The impact of public transport expansions on informality: the case of the São Paulo Metropolitan Region*

Ana Isabel Moreno-Monroy†
Frederico Roman Ramos‡

February 27, 2015

Preliminary version

Abstract

The São Paulo Metropolitan Region (SPMR) displays a strong core-periphery divide. Central areas concentrate the bulk of formal jobs while peripheral areas display high incidence of informal employment. This pattern is reinforced by a large deficit in urban transport provision. Against this background, we estimate the impact of expansions of the public transport system on local informality rates for the SPMR between 2000 and 2010. We compare the average changes in informality in areas which received new public transport infrastructure with the average changes in areas which were supposed to receive infrastructure according to official plans, but did not because of delays. After controlling for endogenous selection, we find that informality decreased on average 16 percent faster in areas receiving new public transport infrastructure compared to areas that faced project delays.

JEL codes: F12; O14; O17; R12

*We would like to thank Ciro Biderman and the participants of seminars at CEPESP for useful suggestions. Moreno-Monroy gratefully acknowledges financial support from FAPESP and a Marie Curie Intra European Fellowship within the 7th European Community Framework Programme (PIEF-GA-2013-627114). Ramos gratefully acknowledges funding from CNPq, postdoctoral fellowship 150599/2014-5.

†Economics Department, Universitat Rovira i Virgili and Center for the Study of the Politics and Economics of the Public Sector, Fundação Getulio Vargas. Postal address: Avenida de la Universidad 1, 43204, Reus, Spain. Tel: 977 75 98 11. Fax: 977 30 06 61. Email: anaisabel.moreno@urv.cat

‡Center for the Study of the Politics and Economics of the Public Sector, Fundação Getulio Vargas. Postal address: Rua Itapeva 294, São Paulo, Brazil. Tel: 11-3799-3228. Fax: 11-3799-3244 Email: fred.r.ramos@gmail.com

1 Introduction

Cities in developing and emerging economies display high levels of socio-economic segregation. Central areas with good accessibility concentrate the bulk of formal jobs, that is, jobs that are fairly remunerated, stable, secure, legally recognized and protected. Lower-income peripheral areas, on the other hand, display limited accessibility and high incidence of informal employment. This division is reinforced by a suboptimal and skewed provision of urban public transport. Because of acute public transport deficits and the historical prioritization of individual over collective modes, a large segment of the lower-income population has to bear not only longer commuting distances, but also longer commuting times for the same distance traveled (Biderman, 2008). As a result, workers may opt for carrying out productive informal activities within or near home. Informality has negative consequences not only in terms of productivity and incomes, but also in terms of the budget burden in countries where a large proportion of the population does not contribute to the social protection system. Against this background, transport policies, and more specifically, the expansion of public transport networks, be seen as an alternative for reducing informality rates. To date, however, there are no estimates of the effect of improved accesibility on informality.

In this paper we estimate the impact expansions of public transport on informality for the case of the São Paulo Metropolitan Region (SPMR). With a population of 20 million inhabitants in 2010, the SPMR is the Brazilian economic powerhouse contributing with approximately 20 percent to the national GDP and concentrating 10 percent of the Brazilian population. Despite an expansion of formal employment leading to a sharp decrease of nearly 9 percentage points in the informality rates between 2000 and 2010, the SPMR still displays a particularly marked core-periphery split and a rather monocentric structure (Ramos, 2014). The region has also faced serious mobility issues, partly related to unforeseen delays in public transport projects. One salient example is the metro Line 4, conceived in the 1940s, included in the 1968 network plan, but still under construction in 2014. We investigate whether public transport expansions undertaken between 2000 and 2010 led to reductions in informality rates in areas with improved network access relative to areas which faced project delays.

Theoretical predictions on the expected effect of public transport expansions on local informality rates are scarce and lead to ambiguous predictions. On the one hand, in a model where workers are either informal and save in commuting costs by undertaking some of their productive activities at home, or formal and commute every day to the city center, public transport expansions can result in lower spatial compensation costs for formal firms, and ultimately higher (local) formal job creation (Moreno-Monroy and Posada, 2014). On the other hand, in a model where high-income and low-income workers choose to either pay higher costs but commute faster by car, or spend more time but spend less by commuting by public transport, public transport expansions can result in concentrations of lower-income workers around public transport access points (LeRoy and Sonstelier, 1983). As with the case of infrastructure at large, the direction and magnitude of the impact remains an empirical question

(Gibbons et al, 2012).

Estimating the impact of urban transport expansions is methodologically challenging because in an urban system, residential and job choices are made simultaneously, and are determined by multiple variables, one of which is access to public infrastructure. Furthermore, transport provision is not determined randomly, but it is based on observable and unobservable attributes of the areas which are likely to be correlated with local informality rates. One strand of literature proposes addressing these issues with the use of instrumental variables. An instrument that determines public transport expansions, but remains exogenous to labor quality, can provide a source of quasi-random variation through which the impacts can be estimated net of endogenous selection. As noted by Redding and Turner (2014), most of the existing works estimating the effect of changes in highway networks and railroads on the distribution of economic activity have built such instruments based on (a combination of) past planning infrastructure maps (Baum-Snow, 2007; Duranton and Turner, 2012; Michaels et al, 2012), historical route maps (Garcia-Lopez et al, 2013; Baum-Snow et al, 2012; Duranton and Turner, 2012; Hsu and Zhang, 2012; Martincus et al, 2012) and inconsequential placement of infrastructure (i.e., identifying places that received infrastructure because of reasons other than explicit planning based on their characteristics) (Chandra and Thomson, 2000; Faber, 2013).

Another strand of literature suggests the use of difference-in-difference methods to tackle the endogeneity of urban transport infrastructure allocation. The idea is to find "control areas" which would have experienced similar change in outcomes as areas receiving transport infrastructure had they not received it. Related works have used this strategy to estimate the effect of subway networks and rail lines on real state prices (Ahfeldt et al, 2012; Billings, 2011; Gibbons and Machin, 2005) and poverty (Glaeser et al, 2008).

In this paper, we combine a difference-in-difference method with an instrumental variable strategy. We use a historical network plan for the SPMR as an instrument in order to identify the impact of public transport expansions on a measure of local informality rates between 2000 and 2010. The validity of our strategy relies on the correction for possible endogenous selection, as well as the choice of a "control group" against which to compare our "treatment group", i.e., the areas close to bus corridors and metro and railway stations opened between 2000 and 2010. In order to attribute the estimated impact to public transport expansions, we need to ensure that the chosen areas were in principle suitable for new transport infrastructure, and that they are similar in terms of relevant characteristics. We include the pre-treatment values of relevant socio-economic variables as controls, and carefully construct our sample to include all areas that were pre-selected for transport project interventions within the same time-frame. One advantage of considering areas for which infrastructure plans were laid out but not implemented is that these areas are similar precisely with respect to relevant characteristics for the allocation of transport infrastructure. An additional advantage is that we can interpret the impacts as the "penalty" or cost of transport infrastructure project delays. We find this cost to be significant: in areas close to transport expansions, the average informality rate decreased 16 percent faster than areas that should have received infrastructure but did not because of delays.

