomes of the national lottery. Because of the presence of auctions in real-world groups, the instrument is not perfectly correlated with winning a lottery. For example, an individual predicted to win the lottery may have replaced a previous auction winner in a different period.

We begin our analysis by documenting that the simulated lotteries are a strong predictor of obtaining credit by estimating

$$win_{it} = \alpha_i + \alpha_{gt} + \beta \cdot win\ sim_{it} + \epsilon_{it}, \tag{2}$$

where $win\ sim_{it}$ is a dummy variable that takes the value of one from the year an individual is predicted to obtain credit based on the simulated lotteries and zero before. Since $win\ sim_{it}$ is based on the outcomes of random lotteries, it is orthogonal to $\epsilon_{it}$, conditional on comparing participants in the same group.

In Table 3, we present the results from the first-stage estimation in equation (2). Since the paper's outcome variables are available for different samples, we estimate a first stage for each sample. Across all outcome variables, the results show that being predicted a winner in simulated lotteries is associated with a 22 to 24 percentage point higher probability of being the winner of an actual lottery. Since the simulated lotteries are based on the same lottery numbers and algorithms as the actual lotteries, the instrument is strong with an F-statistic between 6,947 and 14,784.

Having established that the simulated lotteries are a strong predictor of obtaining credit, we examine the reduced form relationship between simulated lotteries and labor market outcomes by estimating

$$Y_{it} = \alpha_i + \alpha_{gt} + \sum_{s=-5, s\neq -1}^{5} \beta_s \cdot win\ sim^s_{it} + \epsilon_{it}, \tag{3}$$

where $win\ sim^s_{it}$ are dummy variables that take the value of one from $s$ years after an individual is predicted to win a lottery in the simulated lotteries. We omit the year before an individual is predicted to receive credit, which is equivalent to normalizing to zero in the year before they are predicted to receive credit. We separately pool the years from 5 to 10 years before individuals are predicted to receive credit, and the years from 5 to 10 years after individuals are predicted to receive credit into one estimate, respectively.

An active literature in econometrics examining staggered treatment has documented that short-term effects may be overweighted when the treatment effect is not constant over time for the same unit (see, e.g., Goodman-Bacon 2021 and Borusyak, Jaravel, and Spiess 2022) or across units such that treatment effects vary with the timing of treatment (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak, Jaravel, and Spiess, 2022; de Chaisemartin and D'Haultfoeuille, 2020; Goldsmith-Pinkham, Hull, and Kolesar, 2022). In our setting, this would occur if treatment effects for earlier and later lottery winners in the same group were different. The core of the problem is that with staggered treatment earlier treated units become a control group for later treated units, which may lead to an estimation bias if treatment effects vary across units over time.

To support the validity of our estimates, we apply the methodology developed in Sun and Abraham (2021), which is robust to heterogeneous treatment effects across individuals over time. We plot the results from estimating equation (3) with (in red) and without (in black) the application of the Sun and Abraham (2021) methodology in Figure 1 for commuting patterns and in Figure 2 for labor market outcomes. We find that the estimates with and without the application of the Sun and Abraham (2021) methodology are almost identical. This suggests that heterogeneous treatment effects are not a concern in our setting. In addition, the results in both figures indicate no systematic pre-treatment trends for any of the outcome variables. In contrast, we observe a sharp increase for all outcomes right after treatment.

The reduced form estimates in Figures 1 and 2 can be interpreted as the labor market effects of winning a credit lottery with the probability given in the respective first stage. To obtain estimates for the labor market effects of an individual winning a credit lottery, we estimate a two-stage least square specification

$$Y_{it} \quad = \quad \alpha_i + \alpha_{gt} + \sum_{s=-5, s\neq -1}^{5} \beta_s \cdot win_{it}^s + \epsilon_{it}, \tag{4}$$

where the variables $win_{it}^s$ are dummy variables taking the value of one from $s$ years after an individual wins a lottery, and are instrumented with the variables $win\ sim_{it}^s$ in equation (3). Since the IV estimates are based on the same variation as the reduced form estimates in Figures 1 and 2, robustness of the reduced form estimates to heterogeneity in treatment effects across units over time implies that the IV estimates are robust to the same type of heterogeneity.

The results are reported in Table 4. The results in column I show that commuting distance starts to increase by 3.56 percent from the year when individuals win a credit lottery relative to individuals who have not won a lottery. This difference increases to 10.98 percent 4 years after winning a lottery, and the long-run estimate for 5 or more years after winning shows a persistent increase of 15.25 percent. The results in column II show that transportation distance increases by 0.96 percent from the year when individuals win a credit lottery relative to individuals who have not won a lottery. This difference increases to 4.31 percent 4 years after winning a lottery, and the long-run estimate for 5 or more years after winning shows a persistent increase of 5.88 percent.

The results in column III show that formal employment increases by 1.32 percentage points (3.2 percent) from the year when individuals win a credit lottery relative to individuals who have not won a lottery. This difference increases to 5.07 percentage points (12.4 percent) 4 years after winning a lottery, and the long-run estimate for 5 or more years after winning shows a persistent increase of 6.64 percentage points (16.2 percent). The results in column IV show that salaries increase by 2.63 percent from the year when individuals win a credit lottery relative to individuals who have not won a lottery. This difference increases to 5.55 percent 4 years after winning a lottery, and the long-run estimate for 5 or more years after winning shows a persistent increase of 8.00 percent.

Altogether, these results suggest that access to credit for investment in individual mobility leads to a permanent increase in the geographic scope of available employment opportunities, which in turn leads to higher rates for formal employment and salaries.

In Table 5, we compare the OLS, reduced form, and IV estimates from equation (1). The results from the IV estimation are reported in Panel C. Since the specification is just identified, the IV estimates are equivalent to dividing the reduced form estimates by the corresponding first-stage estimates in Table 3. We find that commuting distance increases by 7.56 percent after obtaining credit (column I), transportation distance increases by 3.53 percent (column II), formal employment increases by 4.65 percentage points (11.3 percent) (column III), and salary increases by 4.32 percent (column IV).

Across all outcomes the OLS estimates are smaller than the IV estimates. Smaller OLS estimates are consistent with positive selection into the lottery pool over time. Specifically, participants that remain in the lottery pool, and therefore serve as a control group for earlier treated individuals, appear to experience positive labor market shocks leading to a lower estimate of $\beta$ with OLS. For example, individuals may remain in the pool because they do not consider access to a motorcycle sufficiently urgent to make high bids in auctions. Since the reduced form and IV estimates are not subject to endogenous changes in the lottery pool over time due to their reliance on simulated lotteries, they do not suffer from the same estimation bias.

