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The Political Economy and Economic Effects of Large-Scale Public Policies

Author
Vial Lecaros, Felipe Andres

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The Political Economy and Economic Effects of Large-Scale Public Policies

by

Felipe Andrés Vial Lecaros

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Committee in charge:

Professor David Card, Chair
Professor Edward Miguel
Professor Marco González-Navarro

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Abstract

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This dissertation studies the implementation of large-scale public policies in developing countries. Chapter I focuses on how collective action can shape public policy. In particular, I study how organized groups of workers affected the land reform implemented by Salvador Allende´s government in 1970 in Chile. Using an event-study design, we find that the local political action of workers - proxied by land invasions - affected the intensity and location of expropriations. In Chapter II, I change time and location and study how political favoritism affected the expansion of the electric grid in Kenya around the 2013 and 2017 presidential elections. While the aggregate political bias we estimate is meaningful, it is smaller in magnitude than that documented previously, suggesting that increasingly active independent media scrutiny as well as increasingly robust democratic institutions, expanding constraints on executive power, and donor oversight may have partly curbed favoritism. Finally, in Chapter III I study the effects on welfare of a large-scale public investment: the expansion of the subway network in Santiago, Chile between 2002 and 2020. We find that when workers receive access to the subway network, they start taking jobs further away. We see an increase in wages for benefited workers, including those workers who do not switch firms, suggesting a reduction in the labor market power firms have over workers. Our model estimates suggest incorporating labor market power can make an important difference in welfare estimations.

Contents

List of Figures

List of Tables

Acknowledgments

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Dissertation Introduction

This dissertation studies the implementation of large-scale public policies in developing countries. Large-scale policies require rigorous technical design, but they also involve many actors and the power relationships between them. The purpose of this thesis is to further our understanding of the different stages through which public policies go through.

In the first Chapter, I study how collective action can shape public policy. The Chapter shows that organized groups of workers affected the intensity of one of the largest policies of the time: Salvador Allende's land reform in Chile (1970–1973). After being elected president in a contentious election, Allende attempted to create a "democratic road to socialism" and used the existing land reform program to expropriate more than six million hectares with the goal of empowering agricultural workers. In this context, groups of workers exerted pressure to radicalize policies and accelerate the transition to socialism. We show that the collective actions of workers influenced the government to expropriate plots in certain localities, and we interpret this response as an attempt to avoid an uprising (Acemoglu and Robinson, 2006).

The empirical strategy uses month-by-month invasions of plots and the number of expropriations across hundreds of agricultural counties in an event study research design. This strategy exploits the staggered appearance of collective actions after Salvador Allende rose to power in November 1970. Differences in the dates and locations of these actions allow us to control for unobserved heterogeneity by county and month using fixed effects. We estimate that the initial invasion triggered an additional 6–7 invasions in the following 12 months, and together these collective actions induced an additional 2–3 expropriations during the same period, an increase of almost 40 percent. This increase cannot be explained by the displacement of expropriations from the future. Moreover, invasions seem to have increased the intensity of the program as the total number of hectares expropriated increased by 20 percent.

In the second chapter, I analyze political favoritism in the implementation of big public infrastructure projects. To investigate how accountability may constrain favoritism, we study multiple potential channels within a single context: Kenya's Last Mile Connectivity Project. Launched in 2008, this large-scale and politically high-profile public investment aims to provide universal household electricity access by 2022.

We find evidence of substantial political favoritism in the Last Mile Connectivity Project. Wards that voted pro-government in the 2013 Presidential election have 491 meters per

100,000 people, compared to just 357 per 100,000 in opposition wards, a gap of 37.4%. We analyze the sources of this bias by decomposing the connection process into distinct stages of construction, starting with the initial stock of transformers. Pro-government wards started with 19%-38% more transformers per capita than wards that voted for the opposition, potentially due to the fact that this stage was funded by the government of Kenya and therefore experienced little donor oversight. We find a small degree convergence between pro-government and opposition wards in the share of transformers selected for maximization under LMCP, which may have been due to the fact that this process was subject to strict donor requirements and that the lists of sites to be included was scrutinized and publicized in Kenyan media. However, this effect is not large enough to offset the inequality of the initial distribution: the aggregate result is that 16-19% more LMCP transformers are selected per capita in pro-government wards than in opposition wards. This gap in the number of sites selected is further exacerbated by greater construction and stringing progress in pro-government wards, which was difficult to track in part because construction was implemented by dozens of independent private contractors, and is not closed in the provision of meters. The end result is a sizable gap in the number of households connected to electricity between pro-government and opposition wards.

In the third chapter, I evaluate the effect of a large urban transportation infrastructure investment on the labor market. In particular, we study the expansion of the subway network in Santiago, Chile, between 2002 and today, focusing on the effect this has on the labor market power that firms have over workers.

First, we test if transit infrastructure gives workers better job opportunities and more bargaining power, due to the improved access to labor markets in the city. We compare areas affected by the network expansion to areas that were not affected through a panel event-study leveraging the opening of 84 new subway stations. By combining administrative data on monthly earnings from an unemployment insurance database, and data on the residence location of each worker and the business location of each firm, we obtain reduced-form estimates of the effects of improving market access on wages and work locations. Because we include worker and firm fixed effects in our event-study regressions, our estimates of the effects of infrastructure are net of any sorting caused by the treatment. Our reduced-form estimates reveal four facts that then motivate our model: 1) After the subway network expands to connect an additional district, workers that experience an improvement in market access commute longer distances and earn higher wages. 2) After the subway network expands to a district, even workers who live in that district and do not switch jobs start earning more. 3) After the subway network expands to a district, firms in that district start hiring workers from farther away, but pay the same wages on average. 4) Expansions of the subway network lead earnings to converge across space. Specifically, firms start paying workers wages closer to their sector-education-age group average wage after the subway connects the district where the firm is in.

Second, we build a quantitative spatial equilibrium model in which workers commute and firms exert labor market power over workers. The model serves two purposes: One, it allows us to disentangle the channels behind the reduced-form estimates, and two, it provides a tool to quantify the infrastructure expansion's effect on welfare. The model is based on Monte, Redding, and Rossi-Hansberg (2018) and Berger, Herkenhoff, and Mongey (2019). Its main assumption is that firms behave as oligopsonies in the labor market.[1] With model estimates at hand, we quantify the economic impact of transit improvements considering two channels. First, we measure the efficiency gains from the infrastructure expansion, accounting for the direct benefits of reducing commuting costs and the indirect effects from changing labor market power. Second, given that one of the biggest concerns of economists is the rise of market power and inequality, we measure the effects on the distribution of welfare between firms and workers. The aggregate impact of the infrastructure expansion on firm's labor market power can go in either direction. On the one hand, as labor markets become more integrated, more competition among firms for workers reduces labor market power. On the other hand, larger firms may become bigger, increasing their wage-setting ability.

[1] The assumption of oligopsonies is similar to assuming different Nash-Bargaining parameters in search models (Manning, 2021).

Chapter 1

Collective Action and Policy Implementation: Evidence from Salvador Allende's Expropriations

1.1 Introduction

The Cold War motivated the design of U.S. sponsored re-distributive policies in Latin America to fight communism, diminish the influence of the Soviet Union, and avoid the appearance of a "second Cuba" (Taffet, 2007).[1] Among these efforts, agrarian reform programs were one of the most important. More than 40 million hectares were expropriated in Brasil, Bolivia, Chile, Colombia, Ecuador, Guatemala, Mexico, Peru, and Venezuela (Albertus, 2015). Despite their relevance, there has been little empirical attention to how these policies were implemented on the ground. Studying how expropriations took place is not only important to understand the economic impact of land reform programs across the American continent; it also reveals the potential effectiveness of these international policies as tools of political influence during the Cold War.