Our empirical application is connected to a large body of literature analyzing the reasons behind the existence and persistence of an urban informal sector in developing and emerging economies (Camacho et al, 2013; Ferreira and Robalino, 2010). The existence and persistence of an informal sector has been attributed mostly to institutional factors (Ferreira and Robalino, 2010; Perry et al, 2007), while the role of accessibility has not yet been considered. There is an extensive literature on the impact of transport infrastructure on different outcomes such as property values (Baum-Snow and Kahn, 2000), sprawl (Burchfield et al, 2006), economic development (Heres et al, 2014) and poverty (Glaeser et al, 2008), but no works analyzing the impact of transport infrastructure on informality rates or the quality of labor at large. For the particular case of improvements in public transport, the Spatial Mismatch Hypothesis (SMH) empirical literature offers some evidence in support of a positive and significant effect of public transport improvements on labor market outcomes.[1] Kawabata (2003) finds an increase in the likelihood of working and the number of hours worked for individuals who do not have a car as a result of a better job-access by public transport. Holzer et al (2003) based on data on hiring before and after the expansion of the railway system in San Francisco, find that hiring of Latinos increased near the new station. Recent works using experimental designs find that transport subsidies can help reducing youth employment (Franklin, 2014). An important difference of our study with respect to the SMH literature is that informality is not a fixed attribute, such as race or gender, but a status resulting from either choice or necessity (Perry et al, 2007).

Our empirical approach offers an alternative for overcoming the well-known methodological challenges faced by empirical tests of the SMH related to endogenous selection (Ihlanfeldt and Sjoquist, 1998). We consider our methodology to be an attractive alternative to Propensity Score Matching (PSM). Several papers have used PSM to estimate the impact of infrastructure on different outcomes (for a review see Boarnet, 2007 and Baum-Snow and Ferreira, 2014). The basic idea is to retrieve the causal effect of infrastructure changes by accounting for the co-variates that predict receiving the treatment. On our case, we would have to correctly specify the transport infrastructure assignment model based on the characteristics of local areas that influence their likelihood of being chosen to receive infrastructure (i.e., their "program participation" probability). The problem is that the criteria used by planners for assigning new infrastructure at a certain moment in time is not known. This implies that the empirical specification of the determinants of transport infrastructure changes would be ad-hoc and possibly driven by data availability. Under these circumstances, it is likely that it would suffer from omitted variables and mis-specification problems, invalidating the estimated causal effects. Our approach only requires one variable (the instrument) to significantly explain changes in transport

[1] According to the SMH, the adverse labor outcomes of minorities are the result of the spatial disconnection between low-skilled jobs and the places where minorities reside. This hypothesis was inspired in the case of metropolitan areas of the US which, due to innovations in transportation, experienced increased residential suburbanization in the second half of the 20th century together with the dispersion of firms away from central areas within cities. Minorities allegedly relocated at a slower pace than jobs because they faced discrimination in the housing market or were subject to zoning regulations, leading to a concentration of minorities in inner-city areas where low-skilled job creation was slow (Ihlanfeldt and Sjoquist, 1998)

infrastructure, and at the same time exploits the fact that the planner's criteria is revealed also for the case of areas that are pre-selected but that eventually do not receive infrastructure projects.

The paper is organized as follows. Section 2 reviews theoretical predictions regarding the impact of transport expansions on informality rates. Section 3 presents some generalities of our area of study, a brief historical review of the evolution of the public transport system in the region and data and definitions for the empirical analysis. Section 4 presents our empirical approach, detailing our identification challenges and proposed strategies. Section 5 discusses the results. Section 6 discusses and concludes.

2 Theoretical Predictions

Existing theoretical models offer some insights on the effect of new transport infrastructure on variables such as income, productivity and employment levels. From the perspective of the firm, improved access could have a positive effect on employment and productivity through lower input of labor costs, higher agglomeration externalities or more efficient sorting, but also a negative effect through higher commercial rents (Redding and Turner, 2014; Gibbons et al, 2012).

These predictions cannot be readily extended to our case because the underlying model needs to consider worker heterogeneity, that is, how workers living in a city sort into different types of jobs (e.g., formal and informal) and into locations within the city, and how they respond to changes in transport access. The model of Moreno-Monroy and Posada (2014) takes a step in this direction by relating the informality rate to intra-urban transportation costs. It yields predictions regarding the impact of changes in transport costs (such as the ones caused by public transport expansions) on the informality rate. Here we summarize the set up and predictions of the model, and refer the reader to the original paper for details and derivations.

The model considers a linear, monocentric city with a unique Central Business District (CBD), where all formal firms locate. Formal and informal workers optimally decide to reside at any point between the center and the city fringe. In the urban formal sector, the hiring process is subject to search frictions (Pissarides, 2000). Formal workers are assumed to commute every day to work. In the informal sector, the wage is assumed to be fixed, higher at the CBD than at home, but in any case lower than the productivity in the formal sector. Informal workers do not actively seek formal jobs, and receive a social protection transfer from the government. Unlike formal workers, informal workers do not commute daily to the CBD because they can also work at or near home, so they have a strictly lower commuting frequency than formal workers. Consequently, the bid-rents for formal workers are steeper, reflecting their relative preference for proximity to the CBD.

The urban land use equilibrium obtained after defining the bid-rents and instantaneous utilities for each type of worker yields a *segmented city*, where formal workers reside at the CBD, and informal workers reside next to this area. In equilibrium, urban costs for formal workers are always larger because they face higher commuting costs. In equilibrium, the formal wage is a function of the compensation that formal firms have to pay in order to induce unemployed workers to accept a job in the formal sector. This compensation is dependent on the informal sector income, which besides the informal wage, includes subsidies and commuting costs savings. The model yields the following general expression for the informality rate:

$$\theta = f(\mathbf{X}, T) \tag{1}$$

Where $\mathbf{X}$ includes the level of output in the formal sector, the informal wage, the commuting frequency of informal workers, the population level and the social protection transfers, and T are intra-urban commuting costs. Holding all other variables constant, a decrease in commuting costs leads to an decrease in the informality rate, because the required spatial compensation borne by formal firms becomes smaller, leading to more formal job creation.