## 4.3 Interpretation

Since we rely on an instrumental variable strategy, the estimates are local average treatment effects for individuals targeted by the instrument. Specifically, our estimates apply to consorcio participants who obtain credit for motorcycle purchase through lotteries. Consorcio participants are a selected group of individuals. Our estimates apply to this group and may differ for the general population. By revealed preferences, consorcio participants expect to benefit from access to individual mobility, which may not apply to the same extent to the average individual in the population. In addition, our counterfactual is individuals who save toward investment in a motorcycle through a consorcio. In sum, our estimates apply to individuals who believe that obtaining credit for investment in individual mobility will be beneficial for them, but are unable to invest in individual mobility, because they are credit constrained. Obtaining estimates for this group of individuals might be a virtue, since they are likely targets of policy interventions. For example, recent evidence by Beaman et al. (forthcoming) suggests that the return on capital is higher if it targets individuals who self-select into credit markets. Moreover, the high participation rate in motorcycle consorcios in Brazil suggests that the combination of seeking to invest in individual mobility and credit constraints applies to a significant share of the population.

Obtaining access to individual mobility through credit may have different implications than obtaining access to individual mobility through other means. For example, credit may provide additional labor market incentives, since individuals face adverse consequences from defaulting. Although how consorcios are organized differs markedly from standard financial intermediation by banks, from an individual's perspective, obtaining credit through a consorcio is not unlike a regular bank loan. Individuals are responsible for their repayment of the loan, and enforcement operates through physical collateral.

Finally, some participants stop making payments to the group before receiving credit, and therefore will not receive credit and a motorcycle. Since the decision to stop making payments is endogenous, we do not drop those individuals, but instead treat them as if they obtain credit when their number is drawn in the lottery. In our sample, 23 percent of participants do not receive credit when their ticket number is drawn due to missed payments. In addition, 16 percent default on their payments after they receive credit and lose access to the motorcycle. These individuals are only partially treated.[11]

## 4.4 Cross-Sectional Variation in Treatment Effects

In Table 6, we assess whether the effects of access to credit for investment in individual mobility vary with location-specific or individual-specific characteristics by augmenting the IV estimation of equation (1) by adding interactions of the treatment variable with location- and individual-specific characteristics.

**Location-Specific Characteristics** In Panel A, we assess how the effect of access to credit for investment in individual mobility varies with location-specific characteristics. One additional public transportation stop per population in a municipality is associated with a 0.51 percent smaller increase in commuting distance (column I) and a 0.35 percent smaller

[11] There may even be a negative treatment effect for those individuals who save for a motorcycle and default, since they pay a penalty for default.

increase in transportation distance (column II). Formal employment rates (column III) and salaries (column IV) increase 0.09 percentage points and 0.48 percent less per additional public transportation stop, respectively. Individuals' commuting distance increases by 1.92 percent less (column V), and transportation distance increases by 1.35 percent less (column VI) per additional firm scaled by the local population located in the municipality in which an individual lives. Formal employment rates (column VII) and salaries (column VIII) increase by 0.53 percentage points and 1.60 percent less per additional firm, respectively.

**Individual-Specific Characteristics** In Panel B, we assess how the effect of access to credit for investment in individual mobility varies with individual-specific characteristics. A 10 percent lower initial salary is associated with a 0.36 percent greater increase in commuting distance after obtaining credit (column I) and a 0.22 percent greater increase in transportation distance (column II). Moreover, a 10 percent lower initial salary is associated with a 0.81 percentage points greater increase in formal employment (column III) and a 0.19 percent greater increase in salary (column IV). In addition, we find that commuting distance increases by 0.33 percent less per year of age (column V) and transportation distance increases by 0.12 percent less per year of age (column VI). The effect of access to individual mobility on formal employment is 1.19 percentage points lower per year of age (column VII), and the effect of access to individual mobility on salaries is 0.97 percent lower per year of age (column VIII).

Together these results suggest that obtaining access to credit for investment in individual mobility has greater effects on commuting behavior, formal employment rates, and labor income for young, low-income individuals living in areas with less developed public transportation and fewer local employment opportunities.

# 5 Remaining Issues

In this section, we discuss potential remaining concerns with our empirical analysis in the paper and our interpretation of results.

**Distance Measure** We only observe a snapshot of individuals' home address at the end of 2015. To reduce noise from cases in which individuals moved during our sample period, we top-code the commuting distance measure at 20 km in our main analysis. To ensure that our choice of top-coding value does not drive our estimates, in the top plot in Figure 3 we show that reduced form estimates for commuting distance are robust to different cutoffs. Specifically, the estimates are similar for alternative cutoffs of 50 km, 75 km, and 100 km.

Overall, a higher threshold for top-coding increases the magnitude of the estimates, but also leads to more noisy estimates. This suggests that some longer commuting distances may be genuine, whereas many are not.

**Informal Labor Markets** We only observe formal employment. A significant share of the labor market in Brazil is informal. Thus, changes in formal employment may capture transitions from informal to formal employment rather than from unemployment to employment. Similarly, changes in salaries could be driven by workers substituting a job in the informal sector for a formal one or vice versa. Since changes in salaries are only captured conditional on formal employment before and after obtaining access to credit for investment in a motorcycle, transitions between the two sectors do not affect the salary estimate. However, a concern related to informal employment is that workers might substitute two smaller jobs in the informal and the formal sector with a larger job in the formal sector. In this case, we would overestimate the change in salary by missing pre-treatment information about the informal job salary.

To assess the role of informal labor markets, we first exploit heterogeneity in labor market informality across Brazil. In some municipalities, labor market informality is lower than 10 percent; in other municipalities, it exceeds 60 percent. We re-estimate our salary results for municipalities with below-median levels of labor market informality and for municipalities with labor market informality below 20 percent and report the reduced form estimates in middle plot in Figure 3. We find that salary estimates are very stable across labor markets with different levels of labor market informality. This suggests that our salary estimates for formally employed individuals are not affected by the presence of informal labor markets.

To directly address the concern about consolidating two jobs in the formal and informal sector into a larger job in the formal sector, in the bottom plot we report reduced form estimates with the number of hours worked in the formal sector as the dependent variable. We find that the number of hours worked in the formal sector does not change post-treatment. This suggests that salary estimates are not biased due to the consolidation of smaller jobs in the formal and informal sectors into a larger formal sector job.