This paper shows that organized groups of workers affected the intensity of one of the largest policies of the time: Salvador Allende's land reform in Chile (1970-1973). After being elected president in a contentious election, Allende attempted to create a "democratic road to socialism" and used the existing land reform program to expropriate more than six million hectares with the goal of empowering agricultural workers. In this context, groups of workers exerted pressure to radicalize policies and accelerate the transition to socialism. We show that the collective actions of workers influenced the government to expropriate plots in certain localities and we interpret this response as an attempt to avoid an uprising

[1] The program began with John F. Kennedy and the *Alliance for Progress* in 1961. The U.S. invested 20 billion dollars, from a total of 80 billion to be spent, during a ten-year period. See Taffet (2007) for economic details of the program, Darnton (2012) for a discussion about its origins, and Lowenthal (1991) for country case studies.

(Acemoglu and Robinson, 2006).

Chile is an interesting case study for several reasons. Historically, the pressure from radical groups has been suggested as one of the causes behind the economic collapse of Allende's government and the 1973 coup that followed (Boorstein, 1977; Sigmund, 1977). We provide novel evidence of the policy agenda responding to collective actions partly organized by the radical left. Institutionally, the entity in charge of the land reform program kept records of all expropriated plots, allowing us to observe the location and date of expropriations. The collective actions of workers, as measured by land invasions, are also well documented in police reports with their exact locations and dates. These invasions reveal that the pressure from workers to radicalize policies began to appear at different points in time across the country. We combine all these data to construct a panel dataset of counties observed monthly during the government of Salvador Allende.

The empirical strategy uses month-by-month invasions of plots and the number of expropriations across hundreds of agricultural counties in an event study research design. This strategy exploits the staggered appearance of collective actions after Salvador Allende rose to power in November 1970. Differences in the dates and locations of these actions allow us to control for unobserved heterogeneity by county and month using fixed effects. We estimate that the initial invasion triggered an additional 6-7 invasions in the following 12 months and together these collective actions induced an additional 2-3 expropriations during the same period, an increase of almost 40%. This increase cannot be explained by the displacement of expropriations from the future. Moreover, invasions seem to have increased the intensity of the program as the total number of hectares expropriated increased by 20%. These results are robust to the removal of counties without invasions and counties with a first invasion within three months of Allende's rule. We also obtain similar results if we allow for a demanding specification with time shocks across clusters of nearby counties and if we control for the availability of large plots.

Why was the government responding to the collective actions of workers? The answer is far from obvious. One explanation is that Allende's government colluded with groups of workers to organize invasions and thus create a legal justification to expropriate these plots.[2] Although invasions were *not* a legal reason to expropriate, these actions could have exerted pressure for the landowner to offer the plot. An alternative interpretation of results is that radical groups were threatening with a revolt and expropriations were implemented as an attempt to prevent uprisings. Historical and empirical evidence suggests the latter interpretation is relatively more important in the context of Salvador Allende's government. Radical political groups to the left of the coalition in power encouraged and assisted workers to invade plots, creating a "threat to the government's commitment to legality and controlled change" (Winn and Kay, 1974, p. 141).

We end our empirical analysis by exploring whether the displacement of expropriations can explain our findings. Event study estimates reflect within-country comparisons and

[2] In fact, there is some evidence of political parties coordinating land invasions with the goal of acquiring land in the context of a land reform program in Italy (Percoco, 2019).

thus the aggregate effect is confounded by a potential displacement of expropriations across locations. We assume that displacement occurs across nearby counties and estimate a conservative displacement rate of 38%. Using this number we calculate that 6-10% of Salvador Allende's expropriations would not have taken in the absence of the collective actions of workers.

Our primary contribution is to the empirical literature that documents the causes and consequences of social conflict and collective actions more generally (e.g. Acemoglu and Robinson 2006; Blattman and Miguel 2010). Previous research has shown how protests and riots can affect the formation of political movements, political preferences, and the work of incumbent politicians (Madestam, Shoag, Veuger, and Yanagizawa-Drott, 2013; Aidt and Franck, 2015; Larreboure and González, 2020). Other research provides insights into why individuals participate in collective actions when there are private costs and the benefits are common to the group (e.g. Cantoni, Yang, Yuchtman, and Zhang 2019; Manacorda and Tesei 2020; Enikolopov, Makarine, and Petrova 2020; González 2020).[3] In contrast to these studies, we focus on the role of land invasions in shaping the policy agenda of a left-wing government during the Cold War. As emphasized by Downs (1972), public attention can be affected by the collective actions of pressure groups and shape the policy-making process, but empirical evidence is scarce. We contribute to this literature by showing empirically how organized groups of workers affected the redistribution of assets in the context of a large land reform program in Latin America.

The re-distributive nature of land reform makes this paper also related to a literature studying the extension of voting rights under the threat of revolution (e.g. Acemoglu and Robinson 2000, 2006; Aidt and Franck 2015). A collection of results suggest that elites can choose to extend voting rights strategically in order to prevent an uprising, a process of enfranchisement that can also be interpreted as an increase in re-distributive policies (Meltzer and Richard, 1981). In contrast to previous research, we exploit month-to-month frequency of expropriations to emphasize that collective actions can also serve as revolutionary threats and affect the intensity of a policy.

Land reform programs across the world have also received a significant amount of attention from scholars. Previous research has suggested that collective actions affected the *redistribution* of plots in Mexico, Colombia, and Italy (Dell, 2012; López-Uribe, 2019; Percoco, 2019). However, that research uses mostly cross-sectional analyses and it does not differentiate between expropriation and redistribution of plots. As a consequence, the effect of collective action on the intensity of this policy has been difficult to establish. In contrast, we exploit the timing in which collective actions appear using relatively high-frequency data and emphasize the interactions between the policymaker and potential beneficiaries in a highly politicized context.[4] Finally, the study of land invasions is relatively more scarce and

[3] There is also a rich theoretical literature emphasizing the informational role of group actions and when this information can be used by the policymaker and influence voters (Lohmann, 1993, 1994; Battaglini, 2017).

[4] An extensive literature has estimated the effects of land reform and expropriations. See Besley and Burgess (2000), Ghatak and Roy (2007), Albertus and Kaplan (2012); Albertus, Diaz-Cayeros, Magaloni, and Weingast (2016b), Fetzer and Marden (2017), Pino (2018), Uribe-Castro (2019), Montero (2020) among others,

emphasizes the role of economic conditions in driving these actions, particularly in contexts of high inequality (Hidalgo, Naidu, Nichter, and Richardson, 2010; Albertus, Brambor, and Ceneviva, 2016a).

1.2 Historical Background

Land reform and Salvador Allende

Chile's land reform program began in 1962 shortly after the creation of the Alliance for Progress, an economic program between the U.S. and Latin American countries to prevent a "second Cuba" in the region (Wright, 2000; Taffet, 2007). An institution named Corporation of Agrarian Reform (*Corporación de Reforma Agraria*, CORA) was in charge of the process. The original program contained a limited number of legal causes to expropriate a plot and thus only a few plots were expropriated during Alessandri's right-wing government (1958-1964). After a second land reform law was enacted in 1967, which allowed CORA to expropriate "large" or "inefficient" plots, president Eduardo Frei (1964-1970) was able to increase expropriations (Loveman, 1976). The land reform process of a plot began with the expropriation, continued with "asentamientos" (settlements), and ended with the redistribution of the plot. An asentamiento was a transitory collective exploitation of the land under the advice of the state, which acted as the partner, and their goal was to give workers enough time to learn and organize the production process.[5] In the last step of the process the land would be assigned to individuals or communitarian properties.

The expropriation of plots increased significantly under Salvador Allende (November 1970-September 1973). Allende rose to power after a contentious election in which he got 36.6% of the vote running under the umbrella of a left-wing coalition known as Popular Unity (U.P. in Spanish).[6] The land reform program was a crucial part of Allende's policy platform during the 1970 presidential election. The program of the U.P. reveals the pillars of his plan: to nationalize all strategic and large companies, regulate prices, increase the wages of workers, and increase the intensity of expropriations in the context of the existing land reform program (Popular Unity, 1969). These policies had the goal to create a "democratic road to socialism." The land reform process remained largely unchanged, with small changes such as the replacement of asentamientos by Agrarian Reform Centers (*Centros de Reforma Agraria*, CERA) from mid-1971 onwards (Loveman, 1974, p. 152). All in all, the first half of Allende's government was relatively successful but the second half was characterized by an economic collapse and social unrest (Boorstein, 1977, p. 111).

and González (2013), Lillo (2018) for the case of Chile.