The model describes a mechanism through which commuting cost reductions (in terms of time and/or money) lead to lower informality rate levels at the city level. Two qualifications are in order. The first one is that in reality, transport access does not improve in all areas of the city simultaneously. This means that the impacts of improved access are not homogenous throughout the city. Following the logic of the model, we would expect that areas where the new transport infrastructure is placed would experience reductions in their local informality rates, because formal workers commuting daily would seek access to transport infrastructure and outbid informal workers in these areas.

The second qualification is that the model does not consider different transportation modes, and in particular, the role of cars. Currently there are no models considering simultaneously the existence of an informal sector and different modes of transportation. The model of LeRoy and Sonstelie (1983) and the empirical application of Glaeser et al (2008) offer some insights as to how the inclusion of a second mode could change the predictions described above. In a linear city model, assuming a two transportation modes (public transport, which is cheaper but slower, and cars, which are more expensive but faster) and two income groups (the rich and the poor), the optimal car trip length and distribution of rich and poor within the city depends on costs of cars relative to income. Glaeser et al (2008) show that for appropriate values for the income elasticity of housing demand and other parameters, local poverty rates can increase as a result of local improvements in public transport. This happens because, on the one hand, the rich value their time more highly, so they have a preference for car commuting, and, on the other hand, the poor seek proximity to public transport, because their time valuation is lower and this mode is less expensive. This prediction could be extended to the case of formal and informal workers, which would imply that local informality rate would increase in areas near new public transport access points.

The highly stylized models discussed in this section yield contradictory predictions regarding the impact of public transport expansions on local informality rates. As in the case of the impact of transport in other economic outcomes, establishing the direction of the effect remains an empirical question (Gibbons et al, 2012).

3 Case study: São Paulo Metropolitan Region

3.1 General facts

The São Paulo Metropolitan Region (SPMR) hosted nearly 20 million inhabitants in 2010. The formation of the city is the most eloquent example of the rapidly urbanizing process that Brazil experimented during the last century, when the city displayed average annual growth rates higher than 4,5 percent until 1950. After the fifties, the city experienced its most intense expansion process influenced by the placement of industrial parks, leading to a persistent structural spatial reconfiguration that holds close relation with a strong monocentric structural organization. In the 1960's, during the military dictatorship, the city government made efforts to re-organize the urban space through the construction of extensive social housing complexes on the East-side of the city, and through large-scale transport projects such as the metro (Ramalhoso, 2013). However, in the following decades, the vast peripheries occupied by the poor and less instructed population (the so-called "suburban peripheral belt") were formed by a process of unplanned centrifugal expansion. The accompanying extensification of urban land use was associated to a sub-market of informal land allotment, combining the strategic behavior of informal developers seeking cheap and undeveloped land at the outskirts of the city, and the permissiveness of the state (Abramo, 2009; Rolnik, 1997). Today the continuous urbanized area extends for more than 2,000 km2, including areas in 30 different municipalities.

The complexity of the SPMR economy reflects its importance as the highest level in the Brazilian urban hierarchy playing an important role as the connection node between domestic and global economy. During the last decades, the SPMR experienced the transition from a industrial to a service based economy. After the seventies, the industrial sector lost its relative importance to the tertiary sector in a rapid process of productive restructuring reflecting at the same time the decrease in the relative importance of the SPMR in the national industry and a profound internal organizational and technological transformation (Diniz and Diniz, 2007). In 2010, the tertiary sector that includes commerce and services was responsible for more than 75 percent of all the economic production in the metropolis (IBGE, 2010a). In terms of employment, this sector was responsible for 61 percent of the workplaces, while the industrial sector was responsible for 23 percent (RAIS, 2010).

During the last decade, the overall unemployment and informality rates in Brazil fell considerably. The informality rate (measured roughly as the percentage of unregistered employees) for Brazil fell around 8 percentage points, from a level of 57 percent in 2000 to 49 percent in 2010. In the same period, the SPMR experienced a decrease from a 31 percent in 2000 to 23 percent in 2010 (IBGE, 2000; IBGE, 2010b). According to some studies (De Moura and Barbosa Filho, 2014; Andrade, 2013), the drop in informality can be explained by factors related to individual characteristics and also institutional changes. The expansion of formal education and population aging were key variables playing important role in the formalization expansion. Among the institutional changes, the revamp in the small business by the introduction of the so-called SIMPLES scheme -a simplified mechanism for taxing small businesses- contributed significantly to the formalization of the low qualification jobs.

3.2 Evolution of the mass public transport system in the SPMR

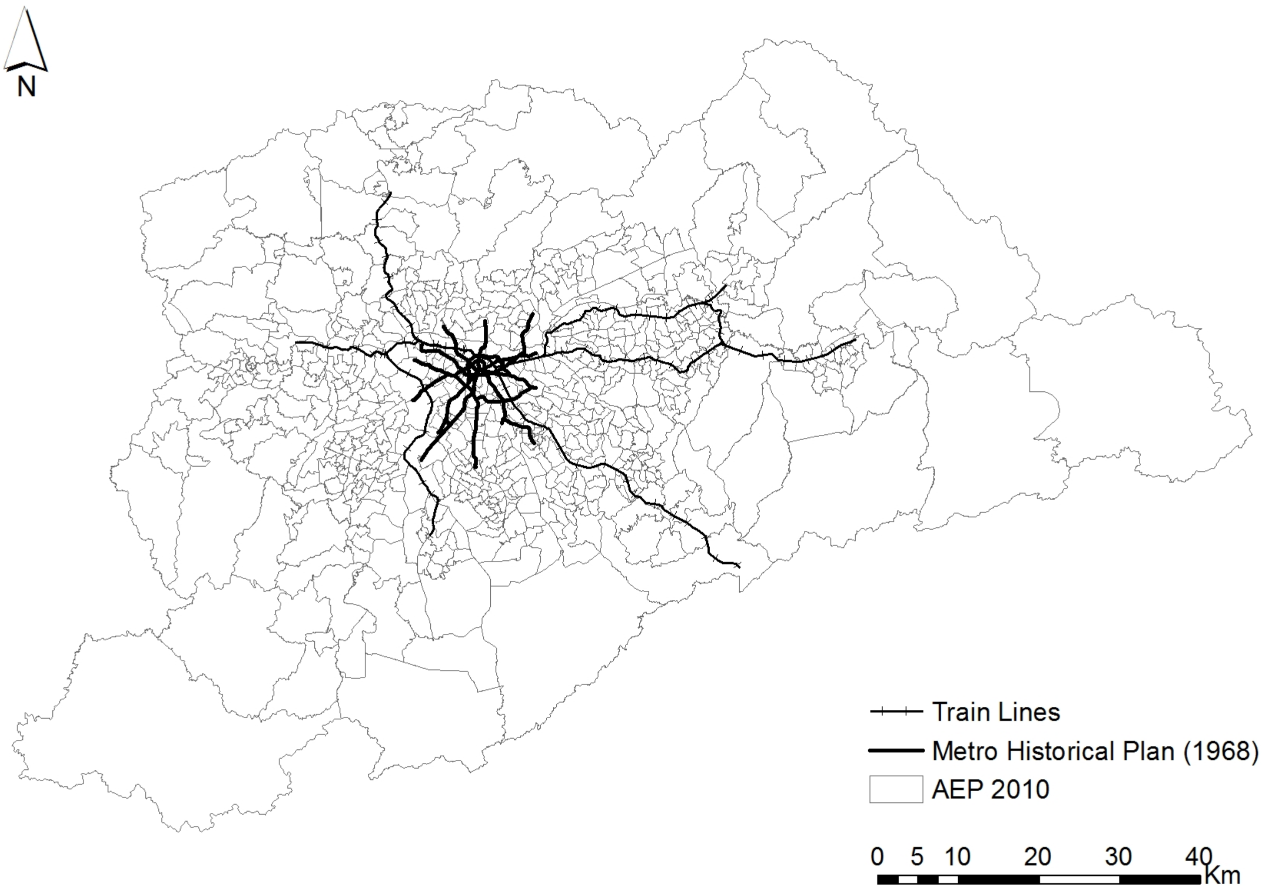
The history of the mass public transport system of the SPMR dates back to the opening of the São Paulo Railway in 1867, linked to the transportation needs of the growing agricultural exports. In the next decades, several railway lines were constructed to connect São Paulo with the rest of the country and with surrounding rural areas. Today, six railway lines make part of the integrated transport system of the city. Between 2000 and 2010, a total of 18 stations where built and re-opened, in an effort to use the existing railroad infrastructure to expand the capacity of the urban rail system. Many of these improvements, and the modernization of the trains, were already planned in the 1980s, but only executed more than two decades later (Kiyoto, 2013).[2]