Beyond assessing the validity of our intensive margin estimates, informal labor markets have further implications. Specifically, the salary results do not account for transitions from the informal to the formal sector and vice versa. Thus, if workers obtain a higher salary by switching their job in the informal sector or between the sectors, we underestimate the aggregate salary effects. Individuals may also be better off in a new formal sector job based on outcomes that are not reflected in salaries e.g., access to government programs which our results also do not capture.

# 6 Return to Credit for Investment in Individual Mobility

In this section, we estimate the value and return to capital for investment in individual mobility, similarly to Bandiera et al. (2017). We focus on changes in salaries. Access to credit for investment in individual mobility may provide additional benefits; for example, reducing commuting time or improving access to education (Muralidharan and Prakash, 2017). In addition, since our salary estimates relate to intensive margin effects in the formal sector, we may miss increases in salaries for individuals who switch jobs in the informal sector or go from an informal to a formal job. Thus, our estimates are likely to be an underestimate of the total value of access to credit for investment in individual mobility.

**Base Estimate** The total effect of access to credit for investment in individual mobility on expected labor income $\mathbb{E}[\pi]$ can be computed as

$$\mathbb{E}[\pi] \;=\; \sum_{s=0}^{T} \frac{\mathbb{E}[log(S_s^M) - log(S_s^B)] \cdot (1-\tau)}{(1+r)^s}, \tag{5}$$

where $T$ is the last period in which an individual is observed, $S_s^M$ and $S_s^B$ are the salaries with and without access to credit, respectively, $\tau$ is the income tax rate, and $r$ is the (real) annual rate to discount future income. The definition of $\mathbb{E}[\pi]$ in equation (5) ensures that it is normalized to zero if access to credit for investment in individual mobility has no effect on salaries.

Our estimates for from Table 4 can be interpreted as estimates for $\mathbb{E}[log(S_s^M) - log(S_s^B)]$ for consorcio participants. Our final estimate $\beta_5$ includes years beyond 5 years after obtaining credit. Since the maximum lifetime of a motorcycle is about 20 years, we limit $T$ to 20 years after gaining access to credit for investment in individual mobility. The simplest assumption is that the expected salary wedge $\mathbb{E}[log(S_s^M) - (S_s^B)]$ stays constant for $s > 5$, as in Bandiera et al. (2017). This might be conservative, given that the gradient of $\beta_s$ with respect to $s$ is positive in the observable range. There is no obvious rate to use for discounting cash flows from investment in individual mobility. The real deposit and 10-year government bond rate are 4.56 and 6.46 percent during our sample period, respectively. We choose the higher rate of 6.46 percent in our calculations, but it is straightforward to adjust the computations for alternative rates.

Based on an average income tax rate of 8 percent for motorcycle consorcio participants and our IV estimates for $\beta_s$ (see Table 4), assuming a real discount rate of $r = 0.0646$, the income effect of access to credit for investment in individual mobility equals 0.75 annual salaries.

Access to individual mobility requires an initial investment. In the simplest case, in which an individual purchases a motorcycle outright, the net benefit of individual mobility minus costs can be computed as

$$\mathbb{E}[\pi_{net}] = \mathbb{E}[\pi] - I, \tag{6}$$

where $I$ is the cost of the motorcycle as a fraction of the base salary. The average cost of a motorcycle across all of our groups divided by the average pre-credit salary of consorcio participants is 0.46. Thus, the net income effect of investment in individual mobility is 0.75-0.46=0.29 annual salaries.

An alternative way to compute the return on providing credit for investment in individual mobility, which facilitates comparison with other types of investment and does not require an assumption on the discount rate, is the internal rate of return (IRR). The IRR can be computed by setting equation (6) to zero:

$$0 = \sum_{s=0}^{20} \frac{\mathbb{E}[log(S_s^M) - log(S_s^B)](1-\tau)}{(1+IRR)^s} - I. \tag{7}$$

Solving equation (7), we obtain an IRR of 11.71 percent. Thus, access to credit for investment in individual mobility generates a real annual return of 11.71 percent over 20 years.[12]

**Financing** The costs of investment in individual mobility depend on the financing. For example, in the case of consorcios, most participants do not obtain credit for investment in a motorcycle at time $s = 0$ but at equal rates over $M$ months, which also include fees, akin to an interest rate $f$. Thus, the cost part in equation (6) changes to $\sum_{s=0}^{M} I/M \cdot (1+f)$ in consorcios.

For a rate of $r = 0.0646$ and $M = 48$, financing a motorcycle through a consorcio with an average fee of $f = 0.16$ rather than outright purchase adds 0.035 annual wages to the costs of investment in a motorcycle.

[12] A potential cost of riding a motorcycle could be that it exposes individuals to higher risk of injury and death. What we see in the data is that death rates from traffic accidents in fact decrease after individuals win credit to buy a new motorcycle. This suggests that relative to alternative modes of transportation (e.g., walking, using a bicycle, or riding an older motorcycle), a new motorcycle may be a safer option.

# 7 Conclusion

By exploiting randomized time-series variation in access to credit for investment in motorcycles through a group-lending mechanism in Brazil (consorcio), we document that access to credit for investment in individual mobility yields high and persistent returns. Consistent with a geographically broader job search, individuals find jobs farther from home and public transportation. The effects are stronger in areas with less developed public transport and scarce local labor markets, and for younger and lower-income individuals.

Among interventions to boost economic development for low-income households, much hope has been placed in the transformative power of financial access. Yet studies typically document that the returns to capital are meager and access to finance rarely has transformative effects. Our findings suggest that extending credit for investment in mobility can generate large returns. It is often assumed that access to labor markets does not require a large upfront investment (e.g., Banerjee and Newman 1993). Our results suggest that overcoming spatial constraints in labor market access may require a large upfront investment and therefore access to capital. This insight is supported by recent evidence in Banerjee, Duflo, and Sharma (2021) that positive long-term effects on labor income from a cash grant program in West Bengal are linked to migration to urban centers. Our results also resonate with theories of spatial mismatch (Kain 1968) and suggest that policies that increase individuals' mobility (Fan 2012) may have important implications for labor market access.

Recent evidence by Beaman et al. (forthcoming) suggests that return on capital is higher if it targets individuals who self-select into credit markets. This suggests that mechanisms that can target this population generate higher returns on capital. This poses a practical challenge for policymakers and may be an important aspect to consider in designing mechanisms and policies. A potential upside of market-based solutions is that to be sustainable, they endogenously require targeting populations that generate high returns, as in the case of consorcios in Brazil. Identifying populations that generate high returns on capital and designing policies and mechanisms to target them is a promising avenue for future research. For example, Hussam, Rigol, and Roth (2022) show that eliciting community information can help identify high-ability entrepreneurs who generate high returns on investment.