[5] The original idea was to create a "joint enterprise in which the workers provided their labor and the CORA the land, technical assistance, credit, and operating capital [...] the value of these inputs would be returned [and] the remainder of any surplus would be distributed among the workers." (Loveman, 1974, p. 150)

[6] Recently declassified documents reveal that Richard Nixon attempted to prevent his confirmation at the Congress (Kornbluh, 2003; Qureshi, 2009). For more details about the land reform program see Garrido (1988); Huerta (1989); Bellisario (2007a,b) and Valdés and Foster (2015).

Chile's experiment with socialism ended with a U.S. backed coup in September 1973 followed by a seventeen-year dictatorship that returned some previously expropriated plots (Qureshi, 2009). The relative contribution of internal versus external forces behind the fall of Salvador Allende remains debated. For example, Fidel Castro famously stated that "the Chilean experiment was failing because of Allende's reluctance to become 'more radical"' (Davis, 1985, p. 44). Internal forces came from left-wing groups and included included strikes, occupations, and land invasions (Haslam, 2005, p. 97). External causes included a U.S. "invisible economic blockade" propelled by president Richard Nixon to "make the [Chilean] economy scream" (Kornbluh, 2003, p. 83).

Land invasions as collective actions

Land invasions were a key characteristic of the countryside during Allende's rule. A number of historians have documented these invasions using case studies from different regions of Chile (e.g. Sánchez 2012; Redondo 2015; Robles-Ortiz 2018). The most common interpretation of these collective actions is that they generated pressure from the countryside to increase the intensity or "radicalize" the land reform program (Kay, 1977; Robles-Ortiz, 2018; Navarrete, 2018). Scholars also emphasize the importance of Allende's victory to increase the overall intensity of invasions, and the acquisition of land rights as invaders' main objective (Bravo, 2012; Redondo, 2015).

Why did peasants use land invasions as a strategy to improve their economic conditions? Peasants began to invade plots because landowners learned to simply replace workers during traditional strikes, and invasions prevented them from doing so (Bengoa, 1972). In addition, a change in workers' demands was key, which moved from demanding better labor conditions to demanding ownership in the context of the agrarian reform (Redondo, 2015, p. 159). The increasing demand for land ownership was at least partially explained by the importance of land reform as a policy during political campaigns on the eve of the 1970 presidential election (Petras, 1971). Moreover, scattered information about specific invasions suggests that invaders were workers from the same plot (Kay, 1977, p. 868), who in the case of an expropriation were likely to have been a part of the later asentamiento and thus the beneficiaries of the land reform program.

Invasions were usually non-violent acts in which workers took control of a property's entrance, typically setting up a camp at the main gate (Robles-Ortiz, 2018). An example comes from the chronicles of American journalist Norman Gall:

> "[the invasion] of the Tres Hijuelas farm came just a few weeks after the inauguration of the Marxist *Unidad Popular* regime of President Salvador Allende, and was the visible beginning of the present wave of peasant insurrection (...) families from the neighboring *Reducción Alhueco* quietly threaded their way across the wheat fields of Cautín Province in southern Chile to pitch crude tents of wheat sacks and old blankets under a hillside cluster of eucalyptus trees on the farm (...) posting guards at the deserted clapboard farmhouse of the Fundo Tres Hijuelas

> – the Owner, Carlos Taladriz, lived in the neighboring town of Lautaro and was away in Santiago at the time – as well as at the machine shed, at the roadside entrance to the farm and at the bridge of planks that crossed over a small stream to the house. The only persons living on the 1,250-acre farm at the time were a shepherd and a tractor driver."

An important question to understand the timing and intensity of land invasions is how were agricultural workers able to solve the collective action problem. This is a hard question to answer but we hypothesize that the 1967 unionization law was an important factor. This law effectively allowed workers in rural areas to collectively bargain to improve their labor conditions and therefore increased the benefits of collective action. Accordingly, unionization numbers began to rise after the enactment of this law. When Allende took office 140 thousand rural workers were unionized, and another 100,000 organized in cooperatives. Moreover, union membership grew by 50% during Salvador Allende's first year in office (Gómez and Klein, 1972).

Historical accounts support the idea that unions were instrumental for invasions. The majority of workers who participated in unions lived in rural estates, and were therefore better off than seasonal workers. Politically, unions supported the Christian democrats, but they became more radicalized after Allende's victory (Winn and Kay, 1974). The work of Robles-Ortiz (2018) provides a clear example of how unions were linked to land invasions:

> "the local *miristas* [left-wing radicals] decided to take over the Neltume estate [...] thus challenging the Popular Unity. The clash took place in the labour union assembly, which voted in favour of taking control of Neltume. The toma took place on December 9, 1970. It was carried out by some 390 workers 'with the support of two extremists' who were 'university students and members of the MIR [left-wing radical movement]'."

Another example comes from a plot in the city of Melipilla where the local union organized an invasion with workers from nearby plots (Kay, 1977, p. 868). According to these investigations, workers and members of the radical left routinely engaged with unions and together led invasions, even though beneficiaries were likely to have been previous workers of the plot.

1.3 Data and Descriptive Evidence

This section describes the data sources we use to measure these historical processes. We then explain how we constructed the panel data used in the empirical strategy and offer a comparison of counties with and without invasions.

Land reform files and invasions

To measure the intensity of the land reform program we use historical files documenting the universe of expropriations. The Corporation of Agrarian Reform was in charge of expropriations and kept administrative records of the entire process. The original data consists of 5,800 files, each one describing an expropriation in a two-sided sheet. The description includes the exact date of expropriation (month and year), the county in which the expropriated plot was located (there were 280 counties), the size of the plot in hectares, and the legal cause used to justify the expropriation.

Table 1.1 presents descriptive statistics that confirm the overall intensity of the program, and the legal causes used, across the three governments of the time. This table makes it clear that expropriations were very intense during the Allende years. Using the 1965 agricultural census as a benchmark we calculate that 2% of the total number of plots was expropriated during this period (4,298 plots), which constituted 20% of all agricultural hectares in the country (6.2 million hectares). Half of these plots and agricultural land was redistributed. Empirically, the three most important legal causes used by the corporation to expropriate plots were: (i) the plot was larger than 80 hectares, (ii) the plot was abandoned or inefficient, and (iii) the plot was offered by the owner. Under Allende's government these causes explain more than 90% of expropriations.

Our work uses countrywide data during the Allende years, combined with the exact dates of expropriations, to study the intensity of this policy. Previous research has studied the land reform program regionally (Robles-Ortiz, 2018), the long-run effects of redistribution (Cuesta, Díaz, Pino, González, and Marshall, 2017; Lillo, 2018), and the political impacts of Eduardo Frei's policy (González, 2013). The study of the intensity of this policy at the micro level can lead us to reinterpret the long-run impacts and to put regional studies into a more general historical perspective. Figure 1.1-A presents the number of expropriations by month, revealing the stark differences between the two halves of Allende's government. Similarly, this is the first effort to combine the land reform files with land invasions data and unions in a countrywide dataset of counties observed monthly.

We also digitized the universe of recorded land invasions during the Allende years, which reveals new historical patterns. We measure the exact location and time of land invasions using data from police reports that were published by the Chilean Congress in May 1972 as part of Ordinary Session V in which the state of the countryside was discussed. The origins of the data can be found in allegations of a congressman who accused the government of orchestrating these invasions to intensify the land reform program (National Congress of Chile, 1972, p. 270-290). After several rounds of discussions with the Ministry of Agriculture, the congressman mandated the Ministry of the Interior to construct a registry with all the invasions. The police wrote this report, which they sent to the congress, generating a discussion about invasions, expropriations, and the role of the government. We account for the inherent reporting bias in these reports by using county-level fixed effects.