The first official plan for the metro network dates back to 1968, when São Paulo was already an established metropolis of around seven million inhabitants. The basic network plan, elaborated by the German-Brazilian consortium HMD (Hochtief - Montreal Enterprises - Deconsult), consisted of 66,2 km divided into four lines and 68 stations, and was projected to be finished by 1987 (Figure 1) (Ramalhoso, 2013). By 2010, the number of stations and extension of the network was below the levels projected by the 1968 plan. The construction paced two km per year on average between 1974 and 1990. By 1990, the network had reached an extension of 45 km, while the urban population had more than duplicated (De Carvalho, 2010). The construction pace did not take off in the following decades, mostly due to financial restrictions and the priority role given to road traffic. By 2010, the metro network reached 62,3 km of extension, meaning that there were on average 3,2 km of metro line per million inhabitants. Between 2000 and 2010, 11,2 km of line were added to the network, split between a new line in 2002 (with five stations) and an extension of an existing line in 2006 (two new stations).

According to the 1999 official transport infrastructure plan (PITU 2020), five new stations in two different lines should have been finished by 2010 but eventually were not delivered. The delays concerning the construction of Line 4 are particularly salient, since it was planned since 1940, and its actual configuration was established in 1995. The first phase was officially planned to be concluded in 2006, but it was only delivered between June 2010 and 2011. According to a World Bank's independent implementation completion report review of the São Paulo Metro Line 4 Project (World Bank, 2012), the four year delay was due to "procurement litigation, unavailability of counterpart funds, accident caused by engineering defects resulting in 7 deaths, and resettlement problems suggesting inadequate social assessment".

[2]See Figure A.1 in the appendix for a map of the SPMR urban transport network in 2010.

Figure 1: HMD network plan



The integrated mass transport system of the city is complemented by exclusive lines for buses, although only one line (*Expresso Tiradentes*) meets the Bus Rapid Transport (Silver) standard. The first corridors were opened in 1980, but only until the 2000s the bus network was made part of the integrated transport system. To this end, the construction of 108 km of bus lines between 2000 and 2010 was complemented by the use of an electronic ticket (which by 2004 was integrated with the metro and urban railways ticket under the *Bilhete Único*). The implementation of exclusive bus lines has also faced considerable delays, mostly due to changes from the initial plans to favoring monorails, and community resistance to expropriations. By 2012, there was an estimated delay of 66 km of bus lines compared to the goals set in 2008. An example is the Celso Garcia corridor on the East side of the city, which was expected to cover a demand of 400,000 passengers per day. The corridor was first projected to open in 2007, but as of 2014 was still in the bidding phase.

4 Empirical approach

4.1 Data sources and definitions

For our empirical analysis, we consider the variation in the informality rate across units within the SPMR and across time. In order to construct the informality rate by area, we aggregate the number of informal workers in each area, and divide it by the total number of workers (i.e., the sum of formal and informal workers). In order to determine the status of each worker, we use micro-data from the 2000 and 2010 Demographic Census of Brazil on: occupational status and type of employment (on the main job), and whether the person made contributions to social security. A worker is classified as informal if he or she is an unregistered employee (*empregado sem carteira assinada*), or a self-employed individual not contributing to social security, or an employer not contributing to social security (Jonasson, 2011; Henley et al, 2009). A formal worker, by contrast, is a registered employee (*empregado com carteira assinada*), or self-employed individual contributing to social security, or an employer contributing to social security. As explained by Jonasson (2011), who uses a similar criteria, this definition corresponds to the "no signed labor card" criteria of Henley et al (2009). As the questions and possible answers are very similar for the two census rounds, this measure can be considered consistent over time.

Our geographical unit of analysis is the Weighted Spatial Area (*Área Espacial de Ponderacão*, AEP) defined by the IBGE (2000; 2010b) for the Census sample 2000 and 2010 as an area composed by a mutually exclusive set of censuses tracts designed to give the necessary statistical robustness to the sampling strategy. As the number of AEP is larger in 2000 than in 2010, we set the 2010 areas as reference and reconcile the 2000 areas by a process of aggregation using geoprocessing tools. In the Census sample 2000, the number of AEPs was 812. For the Census sample 2010, there are a total of 633 units covering all the geographical extension of the administrative territory of the SPMR. After the aggregation procedure, in terms of population, we have an average of 28,243 inhabitants in 2000 and 31,096 in 2010. As the AEP also covers rural areas of the SPMR, the minimum population for the entire sample was 6,382 inhabitants in 2000 and 8,258 in 2010, and a maximum of 123,112 and 155,804 for 2000 and 2010 respectively.

4.2 Empirical strategy

Our aim is to estimate the impact of public transport expansions on informality. To do so, we can compare the average changes in informality rates in areas which received new transport infrastructure with the average changes in informality rates in areas which were supposed to receive new transport infrastructure but did not for diverse reasons. In terms of the differences-in-differences strategy that we will implement, our sample is composed of areas which were supposed to have, according to official plans, new transport infrastructure by the end period, and split it into areas which effectively received the new infrastructure (the treatment group) and areas which did not (the control group).