Our findings have broader policy implications. For example, our results have implications for urban and infrastructure planning to mitigate spatial mismatch between workers and firms. Our results also suggest that providing access to mobility for job seekers significantly improves their labor market prospects; for example, in the context of welfare-to-work programs. Similarly, in addition to facilitating access to credit for investment in individual mobility, it may be beneficial to allow financially distressed individuals to maintain access to individual mobility; for example, through asset exemption rules in bankruptcy proceedings.

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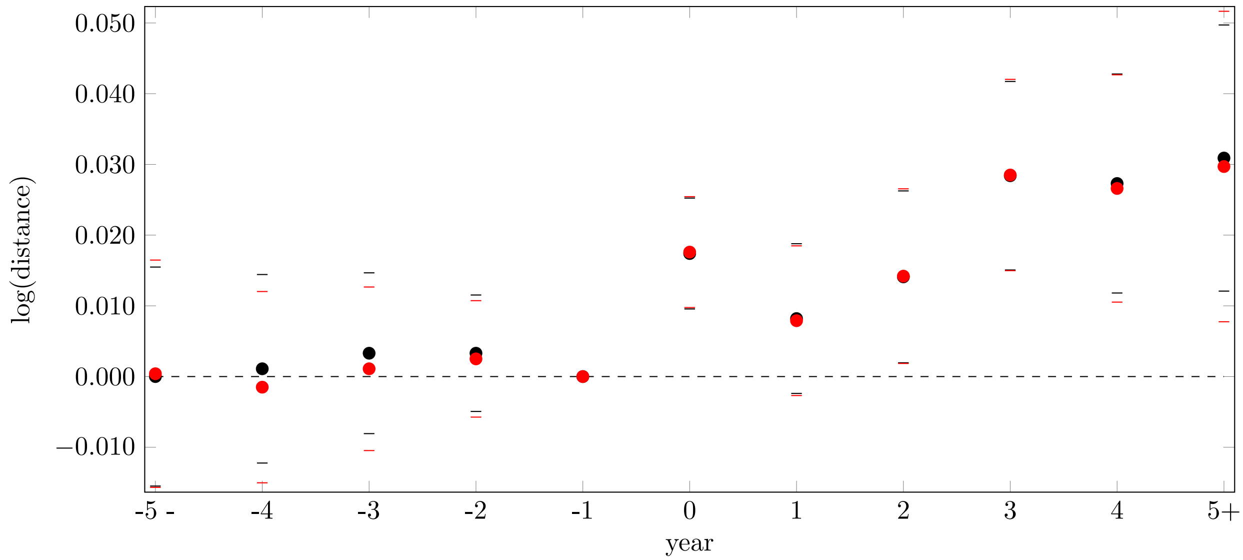
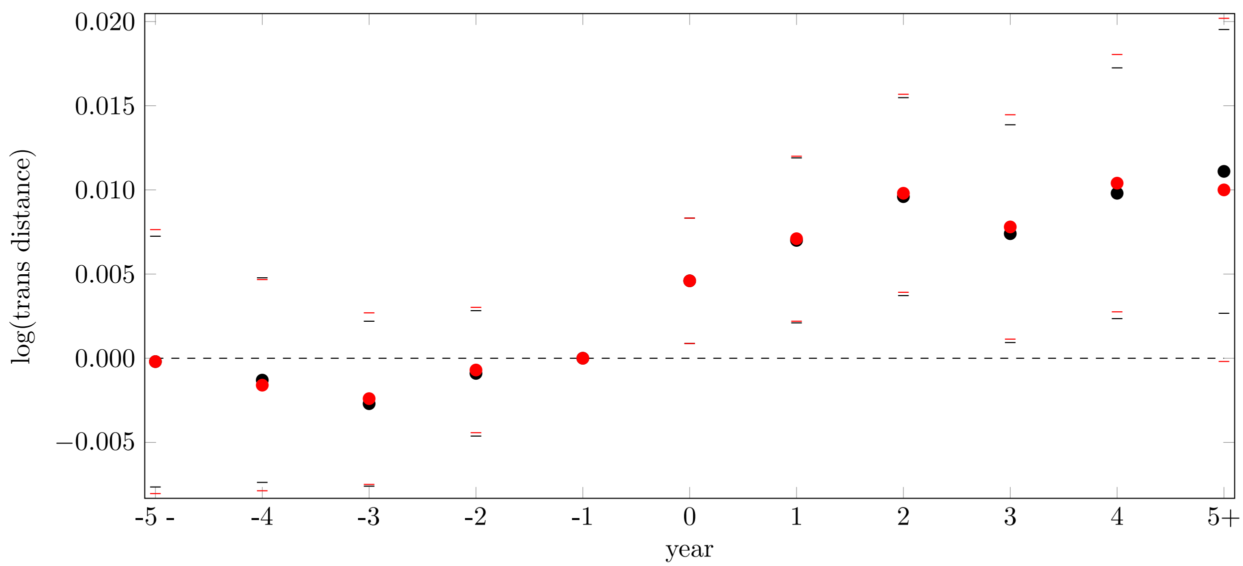
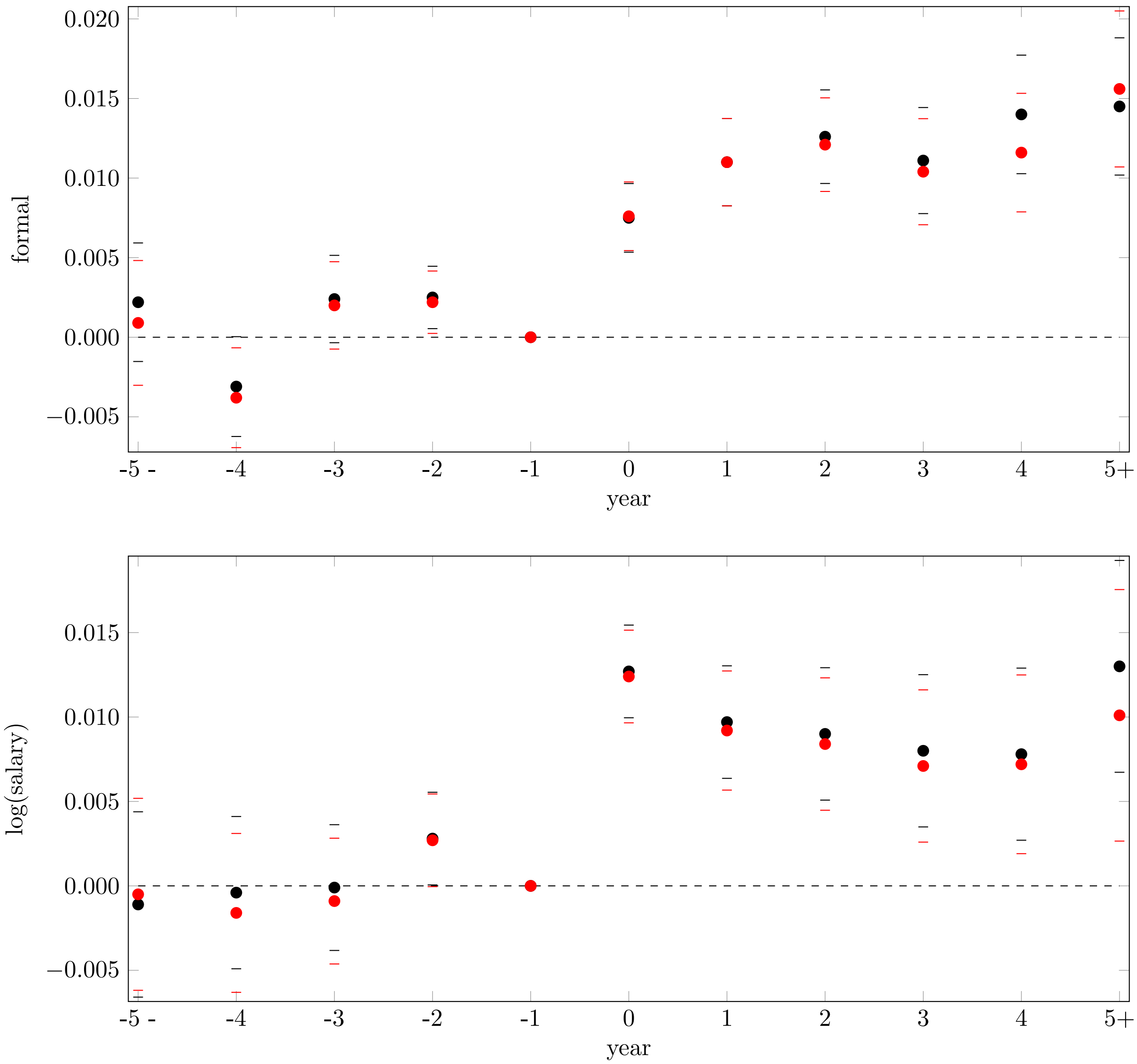
Figure 1: **Commuting Patterns**