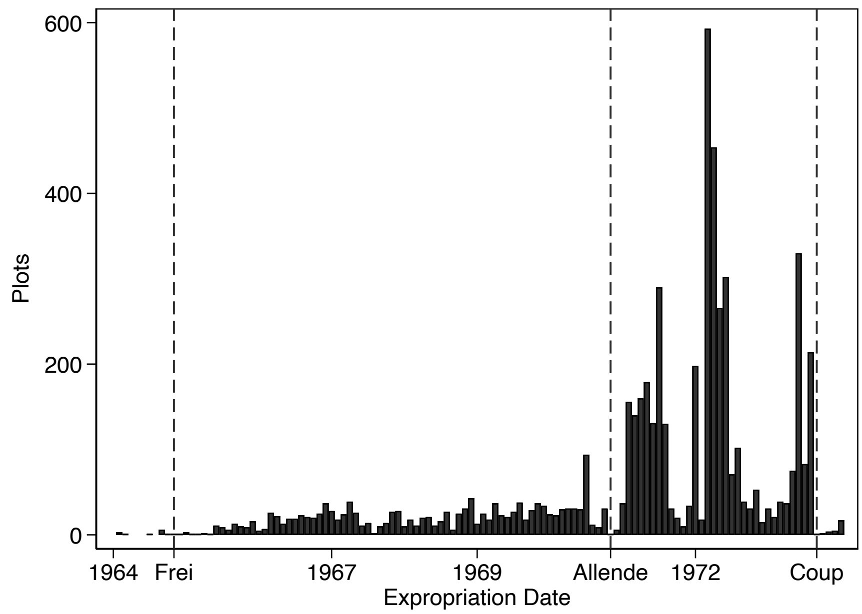
Although previous research has used qualitative information from the reports as part of regional studies (Sánchez, 2012; Redondo, 2015), the universe of the data in this report has never been used before to construct a national study. Moreover, a quantitative analysis of these invasions and their relation to expropriations has been notably absent. The report includes 1,747 land invasions happening between November 1970 and April 1972 with the *county* in which each one took place. We complement these data with the number of invasions

Table 1.1: The land reform program under different governments

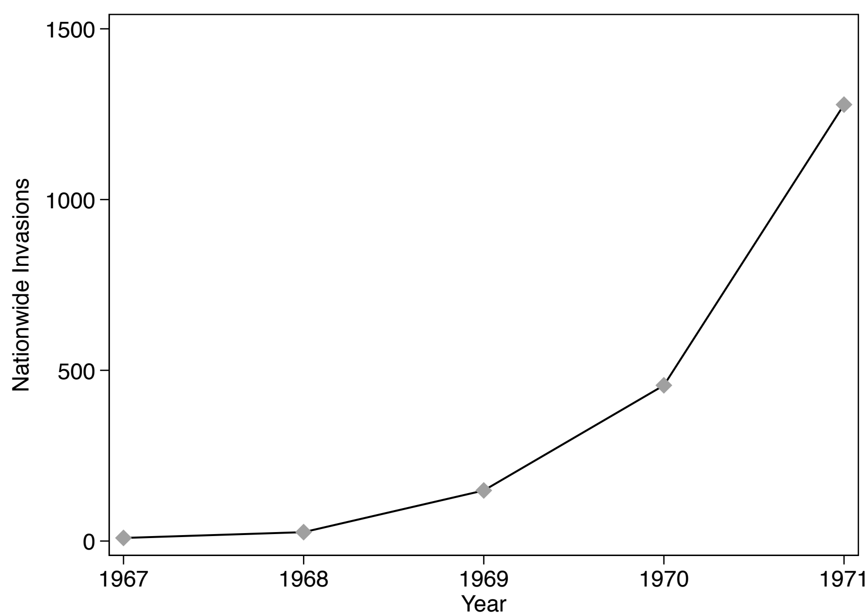
	Jorge Alessandri (1958-1964)	Eduardo Frei (1964-1970)	Salvador Allende (1970-1973)
	(1)	(2)	(3)
Number of plots expropriated	21	1,436	4,298
% of agricultural plots in 1965	<0.1%	<0.1%	2%
Number of plots redistributed	16	1,188	2,447
% of expropriated plots	76%	83%	57%
Number of hectares expropriated	137,838	3,948,253	6,193,851
% of agricultural hectares in 1965	<0.1%	13%	20%
Number of hectares redistributed	120,813	2,922,977	3,050,984
% of expropriated hectares	88%	74%	49%
Number of land invasions	0	501	1,720
Legal causes to expropriate			
Plot was divided in 1965-1967	0%	6%	0%
Plot can serve social purpose	0%	0%	0%
Plot is larger than 80 hrb.	14%	25%	46%
Plot abandoned or inefficient	0%	0%	21%
Plot is large and was divided	0%	2%	0%
Plot owner is legal person	0%	5%	7%
Plot has multiple owners	0%	0%	2%
Plot was offered by owner	5%	26%	22%
Plot expropriated before 1964	0%	7%	0%
Unknown	81%	29%	1%

Notes: This table presents descriptive statistics of land expropriations under different governments. The upper panel describes the total number of plots and hectares expropriated, together with the number of land invasions. The lower panel present the legal causes used to expropriate plots.
Source: Land reform data files.

Figure 1.1: Chile's agrarian reform and land invasions



(a) Number of plots expropriated



(b) Number of land invasions

Notes: Panel (a) presents the number of plots expropriated by month between January 1964 and December 1973. Panel (b) presents the number of land invasions per year between 1967 and 1971.
Source: Land reform data files and police reports of land invasions.

by *province* before Allende reported in Klein (1972). Provinces are larger administrative units than counties, so we employ counties throughout the analysis but complement it with province data when needed. Figure 1.1-B presents the number of land invasions per year. Taken together, all of these sources confirm that most invasions took place under Allende's government (1,700 of 2,200), although the increase in invasions began before his government, a pattern that has not been recognized before and that we hypothesize is related to the 1967 unionization law. Figure 1.2-A presents the number of invasions per month, revealing a significant amount of persistence and variation in their intensity throughout this period.

The importance of unions

We hypothesize that the historical origins of invasions can be found in the 1967 unionization law previously described. As a consequence of this law the number of unions spread rapidly throughout the country. We digitized data on the number of *sindicatos* (unions) by county from a registry originally constructed by Gómez and Klein (1972) to understand the state of local organizations. The authors define their work as a census derived from their collaboration with the Institute for Agricultural Development, an entity created by the agrarian reform law which operated under the umbrella of the Ministry of Agriculture. The goal was to "develop a global quantitative report of public use that serves as a guide for workers in the agricultural sector" (p. 1, own translation). This census was implemented between the last week of January and the first week of February of 1972. Most of these unions met weekly or monthly.

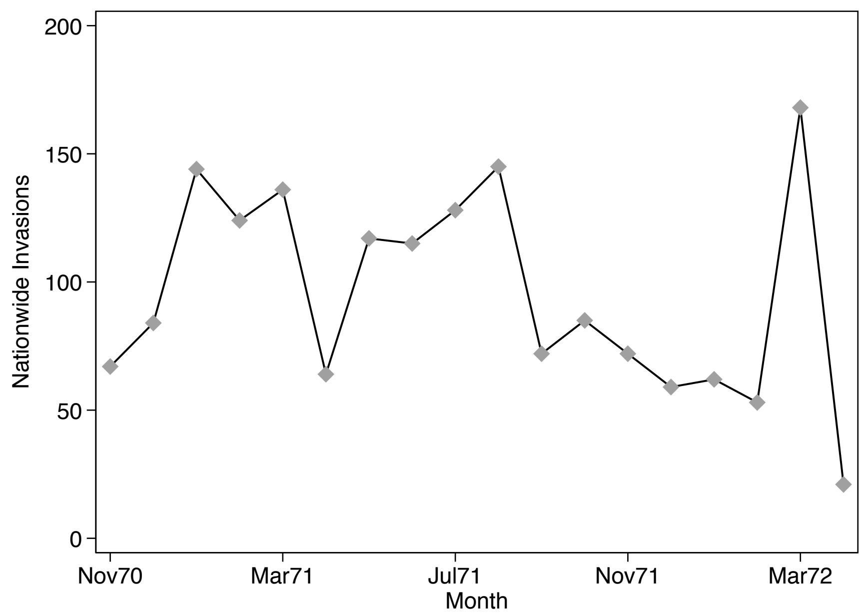
These data support the existence of a link between unions and invasions. Figure 1.3-A shows that there is a positive partial correlation between unionization per county and invasions. Moreover, Figure 1.3-B shows a similar province-level relationship between these variables in the period 1967-1970. That is, unions seem to have helped to coordinate invasions, and this suggestive evidence appears both during Allende but also before. This evidence is revealing as most previous studies argue that it was the election of Allende that triggered invasions. This and previous patterns suggest that his election could have accelerated this process, but invasions and their foundations were there before his arrival. Appendix Figure A.1 and Table A.1 add control variables to this analysis to show that this is a robust correlation. Overall, we interpret these patterns together with historical accounts as suggestive evidence consistent with our hypothesis regarding the importance of this law.