Coming back to equation 1, a general structural equation describing the informality rate could be expressed as:

$$Inf_{it} = \delta T_{it} + \mathbf{X_{it}}\beta + U_i + e_{it} \tag{2}$$

where Inf_{it} is the informality rate in area i and year t, T_{it} is a vector of treatment variables which supposedly have a (causal) effect on the informality rate; $\mathbf{X_{it}}$ is a matrix of observed control variables; U_i is a vector unobserved components influencing the informality rate and e_{it} is an error term. Assuming there are only two periods, and that all areas have not received treatment in the base period (i.e. the treatment variable is 1 only in the post-treatment period), first differencing equation (2) yields:

$$\Delta Inf_i = \delta \Delta T_i + \widetilde{\mathbf{X}}_{\mathbf{i}}\beta + \varepsilon_i \tag{3}$$

First differencing allows canceling out unobserved time-invariant fixed effects, and also time-invariant observable controls that are uncorrelated with T. Thus, $\widetilde{\mathbf{X}}_{\mathbf{i}}$ includes a vector of ones, and the initial values of $\mathbf{X_i}$ in the pre-treatment period.[3] Under the condition that treatment is fully randomized, an OLS estimate of δ can be interpreted as the 'intention to treat effect' (ITT) given that some people may not make use of the new infrastructure (Gibbons et al, 2012).

A fundamental issue with our identification strategy is the need to select areas which are similar in terms of relevant characteristics, but which differ in their level of treatment. How comparable are our treatment and control groups? Note that the fact that all the areas in the sample were officially considered to be suitable for transport projects means that they share similar characteristics precisely in terms of those variables which are relevant for the allocation of transport infrastructure (e.g. unmet demand for mass transport, soil quality, distance to the existing network, etc.). However, this does not guarantee that the treatment and control groups have the same joint distribution of observables and unobservables, as required for estimating ITT effects. One option to re-balance the treatment and control groups is to control for a series of relevant observable area attributes. We include the pre-treatment values of relevant socio-economic variables interacted with a time dummy (which can be consequently seen as exogenous to treatment).

4.3 Identification and econometric issues

Two issues remain. The first one is the possibility that the treatment is not random. The second one is selection into treatment, which would compromise the required random allocation of the treatment.

[3]We do not include the first difference of controls that are likely to be correlated with ΔT, as this would render the estimates of δ inconsistent (Baum-Snow and Ferreira, 2014).

Selection is likely to happen for two reasons. The first one is that people who will be impacted by future public transport expansions may be actively involved in the project decision making. The second one is that the selection procedure follows a predefined logic where areas with certain characteristics are preferred over others (e.g., central areas or areas that are closer to the existing network may be preferred by planners). The presence of endogenous selection into treatment means that OLS estimates are biased.

These concerns can be potentially tamed with an Instrumental Variables (IV) strategy. Coming back to equation 3, we can think of ΔT as an endogenous binary treatment variable modeled as stemming from an unobserved latent variable ΔT^*, which is, in turn, specified as a linear function of an exogenous covariate (instrument) z and a random component μ_i:

$$\Delta T_i^* = \gamma z_i + \mu_i \tag{4}$$

and the observed treatment decision rule is $\Delta T_i = 1$ if $\Delta T_i^* > 0$, 0 otherwise. ε_i and μ_i are bivariate normal with covariance matrix

$$\begin{bmatrix} \sigma^2 & \rho\sigma \\ \rho\sigma & 1 \end{bmatrix}$$

By replacing the decision rule in equation 3, we can also express the model as a switching regression with two regimes (treatment and non-treatment) (Quandt, 1972):

when $\Delta T_i^* > 0$, $\Delta T = 1$: $\Delta Inf_i^1 = \delta\Delta T_i + \widetilde{\mathbf{X}}_\mathbf{i}\beta + \varepsilon_i$ and when $\Delta T_i^* < 0$, $\Delta T = 0$: $\Delta Inf_i^0 = \widetilde{\mathbf{X}}_\mathbf{i}\beta + \varepsilon_i$. If the allocation of treatment is not randomized, there is possible correlation between ε_i and μ_i. If there is endogenous selection into treatment, there is possible correlation between ε_i and an unobserved variable driving selection, and μ_i and the same variable that drives selection, resulting in correlation between ε_i and μ_i through this third variable.

It is possible to derive a join density function of ΔInf_i and ΔT, a likelihood function of the model represented by equations 3 and 4, and an efficient Maximum Likelihood estimator (Maddala, 1983). For achieving consistency, the instrument z has to meet two conditions: to explain changes in public transport (i.e., have power on a first-stage regression with dependent variable ΔT), and satisfy the restriction of affecting the outcome exclusively through the measure of public transport expansions conditional on other controls.

The correlation between ε_i and μ_i, ρ, signs the endogeneity bias. If the null hypothesis that ε_i and μ_i are uncorrelated is rejected, OLS estimates are biased, and the sign of ρ indicates whether OLS estimates are upward or downward biased. It is difficult to conceptually pinpoint the direction of the endogeneity bias, as transport infrastructure provision is guided by multiple, overlapping criteria. For instance, planners could prioritize central, rich areas where informality rates are improving because they concentrate jobs; but if infrastructure provision is part of a poverty reduction strategy, they could also favor lagged areas where informality rates are worsening.

4.4 Measurement and estimation

4.4.1 Variables and sample

Our dependent variable is the informality rate growth between 2000 and 2010, approximated by the difference of the natural logarithms of the post-treatment and pre-treatment values: $\Delta lnInf_i = ln(Inf_i^{2010}) - ln(Inf_i^{2000})$. To build the sample, we first drop all the areas that had a metro station, a train station and/or a bus corridor by end December 1999. We then build our treatment group. The treatment variable ΔT equals one for area i if a metro station, a train station or bus corridor was opened between January 2000 and January 2010 in that area. We consider buffers of 100 and 200 meters around the stations and corridors as a robustness check.