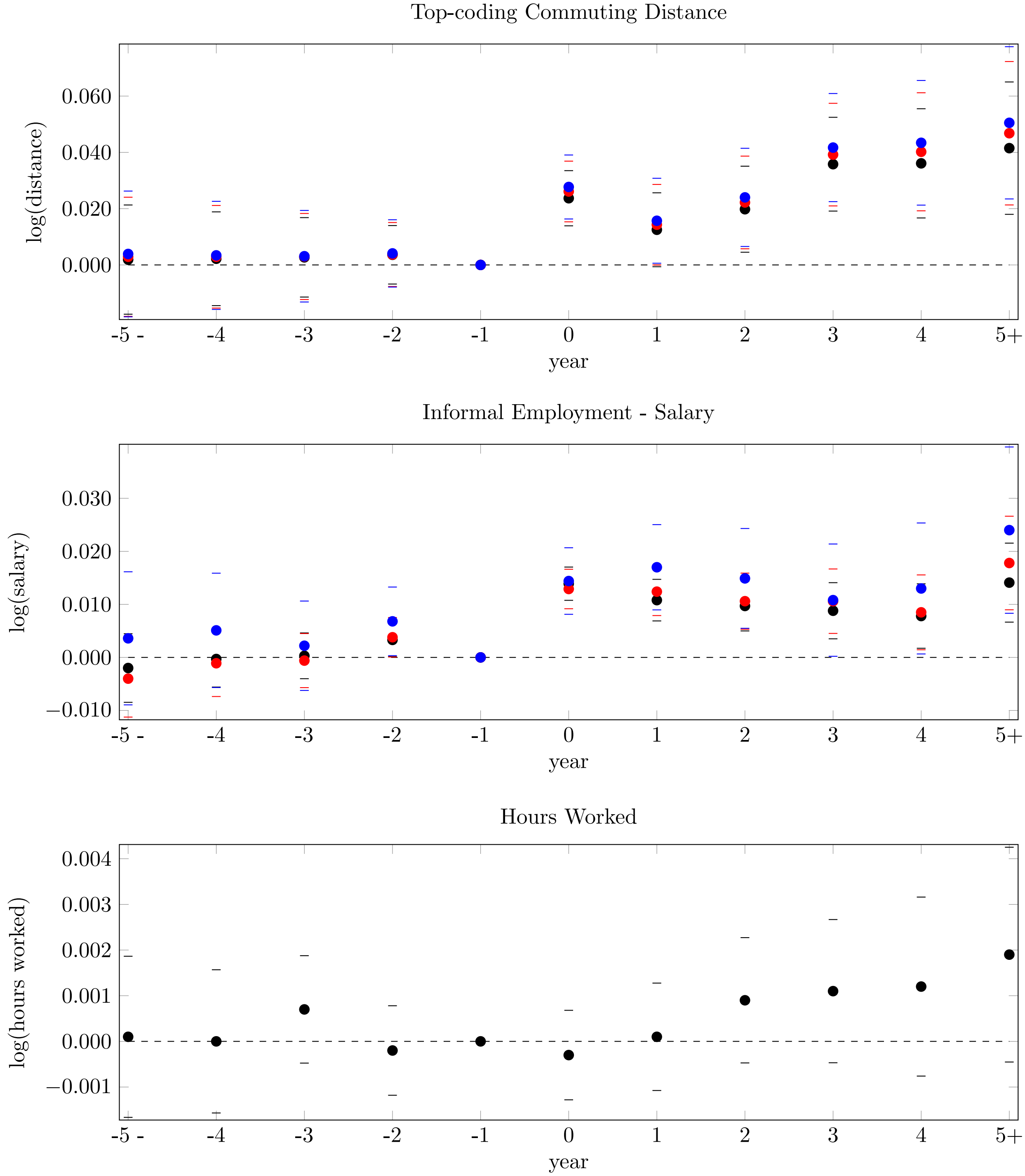

This figure depicts the results from the reduced form estimation in equation (3) with 95 percent confidence bounds for the log distance between an individual's home and workplace in the top panel and the log distance between an individual's workplace and the nearest public transportation stop in the bottom panel. Estimates based on the methodology in Sun and Abraham (2021) are depicted in red.