Descriptive statistics

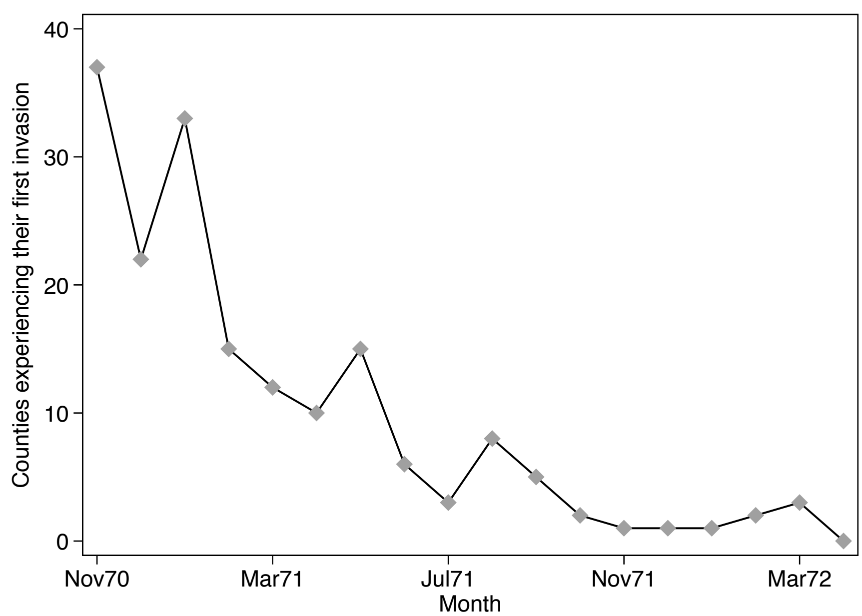
We constructed a panel of 221 counties observed between November 1969 and December 1973 for a total of 11,050 county-month observations.[7] A county enters our final sample if it experienced at least one occupation or one expropriation during this period. There are

[7] Land invasions data only spans the period between November 1970 and April 1972, but we add expropriation data before and after these dates to improve our event study design described in the next section.

Figure 1.2: Land invasions during Salvador Allende's government



(a) Land invasions by month

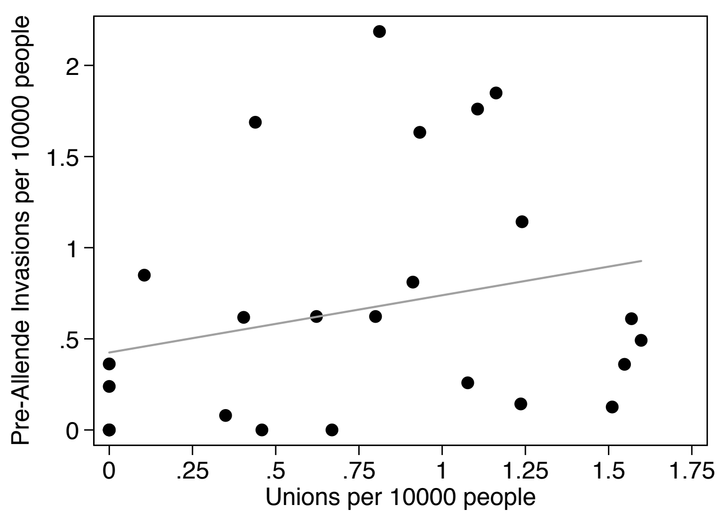


(b) Counties experiencing first invasion

Notes: Panel (a) presents the number of land invasions per month from the first month in which Salvador Allende held office until the last month with data on invasions. Panel (b) presents the number of counties experiencing their first land invasion.
Source: Police reports of land invasions.

Figure 1.3: Land invasions and the 1967 unionization law

(a) Unions and invasions *during* Allende's government (1970-73). County-level relationship.



(b) Unions and invasions *before* Allende's government (1967-70). Province-level relationship.

Notes: Panel (a) presents a bin scatter plot and linear fit between the number of land invasions per 10,000 inhabitants in the period 1970-1972 (y-axis) and the number of unions per 10,000 inhabitants (x-axis) at the county level. Panel (b) presents a scatter plot and linear fit between the number of land invasions peer 10,000 inhabitants in the period 1967-1970 (y-axis) and the number of unions per 10,000 inhabitants (x-axis) at the province level. Data on invasions before 1970 is only available at the province level.
Source: Police reports of land invasions and data from Gómez and Klein (1972).

176 (80%) counties with at least one invasion and 45 counties (20%) with zero invasions but at least one expropriation. Counties without expropriations and invasions host mostly urban centers or very small towns. Appendix Figure A.2 presents a map of the country with expropriations, invasions, and the final sample of counties. The average county in the final sample experienced 8 land invasions between November 1970 and April 1972, i.e. 0.43 invasions per month or 2.6 invasions every 6 months. A total of 12 plots were expropriated in the average county, i.e. one every two months.

We also use data from the 1955 and 1965 agricultural censuses originally digitized by Cuesta, Gallego, and González (2015). From this data we obtain measures of agricultural production at the county level, a land inequality measure (gini), the number of agricultural workers, agricultural equipment, and plot sizes. Although we cannot combine the agricultural censuses with expropriations data at the plot level, we can do this at the county level. The census data provides us with a baseline measure of the state of the agrarian economy at the local level before the land reform process and invasions began. We also digitized electoral outcomes from the 1970 presidential election.

Table 1.2 offers a comparison of these variables across counties with and without invasions. Columns 1 and 2 present the average and standard deviation. Column 3 presents the difference between averages in previous columns and its statistical significance. Counties that experienced invasions have on average more plots and more agricultural workers. Although at the time Chile exhibited high inequality and volatile economic conditions, counties with and without land invasions had similar economic characteristics, as measured by inequality in land property rights and productivity per hectare or worker. Similarly, both types of counties had experienced the agrarian reform similarly until 1969 and were located at the same distance of the capital.

In terms of political affiliation and organizational characteristics, the two groups of counties exhibited similar political support for Allende in the 1970 presidential elections and similar political participation as measured by total votes over population in 1970. Finally, the number of social organizations per 10,000 inhabitants formed before Allende's government is slightly higher in counties with invasions but the difference is not statistically significant at conventional levels.[8] All in all, we conclude that the two sets of counties were somewhat different, reinforcing the importance of using county-level fixed effects to account for these differences.

1.4 Empirical Strategy

To estimate the effect of the collective actions of workers on expropriations of agricultural plots, we use an event study research design. This method is a generalization of a difference-in-difference model in which the "treatment" occurs at different points in time and was popularized by financial economists (Campbell, 1997). Crucial in this methodology is the

[8] These organizations include any non-profit group registered in the official state institution. Examples of these are sport and social clubs, neighbors' organizations, and religious groups.