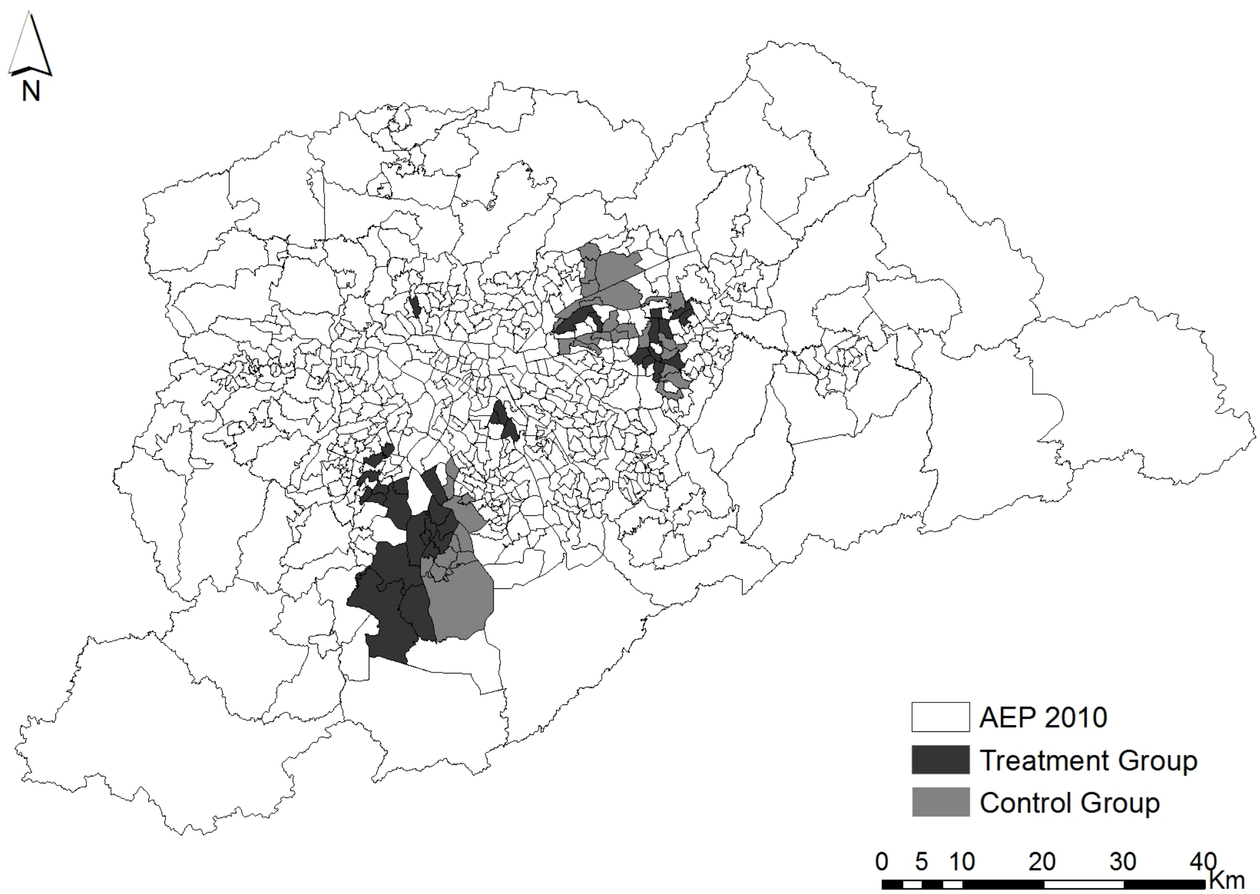
To build the control group, we carefully analyzed official transport infrastructure plans (such as PITU 2020, released in 1999) and news reports about public transport project delays in the period 2000-2010 (STM, 1999). We also used information on bids for bus corridors for construction works which were supposed to be finalized by 2010 but which in reality had not started by 2010. All variables except income per capita in 2000 seem to be within the same range. To make the groups comparable, we drop observations in the tenth decile of income per capita.

Table 1 shows the summary statistics for the final sample. Although the treatment and control areas have similar average levels of initial conditions and geography variables, we include these variables as control variables in the regressions. Figure 4 displays the location of the treatment and control groups.

Table 1: Summary Statistics

	Mean	Std. Dev.	Min.	Max	Mean	Std. Dev.	Min.	Max.
Variable	Treated (n=35)				Control (n=30)			
Informality rate 2000	0.42	0.05	0.32	0.51	0.43	0.04	0.35	0.50
Informality rate 2010	0.29	0.03	0.20	0.36	0.30	0.03	0.24	0.38
Growth informality rate 2000-2010	-0.37	0.09	-0.53	-0.19	-0.35	0.11	-0.63	-0.12
Income per capita 2000	445	233	234	1208	400	191	227	997
Population 2000	35,989	98,806	13,440	54,421	38,833	11,568	4,331	54,518
Population Density	0.012	0.006	0.001	0.024	0.011	0.005	0.001	0.018
Area (sq. km)	60.72	9.59	1.38	52.04	67.25	11.43	1.70	62.83
Distance to CBD (km)	19.84	6.15	6.12	34.27	20.50	4.29	1.20	28.38
Topography	15.99	5.12	1.94	25.75	15.76	3.99	8.97	27.25

Figure 2: Treatment and control groups



4.4.2 Instrument and estimator

Following the most recent developments in the econometric estimation of transport change impacts (Redding and Turner, 2014; Baum-Snow and Ferreira, 2014), we use a historical plan as an instrument for transport access changes. In particular, we construct a variable defined as the km of line of the plan outlined by the HMD consortia in 1968 that cross the area (Figure 1). The idea is that initial network plans can predict future network developments, but are exogenous to changes in informality rates. The exogeneity argument relies on the changes experimented in the size and structure of the city between 1968 and 2000, and which could not be foreseen by planners in the 1960s. What is required in particular is that the 1968 plan was not designed to anticipate the change in the informality rate between 2000 and 2010. The 1968 plan was made in a context of high economic growth and strong planning during the military dictatorship, mostly to satisfy the immediate transport demands of existing central and high-density residential areas (Kiyoto, 2013). Ramalhoso (2013) discusses how the HMD consortia acknowledged they had based their plan on "natural commuting trends" because they lacked a general urban plan for the city (a plan that was eventually delivered in 1969). It seems plausible, then, that urban transport planners at the time could not foresee the pattern of urban occupation through massive rural-urban migration waves in the following decades, nor the emergence and evolution of a segmented labor market.

Still, it is possible to argue that the historical network plans are correlated with third variables that may be related to the current distribution of the informality rate, such as distance to the center, ruggedness of the terrain and the size of areas. We include these variables as additional controls. What is desirable is that the significance of the instrument z on the first-stage regression is not affected by the inclusion of these controls, as in Garcia-Lopez (2012).

We proxy distance to the center as the linear Euclidean distance between each area's centroid and the geographical coordinates of the main center of the urban agglomeration, identified as the place with higher employment density and higher number of employments among all the AEP (Ramos, 2014); the ruggedness of the terrain as standard deviation of the altitude of the terrain, considering the digital elevation model derived from the Shuttle Radar Topographic Mission (Biderman and Ramos, 2013); and the total size of the area, calculated from the geometric attribute of the georeferenced polygonal data. All geography controls are transformed using natural logarithms to improve normality.