Figure 2: **Labor Market Outcomes**




This figure depicts the results from the reduced form estimation in equation (3) with 95 percent confidence bounds for formal employment rates in the top panel and the log of salary in the bottom panel. Estimates based on the methodology in Sun and Abraham (2021) are depicted in red.

Figure 3: **Robustness Tests**




This figure depicts results from the reduced form estimation in equation (3) with 95 percent confidence bounds. In the top panel, the dependent variable is the log distance between an individual's home and workplace for different levels of topcoding (50km (black), 75km (red), 100km (blue)). In the middle panel, the dependent variable is the log salary for municipalities with below equally- (black) and value-weighted (red) median levels of labor market informality, and municipalities with labor market informality below 20 percent (blue). In the bottom panel, the dependent variable is the log of hours worked in the formal sector.

Table 1: **Spatial Mobility and Employment Opportunities**

| Commuting Distance | 1 km | 3 km | 5 km | 10 km | 20 km | 50 km | 100 km |
|---|---|---|---|---|---|---|---|
| Number of Firms | 234 | 1,488 | 2,934 | 5,762 | 9,227 | 14,809 | 27,214 |
| Number of Jobs | 2,785 | 23,387 | 53,217 | 120,041 | 179,169 | 306,248 | 554,899 |
| Number of Occupations | 33 | 138 | 227 | 347 | 439 | 554 | 744 |

This table reports the median numbers of formal firms, jobs, and distinct occupations in 2015 at different commuting distances for a random sample of 10,000 individuals and 1,000,000 firms in our sample.

Table 2: **Descriptive Statistics**

| Panel A: Consorcios | Mean | Median | Std. |
|---|---|---|---|
| Groups | 8,777 | | |
| Members per group | 56.24 | 43.00 | 36.03 |
| Duration (months) | 43.13 | 36.00 | 16.99 |
| Commuting Distance | 9.92 | 7.64 | 7.59 |
| **Panel B: Individual Characteristics (means)** | **Working-Age** | **Formally Employed** | **Consorcios** |
| Formal Employment Share | 0.37 | 1.00 | 0.41 |
| Salary | 1,456 | 1,427 | 1,160 |
| Age | 38.68 | 34.68 | 32.28 |
| Male | 0.52 | 0.59 | 0.69 |
| University Education | 0.15 | 0.14 | 0.18 |
| Agriculture & Fishing | 0.02 | 0.02 | 0.02 |
| Construction | 0.07 | 0.07 | 0.08 |
| Government | 0.16 | 0.16 | 0.21 |
| Health & Education | 0.08 | 0.08 | 0.05 |
| Hotel & Transport | 0.09 | 0.09 | 0.10 |
| Manufacturing | 0.17 | 0.17 | 0.15 |
| Real Estate & Finance | 0.02 | 0.02 | 0.02 |
| Repairs | 0.19 | 0.20 | 0.27 |
| Nr. Observations | 2,006,203,455 | 786,979,214 | 4,272,902 |

This table provides descriptive statistics. Panel A provides descriptive statistics on the number of consorcios, the number of members per group, the duration of groups, and the average pre-treatment commuting distance of formally employed individuals. Panel B provides descriptive statistics for all working-age individuals, all formally employed workers, and consorcio participants before they obtain credit.

Table 3: **First Stage: Simulated Lotteries**

| Dep. Var.: $win_{it}$ | I | II | III | IV |
|---|---|---|---|---|
| Sample | Commuting Distance | Transportation Distance | Formal Employment | Salary |
| $win\ sim_{it}$ | 0.2367 | 0.2379 | 0.2201 | 0.2328 |
| | [0.0025] | [0.0029] | [0.0018] | [0.0021] |
| Group-Year FE | yes | yes | yes | yes |
| Individual FE | yes | yes | yes | yes |
| Clustered SE | group | group | group | group |
| Observations | 1,402,027 | 1,259,262 | 6,552,182 | 2,859,164 |
| $R^2$ | 0.861 | 0.870 | 0.848 | 0.852 |
| K-P F-Stat | 8,707 | 6,947 | 14,784 | 12,526 |
| Individuals | 176,148 | 194,774 | 493,632 | 330,528 |

This table reports results from the first stage estimation in equation (2) for different samples based on the availability of the outcome measures: log distance between individual $i$'s home and workplace in column I, log distance between individual $i$'s workplace and the nearest public transportation stop in column II, a dummy variable that takes the value of one if individual $i$ is formally employed and zero otherwise in column III, and the log of individual $i$'s salary in column IV. The variable $win_{it}$ takes the value of one for individuals who obtain credit in year $t$ or earlier and zero otherwise. The variable $win\ sim_{it}$ takes the value of one for individuals who are predicted to obtain credit in year $t$ or earlier and zero otherwise. Standard errors are reported in parentheses. The bottom of the table provides information on fixed effects, the clustering of standard errors, and the number of individuals for each outcome variable.