Table 1.2: Description of counties before Allende's government (1970-1973)

	Counties with invasions	Counties without invasions	Difference	Month of first invasion (avg. 4.7)
Agriculture before 1970	(1)	(2)	(1) − (2)	(4)
Number of agricultural plots	1,126	733	393***	0.7
	(861)	(449)	(133)	(0.9)
Hectares in agricultural plots	20,259	13,993	6,266**	-0.6
	(19,238)	(14,890)	(3082)	(0.6)
Agricultural workers	3,961	2,259	1,701***	-0.5
	(3,085)	(1,177)	(469)	(0.6)
Land gini	0.97	0.97	-0.002	-0.7*
	(0.02)	(.03)	(.004)	(0.4)
Productivity per hectare†	118.3	126.6	-8.3	1.5
	(108.9)	(243.7)	(24.4)	(0.9)
Productivity per worker†	793	883	-90	-0.2
	(793)	(1905)	(185)	(0.6)
Agrarian reform until 1969	0.09	0.05	0.04	-0.02
	(0.16)	(0.1)	(0.02)	(0.3)
Province-level invasions$^{\nabla}$	–	–	–	-0.1
				(0.1)
Other variables				
Distance to Santiago (in km.)	387	389	-2	-3.3
	(350)	(439)	(62)	(2.9)
Distance to regional capital (in km.)	107	141	-34*	-0.2
	(116)	(140)	(20)	(0.6)
Vote share Salvador Allende in 1970	0.33	0.35	-0.02	0.9**
	(0.11)	(0.14)	(0.02)	(0.4)
Turnout in 1970	0.26	0.26	0.00	-0.3
	(0.12)	(0.13)	(0.02)	(0.2)
Social organizations per 10,000 inhab. in 1970	6.2	4.7	1.5	0.1
	(13.8)	(7.7)	(2.1)	(0.1)
Counties	176	45		

Notes: Descriptive statistics for rural counties in Chile. Column 1 describes counties with at least one invasion during Allende's government, column 2 describes counties without invasions, and column 3 presents the difference between columns 1 and 2. Column 4 presents coefficients from a cross-sectional regression using the month of first invasion as dependent variable (month 1 is November 1970, month 18 is April 1972) and (standardized) variables and region fixed effects as predictors. The average of the month of first invasion is 4.7 and its standard deviation is 3.8. Standard deviations in parentheses in columns 1-2 and standard errors in columns 3-4.†Measured in thousands of Chilean pesos.$^{\nabla}$Comes from a separate regression using province-level invasions before 1970 and provinces as units of observation. Statistical significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: Land reform data files, 1965 Agricultural Census, police reports of land invasions, Electoral Service, and Civil Registry.

definition of the "event" (i.e. the treatment) to be studied. We define the event as the *first* invasion of a plot after November 1970, when Allende rose to power. Although we could use *any* invasion as an event, first invasions were relatively more unexpected while subsequent invasions were not and thus by focusing on the former we are able to minimize potential anticipation effects. An example of this comes from an important agricultural region in the south of the country, where the first wave of invasions "took Panguipulli by storm in the summer of 1971" (Robles-Ortiz, 2018, p. 13). Importantly, invasions could have been part of a "package" of political actions unobserved to us and therefore we interpret invasions as a proxy for the collective actions of workers. Figure 1.2-B plots the number of first invasions by month.

Motivated by the previous observations, we centered the data around first invasions and focus on the months before and after these events, which allows us to control for county- and month-level unobservable variables by using fixed effects. The strategy effectively exploits the *timing* in which invasions began to appear in different parts of the country. We begin by using a semi-parametric version of this strategy and estimate the following regression equation by ordinary least squares:

$$Expropriations_{ct} = \sum_{k=-12}^{12} \beta_k D_{ct}^k + \gamma_c + \lambda_t + \varepsilon_{ct} \tag{1.1}$$

where D_{ct}^k are a set of indicators for the months before and after the first invasion in a county, e.g. D_{ct}^1 is equal to one in county c in month t only if the first land invasion took place in the previous month. In addition, γ_c and λ_t are a full set of county and month fixed effects, which control for unobserved time-invariant differences across counties and temporal factors affecting all counties. The former accounts for the fact that some counties are simply more exposed to land reform because of, for example, their economic structure, and the latter for reasons such as the arrival of a socialist government increasing the probability of expropriations. The error term ε_{ct} has a mean of zero and we allow it to be correlated within counties over time.

The coefficients of interest are $(\beta_{-12}, \beta_{-11}, \ldots, \beta_{12})$ and measure the change in expropriations in the twelve months before and after the first invasion of a plot under Allende's government.[9] Operationally, the indicator D_{ct}^0 takes the value of one in the month of the first invasion and we omit the indicator D_{ct}^{-1} from equation (1.1). Therefore, the coefficients of all remaining indicators need to be interpreted relative to the month before the event. For example, if $\beta_1 > 0$ then there was increase in the number of expropriations in the following month after the first invasion, relative to the month before the event. In this sense, the coefficients β_k with $k \in [-12, -1]$ serve as a measure of the trend in expropriations in a county before it experienced the first invasion.

[9] In order to estimate the coefficients for the twelve months before the arrival of Allende and the twelve months after the end of the invasions data, we use the panel of expropriations from November 1969 until December 1973.

Equation (1.1) can be considered a fairly non-parametric estimate of how land invasions affected expropriations. As a complement, we also estimate the following parametric version:

$$Expropriations_{ct} = \beta D_{ct} + \gamma_c + \lambda_t + \varepsilon_{ct} \quad (1.2)$$

where D_{ct} takes the value of one for the twelve-month period after the first invasion and zero otherwise. Note that in this equation β captures the *average* change in expropriations in the months after the event, and we are also imposing that the coefficients before the event are zero. In this sense, this equation contains less information and more restrictions but it is nevertheless useful because it is a simpler model and it allows us to improve efficiency by estimating fewer parameters. All remaining variables in equation (1.2) are defined as in equation (1.1).

Column 4 in Table 1.2 presents suggestive evidence for the validity of our design. Our concern is omitted variables changing over time that affected the appearance of first invasions and expropriations. To check for this we estimate a cross-sectional regression using the month of first invasion across counties as dependent variable and a large set of predetermined variables as predictors.[10] Column 4 presents estimates and their standard errors using standardized predictors to facilitate their interpretation. In almost all cases a one standard deviation increase in a predictor has a small and statistically insignificant effect in the month of first invasion. Moreover, province-level invasions before Allende have little predictive power of the average month of first invasion in a province. All in all, the timing of first invasions appears unlikely to be driven by variables that affected expropriations.

Finally, we emphasize that there are modeling decisions when estimating equations (1.1) and (1.2). These decisions are important for both interpreting results and to check for their robustness. In the first place, we measure expropriations in different ways, including the total number of plots expropriated, the total number of hectares expropriated, and the percentage of hectares in the county that were expropriated, among others. In addition, when estimating equation (1.1) we can only consider first invasions during Allende's government because invasions by month are unavailable for other periods. As expected, many of the first invasions in the data occurred at the beginning of the new government. Thus in the following section we check if the dispersion of events has some effect on results. And third, given the observed differences between counties with and without invasions we estimate both equations using (i) all counties, and (ii) counties with at least one invasion.

1.5 Main Results

Using the previously described event study research design, this section shows that the collective actions of workers affected the intensity and location of expropriations. We then

[10]The month of first invasion takes the value of one if the first invasion was in November 1970, and increases by one each chronological month since that date until the value of 18 if the first invasion was in April 1972 (last month in our invasions data). The average of this variable is 4.7 and its standard deviation is 3.8.

present and discuss a battery of empirical exercises that suggest these results represent robust findings.

Invasions and local political actions

Figure 1.4-A presents estimates of β_k in equation (1.1), with their corresponding 95 percent confidence intervals, using land invasions as the dependent variable. The motivation to begin with this specification is that after the first plot was invaded by agricultural workers there might be more invasions and political actions afterwards. Testing for the dynamics of these collective actions is important to understand the event in our research design. The x-axis in this figure denotes the months relative to the first land invasion ($t = 0$) and the y-axis measures the change in the number of invasions. The coefficients to the left of the event represent invasions before the first invasion and are by definition equal to zero. The coefficients to the right measure the change in land invasions after the first one.