Maddala (1983) derives the likelihood function of the model represented by equations 3 and 4, and derives a Maximum Likelihood (ML) estimator of ΔT , which is more efficient than a two-step estimator. We initially use the ML estimator provided in the Stata command *treatreg*, and given the likely presence of heterogeneity, we use the robust variance estimator. Note that in principle, in this model the instrument z refers to a treatment allocation rule so that if z is below some threshold, treatment is given, and otherwise if z is below the threshold. As it is difficult to find an observable treatment allocation rule in our case, we treat the historical plan as such rule. There is a risk, however, that equation 4 is misspecified if the distributional assumption of joint normality of ε_i and μ_i is not correct. In this case, the ML estimator is inconsistent. We also obtain two-step consistent estimates. If these estimates are close to the ML estimates (but as expected, less efficient), we can conclude that the restriction imposed by the distributional assumption is not problematic.

5 Results

5.1 OLS

We first estimate equation 3 by OLS. Table 2 presents the results. Column (1) shows the results using the full sample. As explained earlier, these estimates are likely to be biased because we are comparing the treatment group with the very heterogeneous group of areas that were unconnected by 2000 and remained so by 2010. Columns (2) and (3) show the OLS results for the restricted control group after controlling for initial conditions and geography. All geography controls are not statistically significant (not reported) at the 10 percent level. The effect of public transport expansions on informality rates, conditional on initial conditions and/or geography, is negative but small and remains insignificant. We suspect that the OLS estimates are biased because of selection and endogeneity, for which we turn our attention to the endogenous treatment results.

Table 2: OLS Results

Dependent variable: Informality rate growth 2000-2010				
	(1)	(2)	(3)	(4)
Public transport expansion	-0.0120	-0.0168	-0.0146	-0.0116
	(0.0164)	(0.0256)	(0.0257)	(0.0256)
ln Income per capita 2000			0.000507	-0.0570
			(0.0316)	(0.0567)
ln Population density 2000			-0.0234**	0.00926
			(0.0101)	(0.0215)
Geography controls	N	N	N	Y
Observations	495	65	65	65
R-squared	0.001	0.007	0.050	0.105

Regressions include a constant term. Robust standard errors in parentheses.
OLS estimator. *** p<0.01, ** p<0.05, * p<0.1

5.2 Endogenous treatment-effects

5.2.1 First stage

As explained before, the validity of the proposed instrument relies on its relevance and exogeneity with respect to the outcome variable. To assess relevance, we perform a probit estimation of equation 4. Table 3 shows the results. As can be seen in Column (1), the instrument is statistically significant and positive, indicating that areas that were part of the original network plan of 1968 were more likely to receive infrastructure between 2000 and 2010. The instrument seems to be relevant, as it can, by itself, explain 11 percent of the variation in the treatment variable. By adding initial and geography controls, we test the possibility that the relevance of the instrument is not affected by the inclusion of factors that could affect both the changes in transport access and the historical network plan. This seems to be the case, as the magnitude and significance of the point estimates associated with the historical plan are barely affected by the inclusion of initial or geography controls.

Table 3: First stage probit estimation results

Dependent variable: Transport Access Dummy			
	(1)	(2)	(3)
Historical Plan	0.736***	0.722***	0.741***
	(0.273)	(0.278)	(0.286)
ln Income per capita 2000		0.159	-0.101
		(0.453)	(0.812)
ln Population Density 2000		0.0100	-0.193
		(0.184)	(0.403)
Distance to CBD			-0.402
			(1.086)
Area			-0.202
			(0.472)
Topography			-0.0801
			(0.417)
Pseudo R-squared	0.109	0.111	0.116
Observations	65	65	65

Regressions include a constant term.
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.2.2 Endogenous treatment-effects

We now turn to the results of the endogenous treatment-effects model. We estimate equation 3 using the ML estimator described in section 4.4.2. Our instrument list includes, besides the historical network plan variable, other exogenous covariates if appropriate (initial controls or initial and geography controls). The results are shown in Table 4. Before analyzing the results, we discuss the validity of the model and the approach. We begin by assessing the goodness of fit of the models. The p-value of the Wald test of all coefficients in the regression being zero supports the relevance of the covariates used in the regression. We then assess the appropriateness of the endogenous treatment model. The p-value of the Wald test of independence of equations 3 and 4 is shown at the bottom of Table 4. The null hypothesis is that ρ is equal to zero, or in other words, that equations 3 and 4 are independent. We can reject the null hypothesis at a 5 percent level for all the specifications. This point to evidence of possible selection and endogeneity bias in the OLS estimations.

We now turn to the discussion of the main results. The estimated "intention to treat" effect, which is an indicator of public transport expansions impact net of observed selection bias, is given by the

Table 4: Endogenous treatment-effects results

Dependent variable: Informality rate growth 2000-2010			
	(1)	(2)	(3)
Public transport expansion	-0.160***	-0.172***	-0.183***
	(0.0406)	(0.0373)	(0.0396)
ln Income per capita 2000		0.0255	-0.0255
		(0.0312)	(0.0539)
ln Population Density 2000		-0.0200	-0.00146
		(0.0140)	(0.0348)
Geography controls	N	N	Y
Observations	65	65	65
Wald test coefficients χ^2	15.59	24.37	29.22
Wald test coefficients Prob > χ^2	0.000	0.000	0.000
ρ	0.837	0.873	0.919
Wald test ($\rho = 0$): χ^2	6.26	7.81	8.37
Wald test ($\rho = 0$): Prob > χ^2	0.0123	0.0052	0.0038

Regressions include a constant term. Robust standard errors in parentheses.
ML estimator. *** p<0.01, ** p<0.05, * p<0.1

Instrument list: (1) z, (2) z, initial controls, (3) z, initial and geography controls

coefficient associated with the public transport expansions dummy (δ in equation 3). As can be seen in Table 4, this coefficient varies between -0.16 and -0.18. Initial and geography controls and geography do not seem to influence the magnitude and significance of the treatment variable, and are statistically not significant in the full specification.[4]

Before we interpret the results, we compare the OLS and endogenous treatment estimates. As can be seen at the bottom of Table 4, ρ is estimated to be positive, which means that the OLS estimates are biased up. Since δ is negative in the estimations, the ML estimator yields a smaller point estimate of δ than the OLS estimator (a larger negative number). The usual expectation is that IV estimates should be smaller than OLS estimates (Wooldridge, 2002), but works using a historical plan to estimate the impact of highways on population density have found both IV estimates larger (Baum-Snow, 2007; Duranton and Turner, 2012), and smaller than OLS estimates (Baum-Snow et al, 2012; Garcia-Lopez et al, 2013). Redding and Turner (2014) suggest that comparing OLS and IV estimates gives implicit evidence on the underlying transport infrastructure allocation process. In our case, IV (negative) estimates smaller than OLS (negative) estimates would suggest that urban transport allocation is biased towards areas experiencing a smaller decrease in informality rates.[5]

The OLS estimates underestimate the difference in impact of transport access on changes in informality rates between areas that received infrastructure and areas that did not but that should have

[4]These results hold for the two-step estimator (results available upon request).