Table 4: **IV Estimates**

| | I | II | III | IV |
|---|---|---|---|---|
| Dep. Var.: | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ |
| $win_{it}^{-5}$ | -0.0133 | 0.0009 | 0.0061 | -0.0107 |
| | [0.0332] | [0.0164] | [0.0073] | [0.0118] |
| $win_{it}^{-4}$ | -0.0089 | -0.0006 | -0.0032 | -0.008 |
| | [0.0241] | [0.0118] | [0.0054] | [0.0084] |
| $win_{it}^{-3}$ | -0.0026 | -0.0039 | 0.004 | -0.0061 |
| | [0.0173] | [0.0083] | [0.004] | [0.006] |
| $win_{it}^{-2}$ | 0.0018 | -0.0016 | 0.0041 | 0.0015 |
| | [0.0098] | [0.0047] | [0.0023] | [0.0034] |
| $win_{it}^{0}$ | 0.0356 | 0.0096 | 0.0132 | 0.0263 |
| | [0.0099] | [0.0047] | [0.0025] | [0.0035] |
| $win_{it}^{1}$ | 0.0322 | 0.0181 | 0.0237 | 0.0312 |
| | [0.0175] | [0.0083] | [0.0041] | [0.0061] |
| $win_{it}^{2}$ | 0.0527 | 0.0289 | 0.0336 | 0.0399 |
| | [0.0250] | [0.0119] | [0.0059] | [0.0089] |
| $win_{it}^{3}$ | 0.0913 | 0.0326 | 0.0389 | 0.0473 |
| | [0.0326] | [0.0156] | [0.0076] | [0.0117] |
| $win_{it}^{4}$ | 0.1098 | 0.0431 | 0.0507 | 0.0555 |
| | [0.0404] | [0.0195] | [0.0092] | [0.0146] |
| $win_{it}^{5}$ | 0.1525 | 0.0588 | 0.0664 | 0.0800 |
| | [0.0548] | [0.0262] | [0.0123] | [0.0196] |
| Group-Year FE | yes | yes | yes | yes |
| Individual FE | yes | yes | yes | yes |
| Clustered SE | group | group | group | group |
| Observations | 1,402,027 | 1,259,262 | 6,552,182 | 2,859,164 |
| K-P F-Stat | 1,211 | 923 | 4,328 | 2,329 |

This table reports results from the IV estimation in equation (4). The dependent variable is the log distance between individual $i$'s home and workplace in column I, the log distance between individual $i$'s workplace and the nearest public transportation stop in column II, a dummy variable that takes the value of one if individual $i$ is formally employed and zero otherwise in column III, and the log of individual $i$'s salary in column IV. The variables $win_{it}^{s}$ indicate the leads and lags relative to the year in which an individual obtained credit and is instrumented by the respective leads and lags for the simulated lotteries given in equation (3). Standard errors are reported in parentheses. The bottom of the table provides information on fixed effects and the clustering of standard errors.

Table 5: **Difference-in-Differences Estimates**

| | I | II | III | IV |
|---|---|---|---|---|
| Dep. Var.: | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ |
| Panel A: OLS | | | | |
| $win_{it}$ | 0.0154 | 0.0070 | 0.0172 | 0.0159 |
| | [0.0029] | [0.0013] | [0.0009] | [0.0010] |
| $R^2$ | 0.821 | 0.770 | 0.603 | 0.846 |
| Panel B: Reduced Form | | | | |
| $win\ sim_{it}$ | 0.0179 | 0.0084 | 0.0102 | 0.0100 |
| | [0.0050] | [0.0023] | [0.0013] | [0.0018] |
| $R^2$ | 0.821 | 0.770 | 0.603 | 0.846 |
| Panel C: IV | | | | |
| $win_{it}$ | 0.0756 | 0.0353 | 0.0465 | 0.0432 |
| | [0.0213] | [0.0095] | [0.0061] | [0.0077] |
| K-P F-Stat | 8,707 | 6,947 | 14,784 | 12,523 |
| Group-Year FE | yes | yes | yes | yes |
| Individual FE | yes | yes | yes | yes |
| Clustered SE | group | group | group | group |
| Observations | 1,402,027 | 1,259,262 | 6,552,182 | 2,859,164 |
| Individuals | 176,148 | 194,774 | 493,632 | 330,528 |

This table reports results from estimating equation (1) with OLS in Panel A, the reduced form in Panel B, and IV in Panel C. The dependent variable is the log distance between individual $i$'s home and workplace in column I, the log distance between individual $i$'s workplace and the nearest public transportation stop in column II, a dummy variable that takes the value of one if individual $i$ is formally employed and zero otherwise in column III, and the log of individual $i$'s salary in column IV. The variable $win_{it}$ takes the value of one for individuals who obtain credit in year $t$ or earlier and zero otherwise. The variable $win\ sim_{it}$ takes the value of one for individuals who are predicted to obtain credit in year $t$ or earlier and zero otherwise. In Panel C, $win_{it}$ is instrumented by $win\ sim_{it}$. The corresponding first-stage regressions are reported in Table 3. Standard errors are reported in parentheses. The bottom of the table provides information on fixed effects, the clustering of standard errors, and the number of individuals for each outcome variable.

Table 6: **Location- and Individual-Specific Characteristics**

| | I | II | III | IV | V | VI | VII | VIII |
|---|---|---|---|---|---|---|---|---|
| | Panel A: Location-Specific Characteristics | | | | | | | |
| | Public Transport | | | | Employment Opportunities | | | |
| Dep. Var.: | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ |
| $win_{it}$ | 0.0806 | 0.0483 | 0.0465 | 0.0481 | 0.1945 | 0.1138 | 0.0763 | 0.1362 |
| | [0.0222] | [0.0101] | [0.0072] | [0.0086] | [0.0361] | [0.0158] | [0.0087] | [0.0126] |
| $win_{it} * nb\ trans\ stops_m$ | -0.0051 | -0.0035 | -0.0009 | -0.0048 | | | | |
| | [0.0011] | [0.0005] | [0.0004] | [0.0006] | | | | |
| $win_{it} * nb\ firms_m$ | | | | | -0.0191 | -0.0134 | -0.0053 | -0.0160 |
| | | | | | [0.0043] | [0.0023] | [0.0012] | [0.0018] |
| Observations | 1,323,785 | 1,132,042 | 5,124,678 | 2,340,829 | 1,401,397 | 1,259,262 | 6,552,182 | 2,859,164 |
| K-P F-Stat | 4,164 | 3,327 | 7,033 | 5,687 | 4,358 | 3,495 | 7,440 | 6,276 |
| | Panel B: Individual-Specific Characteristics | | | | | | | |
| | Salary | | | | Age | | | |
| Dep. Var.: | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ | $log(distance)_{it}$ | $log(trans\ distance)_{it}$ | $formal_{it}$ | $log(salary)_{it}$ |
| $win_{it}$ | 0.3074 | 0.1769 | 0.5783 | 0.1639 | 0.1698 | 0.0679 | 0.3974 | 0.3232 |
| | [0.0539] | [0.0269] | [0.0202] | [0.0204] | [0.0276] | [0.0122] | [0.00067] | [0.0089] |
| $win_{it} * log(salary)_i$ | -0.0355 | -0.0221 | -0.0809 | -0.0187 | | | | |
| | [0.0072] | [0.0038] | [0.0028] | [0.0031] | | | | |
| $win_{it} * age_i$ | | | | | -0.0033 | -0.0012 | -0.0119 | -0.0097 |
| | | | | | [0.0005] | [0.0002] | [0.0001] | [0.0002] |
| Observations | 1,402,027 | 1,259,262 | 4,619,680 | 2,859,164 | 1,402,027 | 1,259,262 | 6,552,182 | 2,859,164 |
| K-P F-Stat | 4,349 | 3,447 | 7,164 | 6,351 | 4,350 | 3,466 | 7,414 | 6,259 |
| Group-Year FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Individual FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Clustered SE | group | group | group | group | group | group | group | group |