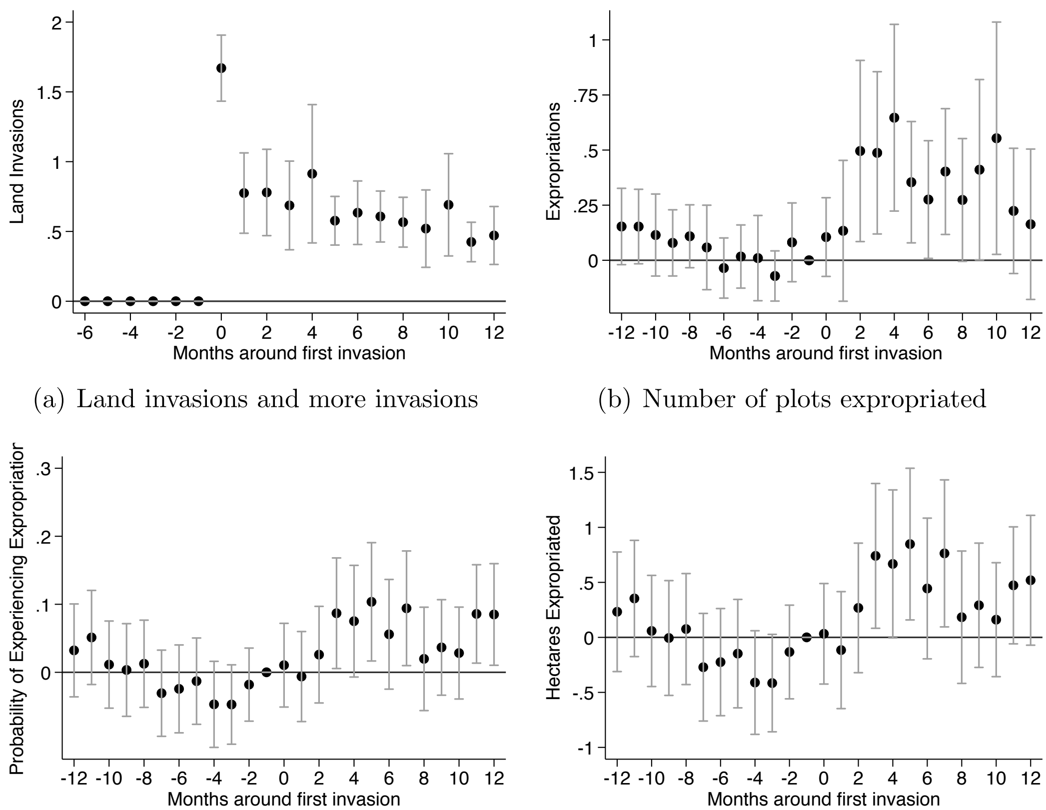
The estimated coefficients reveal that in the months following the first invasion in a county there are significantly more invasions within the same location. In particular, in the month of the first invasion there were on average 1.6 invasions. This is, it was usual that the first invasion came together with another invasion. This result is consistent with the notion that invasions were part of a package of political actions. Moreover, in the following six months we observe approximately four more invasions, an increase of approximately 150 percent over the sample average. The number of invasions within months 6 and 12 of the first invasion also increases, but in a smaller magnitude than in the first six months. Estimates of equation (1.2), the parametric version of the event study, show similar magnitudes and can be found in Table 1.3 column 1.

The dynamic pattern of land invasions across the country is important because it means that the majority of invasions were not randomly allocated across space and time. Indeed, invasions were significantly more likely to occur after the first one took place. There are multiple potential explanations for this pattern, including the diffusion of information, social effects, and packages of political actions. Regardless of the explanation, this result implies that when we study expropriations in the months after the event, the estimated coefficients represent the effect of multiple political actions which were triggered by the first one.

Expropriations

Figure 1.4-B presents estimates of equation (1.1). The omitted category is the month before the first invasion. These estimates show that the total number of plots expropriated in a county increased significantly after the first plot was invaded. All coefficients after the event are positive and most are statistically significant (p-values<0.05, except for the first and last two). By integrating coefficients, we calculate that there were on average 2-3 more plots expropriated within six months of the event. Given that all coefficients after the event are positive, the displacement of expropriations from months in the future to the present is unlikely to be an explanation behind our results. The number of monthly

Figure 1.4: Land invasions and expropriations



(a) Land invasions and more invasions

(b) Number of plots expropriated

(c) Probability of expropriation

(d) Hectares expropriated

Notes: These figures present estimates of equation (1.1) with their corresponding 95 percent confidence interval. Each panel uses one of four different dependent variables. Source: Land reform data files and police reports of land invasions.

expropriations increased by approximately 20% (Appendix Figure A.4-A). Similarly, the intensity of expropriations also increased between months 6 and 12 but in a relatively smaller magnitude. The effect of invasions appeared two months after the first invasion and peaked for about three months before slowly fading out.

Importantly, the number of expropriations did not exhibit a trend *before* the event. All coefficients before the first invasion hover around zero, are statistically insignificant at conventional levels, and the point estimates are of remarkably small economic magnitude. Our identification assumption is that in the absence of a first invasion the number of expropriations would have been similar, a counterfactual that in this case corresponds to other counties without (yet) a first invasion. Although essentially untestable, the absence of pre-trends before the study and the high-frequency of the data suggest this assumption is likely to hold.

Table 1.3: Land invasions and expropriations using an event study analysis

	Number of invasions	Number of plots expropriated	Indicador at least one expropriation	Number of hectares expropriated	Number of plots redistributed	Number of hectares redistributed
	(1)	(2)	(3)	(4)	(5)	(6)
Indicator for 12-month period after first invasion	0.58***	0.18***	0.025**	0.19**	0.18***	0.20**
	(0.06)	(0.07)	(0.01)	(0.09)	(0.06)	(0.08)
Counties	221	221	221	221	221	221
Observations	11,050	11,050	11,050	11,050	11,050	11,050
County fixed effects	X	X	X	X	X	X
Month fixed effects	X	X	X	X	X	X

Notes: Each column presents estimates of equation (1.2) – the parametric version of the event study methodology – using a different dependent variable. Each observation corresponds to a county-month pair in the period between 01/1970 and 04/1972. The number of hectares expropriated and distributed use the hyperbolic sine transformation proposed by Burbidge et al. (1988). Standard errors are clustered by county. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: Land reform data files and police reports of land invasions.

Similarly to the increase in the number of plots expropriated, Figures 1.4-C and 1.4-D show that the probability of a county experiencing at least one expropriation and the number of hectares expropriated also increased. In the former case we estimated our main equation using an indicator that takes the value of one if the county experienced at least one expropriation and zero otherwise. In the latter, we use the logarithm of hectares expropriated.[11] In the months following the first invasion the probability of a county experiencing an expropriation in a month increased by an average of 2-3 percentage points, with a peak of 8-10 percentage points within months 3-5, from a base of 17% in the sample average. The number of hectares expropriated increased by 21% in the average county with a peak of 70-80% again within months 3 to 5. In both cases the absence of statistically significant trends before and the fading out of expropriations after the sixth month remains as a characteristic of the estimates. As a consequence of these patterns, the average size of an expropriated plot increased (Appendix Figure A.4-B).

Table 1.3 presents estimates of equation (1.2) using the same four previous outcomes. This specification is a relatively more parametric version of equation (1.1) in which we constrain coefficients before the event to be equal to zero and estimate a single indicator variable for the period after the event. Then, the coefficient associated with the latter indicator captures the average increase in a single month. Column 1 shows that the first invasion was followed by 0.6 invasions each month. In column 2 we observe that there were an additional 2.2 plots expropriated within one year of the event (0.18×12 months), an increase of 27% over the annual average. Finally, column 3 shows the probability of experiencing at

[11] Because many counties experienced zero expropriations in a month, we use the hyperbolic sine transformation proposed by Burbidge, Magee, and Robb (1988), which in this case allows us to interpret coefficients as semi-elasticities.