[5]In other words, unobservables associated with decreases in the informality rate tend to occur with unobservables associated with lower public transport expansion.

had. The endogenous treatment-effect estimates imply that, net of endogenous selection and keeping other things equal, areas which received new transport infrastructure between 2000 and 2010 reduced their average informality rate at least 16 percent faster than areas which were supposed to receive infrastructure but did not because of delays. The estimated impact seems to be very high, but it has to be interpreted within the context of the particular time period we are analyzing. As discussed in section 4.2, the informality rate experienced a fast decrease, and all areas, except for one, improved in terms of informality rates between 2000 and 2010. The decrease in informality rates in our sample varied between 67 and 13 percent, with the average area experiencing a decrease of 36 percent. Our estimates suggest that the average area in the control group would have had an informality rate 4 percentage points lower, had it received new urban transport infrastructure.

The ITT estimate provides a lower bound of average treatment effects. Note however that this interpretation would not be valid if the assumption of homogeneous response to treatment does not hold (Blundell and Costa-Dias, 2008). We check for possible heterogeneous response to treatment in the next section.

5.3 Robustness checks

In order to assess the robustness of the results, we conduct a series of robustness checks.[6] First, we experiment with other proxies for our control variables and also include additional control variables in equation 3. Second, we consider the possibility that the area of influence of the new infrastructure is larger, and create 100 and 200 meter buffers around the stations and bus corridors to construct a new sample. We then re-estimate equation 3 with this new sample. Third, we consider the presence of heterogeneous effects. It could be the case that the new infrastructure has a different impact on poorer areas than in richer areas (Boarnet, 2007), or in areas that are closer to the CBD compared to areas that are further away. If this is the case, the ML estimates could be biased. Fourth, we perform a "placebo test". We run the ML regressions using a different control group composed by areas considered for future public transport expansions according to the PITU 2025 official plan (STM, 2006), but which were not expected to receive new infrastructure by 2010. We expect the coefficient associated with transport expansions to be statistically insignificant.

We first replace the proxies for initial level of economic development and population. We estimate equation 3 using the percentage of people with basic education (i.e., at least 7 years) in total population older than 10 years-old instead of the log of income per capita[7], total population in 2000 instead of population density in 2000, and total population and population density in 1991 instead of the 2000 values. These changes do not affect the magnitude, significance or validity of the estimates of δ discussed previously.[8] We also include an additional variable measuring the initial demand for public transport by

[6]For the sake of space, we do not present the results tables here, but they are all available upon request.
[7]The correlation between these two variables is 0.95, so including them simultaneously would induce multicollinearity issues.
[8]For the sake of presentation, we do not display the result tables of these regression. All results are available upon request.

area, proxied by the number of collective trips as a percentage of total trips originated in the area in 1997.[9] Alternatively we use data from the 2000 Population Census on the number of owned cars by household divided by the number of households as an alternative proxy. These variables turn out to not significant in the regressions with and without controls, while the estimates of δ remain unaffected.[10]

By using buffers around stations and corridors, we want to dismiss the possibility that areas in the control group do receive treatment, which would invalidate interpretation of our ITT estimates. The results for the endogenous treatment-effect estimates with and without geography controls hold for the samples with 100 and 200 meter buffers.

Next, we consider the presence of heterogeneous response to treatment. We estimate a binary treatment model with idiosyncratic average effect that controls for selection on unobservables (IV estimation) and for heterogeneous effects. For the estimation, we use the user-written Stata command *ivtreatreg* developed by Cerulli (2012). The command returns probit Two-Stage Least Square (TSLS) estimates, where the predicted probabilities resulting from estimating equation 4 are used as instruments for z (Cerulli, 2012). We consider heterogeneous response to treatment with respect to: 1) initial conditions and, 2) geography. The TSLS estimator yields similar but less precise point estimates of the effect of public transport expansions (this is expected as the ML estimator is superior to a TSLS estimator in terms of efficiency). The point estimates associated with the additional heterogeneous treatment-response (denoted by the suffix ws) are not statistically significant, indicating that heterogeneity in the response to treatment may not be a major concern.

Lastly, the placebo test showed that, as expected, the impact of transport access is no longer significant when we use a new control group in ML regressions with and without controls.

6 Conclusion

In this paper we have estimated the impact of public transport expansions on informality for the São Paulo Metropolitan Region. We measure local informality rates using individual-level data from the 2000 and 2010 populations censuses. We identified areas which received new transport infrastructure (bus lines, and/or train or metro stations) between 2000 and 2010, and compare their informality rates with those of areas which were supposed to receive infrastructure in the same period but that ultimately did not receive it because of project delays. To circumvent possible endogenous selection issues, we instrument public transport expansions with a variable based on a 1968 network plan. The results suggest that endogenous selection is indeed a valid concern. According to our preferred estimates, informality rates decreased on average 16 percent faster in areas receiving new public transport infrastructure compared to areas that faced project delays. These results are robust to the specifica-

[9] We construct this variable using data from the 1997 origin-destination survey for the SPMR).

[10] For the case of the percentage of collective trips, the ML estimator does not converge, so we compare the two-step consistent estimators.

tion changes, alternative control variables, different distance buffers, using a different estimator, and considering heterogeneous effects.

In this paper we have provided a first approach to the study of the effects of public transport expansions on labor market outcomes. By considering project delays, we have given a meaningful interpretation to difference-in-difference estimations, and at the same time, we have included in our sample areas that are in principle comparable. Our study suffers, however, from a series of limitations. First of all, it is necessary to stress that the estimates apply to the case of the selected sample in the SPMR, and cannot be readily applied to other cities, or even to different zones within the SPMR. Second, the unavailability of finer geographical detail in the labor-market data means that we cannot establish the true spatial range of the estimated effects. It also impedes us to analyze separately the effect of metro/train stations and bus corridors. Third, from the point of view of the interpretation of the results, the lack of detailed information on individual residential and work choices impedes us to give a more meaningful interpretation to our results. It would be desirable to know, for instance, which part of the estimated impact is due to reallocation from already formally-employed workers, and which part is due to the improvement in job quality of local residents. Another key missing element is information on modal choices of workers when faced with better quality jobs, better transport access but also with the possibility of switching to cars. Future studies will hopefully address these kind of questions, which are highly relevant for understanding urban segregation and inequality.

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Appendix

Figure A.1: Urban transport network in the SPMR by 2010