This table reports the results from IV estimation of equation (1), additionally including interactions with a measure of public transportation $nb\ trans\ stops_m$ defined as the number of public transportation stops per population in Panel A, columns I to IV, a measure of local employment opportunities $nb\ firms_m$ defined as the number of firms per population in Panel A, columns V to VIII, the log of the pre-treatment salary in Panel B, columns I to IV, and age in Panel B, columns V to VIII. The dependent variable is the log distance between individual $i$'s home and workplace in column I, the log distance between individual $i$'s workplace and the nearest public transportation stop in column II, a variable that takes the value of one if individual $i$ is formally employed and zero otherwise in column III, and the log of individual $i$'s salary in column IV. The variable $win_{it}$ takes the value of one for individuals who obtain credit in year $t$ or earlier and zero otherwise. $win_{it}$ and its interactions with the cross-sectional measures are instrumented with simulated lotteries $win\ sim_{it}$ and its interactions with the cross-sectional measures. Standard errors are reported in parentheses. The bottom of the table provides information on fixed effects and the clustering of standard errors.

# Appendix A. Credit Allocation in Consorcios

In this section, we provide an example of an algorithm to illustrate the credit allocation procedure in consorcios and the implementation of our instrument variable (IV) strategy.

## Appendix A.1. Algorithm: Example

Each week, five five-digit numbers are drawn in Brazil's national lottery. While there are a large number of different algorithms used by different administrators, they all share the feature that each participant has the same unconditional probability of winning the lottery in every allocation period.

The algorithm we use for the example in this section uses the first of the five-digit numbers from the national lottery to determine the allocation of credit. The number is divided by the number of participants in the group and then the remainder is multiplied by the number of participants. For example, if the number from the national lottery is 10,084 and there are 250 participants in the group, the remainder from dividing 10,084 by 250 is 0.336, which multiplied by 250 is 84. Thus, credit would be allocated to the participant with ticket number 84.[13]

If the individual with ticket number 84 has already been awarded credit in a previous round, the algorithm simply adds one to the initial result. In our example, this means that credit would be allocated to the holder of ticket number 85. If this participant has also been awarded credit before, the algorithm subtracts one from the initial result, which in our case would imply that ticket number 83 is awarded credit. The algorithm continues to add and subtract two, then three, and so on, relative to the initial result, until a ticket number is selected that has not been awarded credit before.

## Appendix A.2. Simulated Allocation

The majority of consorcios combine credit allocation through lotteries and auctions. The allocation of credit through auctions is a threat to our empirical analysis because, unlike lotteries, the outcome of auctions is not random and is potentially endogenous with respect to labor market outcomes. For example, individuals with better labor market opportunities are more likely to submit higher bids and therefore obtain credit for motorcycle purchase earlier. This source of endogeneity is not eliminated by limiting attention to lottery winners. Over time, individuals who obtain credit through auctions disappear from the pool of potential lottery winners. This could lead to a bias in estimating the effect of obtaining credit for

[13] If the remainder is zero, credit goes to the highest ticket number.

motorcycle purchase on labor market outcomes.

As a consequence, we resort to an instrumental variable strategy that simulates the allocation of credit in each consorcio *as if* all credit is allocated through lotteries. To do so, we combine data on the outcome of the national lottery with data on the ticket numbers of all consorcio participants and the algorithm used by a given group. This procedure allows us to simulate the allocation of credit within groups, as if only lotteries but no auctions were held. We restrict our analysis to groups for which we have information on the algorithm they use.

Next, we illustrate this procedure using a fictional example. Suppose that a group has 200 members and allocates credit to two members every period, one through a lottery and one through an auction. Suppose that in the first period the lottery winner is ticket number 25 and the auction winner is ticket number 60. In the next period, the lottery is won by ticket number 30 and the auction is won by ticket number 80. In the third period, the algorithm determines ticket number 60 as the winner of the lottery. However, since ticket number 60 obtained credit through the auction in the first period, the ultimate lottery winner in the real group is ticket number 61. Hence, the presence of auctions has altered the order in which credit is allocated, compared with an allocation based purely on lotteries. Instead, in the simulated group, the lottery winner would be ticket number 60, since the outcomes of auctions are ignored.

Thus, for the first three periods our instrument from the simulated lotteries would predict lottery winners to be ticket numbers 25, 30, and 60, since these are the numbers that would have won the lottery if the group did not hold auctions. We simulate all lotteries for each group from the first to the last period and predict lottery winners through this procedure, which avoids distortions in the timing of lottery wins due to the presence of auctions.

## Appendix A.3. Geocoding of Individual and Firm Locations

In this section, we describe the geocoding procedure to compute commuting distances and distances between an individual's employer and the nearest public transportation stop. Geocoding is conducted in cooperation with the Central Bank of Brazil (BCB), and the resulting dataset is ultimately provided through the BCB. Address information comes from the firm and population registries housed by the BCB, and provided by Receita Federal.

The process starts by extracting each individual's and firm's street name, number, municipality, and state, which is then combined into a single string. Only alphanumeric symbols are used. Addresses are then standardized, e.g., Ave. to Avenida or R. to Rua to match the OpenStreetMap categorization. Next, the map of Brazil comes from OpenStreetMap, which is available at https://download.geofabrik.de /south-america/brazil.html (timestamp: 2020-

02-19). With the map data, addresses are geocoded using R and an open-source geocoder Photon (https://github.com/komoot/photon). The final location data covers about 64 percent of firms and 59 percent of individuals. There is no location information for those firms and individuals where the geocoder cannot find a reasonable match due to either poor quality of the address information in the registries or lack of data in OpenStreetMap.

All public transportation stops and their location from OpenStreetMap is extracted using osmextract package in R (https://cran.r-project.org/web/packages/osmextract /vignettes/osmextract.html). Public transportation stop is defined by the tags of bus stop, public transport, bus, railway, trolleybus, ferry, bus route, and bus bay. The nearest public transportation stop for each firm is obtained by combining the geocoded location of firms and public transportation stops datasets.