Table 1.4: Legal causes used to expropriate plots after invasions

The dependent variable is the number of expropriations

	Plots expropriated under legal cause			
	Large plot	Abandoned or inefficient	Owner is legal person	Plot offered by owner
Panel A – Plots expropriated	(1)	(2)	(3)	(4)
Indicator for 12-month period after first invasion	0.13**	-0.02	0.01	0.05**
	(0.05)	(0.01)	(0.01)	(0.02)
Panel B – Hectares expropriated				
Indicator for 12-month period after first invasion	0.19**	0.00	0.04	0.10**
	(0.08)	(0.04)	(0.04)	(0.05)
Counties	221	221	221	221
Observations	11,050	11,050	11,050	11,050
County fixed effects	X	X	X	X
Month fixed effects	X	X	X	X

Notes: Each estimate and its standard error come from an estimation of equation (1.2). Panel A uses the total number of expropriations as dependent variable and Panel B the hyperbolic sine transformation of the total number of hectares expropriated. Different columns use expropriations under different legal causes. Each observation corresponds to a county-month pair in the period between 01/1970 and 04/1972 except otherwise noticed. Standard errors are clustered by county. Statistical significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: Land reform data files and police reports of land invasions.

least one expropriation increases by 2.5 percentage points in a given month and the number of hectares expropriated increased by 19% twelve months after the first plot was invaded. Table 1.4 shows that two-thirds of these expropriations used the legal cause of large plots, while one-third was a plot offered by the owner to the corporation. The remaining causes were barely used after an invasion took place.

Robustness of results

This section provides statistical exercises that check for the robustness of previous estimates. We begin by addressing the fact that most events took place at the beginning of Allende's rule. Then we show that results are unaffected by our specification decisions. We end the section by presenting and discussing more flexible specifications that account for unobserved heterogeneity over time across groups of nearby counties.

Half of the counties in our sample experienced a first invasion within three months of Allende's government. This dispersion of events could constitute a threat to the validity of our research design if unobserved time shocks in the beginning of the new government coincide with the location of counties experiencing a first invasion. An example of this is local elections held in April 1971, which could be driving the timing of expropriations. To test for this concern, we remove from the estimation all counties with a first invasion within three months of Allende's rule. This restriction ensures that the events are relatively spread throughout the period of study, minimizing concerns about unobserved time shocks. Column 1 in Table 1.5 presents results. The estimated coefficient is still positive, statistically significant, and of similar magnitude than when using the full sample. If anything, the point estimate is actually larger than before (0.23 versus 0.21). We conclude that the dispersion of events is unlikely to be driving results.

Approximately 20% of our sample of agricultural counties never experienced an invasion. In terms of observable variables, Table 1.2 shows that these counties were somewhat different from other counties. Hence, never-invaded counties might constitute a poor counterfactual and could produce bias in our estimation in the presence of unobserved time factors interacting with some fixed county characteristic. To check for this potential threat we estimate equation (1.2) using only the sample of 176 counties with at least one invasion in the period of study. When imposing this restriction, identification arises only from the *timing* in which first invasions began to appear across counties. Results are presented in Table 1.5 column 2. Estimates remain of similar magnitude and statistical significance and hence this is unlikely to be a concern. Similarly, results are also robust to different measures of the dependent variables (Appendix Table A.2).

Yet another potential threat is the presence of correlated unobserved time shocks. A leading concern is the availability of large (expropriable) plots which made the county subject to expropriations and invasions right from the beginning of Allende's government, perhaps creating a spurious correlation between these variables. Reassuringly, results are similar when we control for the county-level availability of large plots – as measured by the 1965 agricultural census – interacted with time (calendar) fixed effects (Appendix Figure A.5). More generally, any time-variant policy that affects counties in the south or the north of the country differentially constitutes a potential threat. To address these concerns we estimate equation (1.2) using region-by-year fixed effects. Chile was divided in 13 regions, administrative units composed by clusters of counties. This specification allows for non-parametric regional trends in both invasions and expropriations. Column 3 in Table 1.5 present estimation results for the four expropriation outcomes and estimates remain virtually unchanged. In addition, column 4 shows that all results are robust to the inclusion of county-specific linear trends. Finally, our inference remains unchanged when using two-way clustering to allow correlation of outcomes within event dates (Brown and Warner, 1985), and it is also similar when we allow for spatial correlation across counties during each time period (Conley, 1999).[12]

[12] Appendix Figure A.6 presents results. To allow for spatial correlation we use a heteroskedasticity and autocorrelation consistent covariance estimation with distances from the centroids of counties and a Bartlett kernel. Results are also similar if we follow Bertrand, Duflo, and Mullainathan (2004a) and group months into larger periods such as quarters (Appendix Figure A.7).

Table 1.5: Robustness of parametric event study results

	Sub-samples			
	Removes counties with events within 3 months of Allende's rule	Removes counties without events	Region-by-year fixed effects	County-specific linear trends
Dependent variable	(1)	(2)	(3)	(4)
Number of plots invaded	0.46***	0.52***	0.54***	0.57***
	(0.09)	(0.06)	(0.06)	(0.06)
Number of plots expropriated	0.23**	0.17**	0.15**	0.19***
	(0.11)	(0.08)	(0.07)	(0.07)
Indicator at least one expropriation	0.01	0.02	0.02*	0.03**
	(0.02)	(0.01)	(0.01)	(0.01)
Number of hectares expropriated	0.02	0.17*	0.13	0.21**
	(0.13)	(0.10)	(0.09)	(0.09)
Number of plots redistributed	0.21**	0.17**	0.14***	0.17***
	(0.09)	(0.07)	(0.05)	(0.06)
Number of hectares redistributed	0.07	0.19**	0.15*	0.19**
	(0.11)	(0.08)	(0.08)	(0.08)
Counties	129	176	221	221
Observations	6,450	8,800	11,050	11,050
County fixed effects	X	X	X	X
Month fixed effects	X	X	X	X

Notes: Each estimate and its standard error come from an estimation of equation (1.2) using a different dependent variable. Rows represent different outcomes and columns denote the robustness exercise implemented. Each observation corresponds to a county-month pair in the period between 01/1970 and 04/1972 except otherwise noticed. The number of hectares expropriated and redistributed use the hyperbolic sine transformation proposed by Burbidge et al. (1988). Standard errors are clustered by county. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: Land reform data files and police reports of land invasions.

1.6 Mechanisms and Interpretation

This section evaluates three interpretations of previous results. First, we analyze if the actions of workers can be considered a threat to revolt. Second, we evaluate the possibility that collective actions were orchestrated by the government to facilitate expropriations. And third, we consider whether invasions shaped the policy agenda by changing local public opinions. We end by offering back-of-the-envelope calculations of the role of displacement in explaining our findings.

The threat of a revolution and collusion

Historians have emphasized that organized groups invaded plots to try to exert pressure on the government to radicalize policies and increase redistribution in the short-run (e.g. Robles-Ortiz 2018).[13] This is also a classical theoretical argument formalized by Acemoglu and Robinson (2006). Under this framework, the government observes invasions and chooses to either repress collective actions or expropriate the plot. If repression is chosen, there is a probability of a revolution and the government could be overthrown or impeded to follow its economic and political plans. Then, if we observe the government expropriating after an invasion, we say that existing conditions made the latter option more attractive because of the "threat of a revolution."

Another interpretation of our results is that the government was orchestrating invasions to facilitate expropriations. Although no legal cause can appeal to invaders as a reason to expropriate, the government could have incentivized workers to invade plots with the goal of exerting pressure on the landowner to offer it to the corporation. This legal cause accounted for 22% of expropriations in the Allende years (see column 3 in Table 1.1), therefore at first sight this interpretation might be important. However, the work by Winn and Kay (1974) and Robles-Ortiz (2018) suggests that Allende did *not* orchestrate invasions at the beginning of his government. In contrast, radical left-wing groups outside of the government seem to have triggered most of the early invasions, which lends credibility to our econometric focus on early invasions and the "threat of a revolution" interpretation. Moreover, a battery of empirical exercises suggest that a potential collusion between Allende and invaders is unlikely to explain the empirical relationship between invasions and expropriations we have documented.

The role of left-wing radical groups in triggering early invasions has been previously documented by historians, and the majority claim that the goal was to exert pressure to radicalize the land reform program and "speed up" the revolution. The most well-known groups exerting this pressure were the Revolutionary Left-wing Movement (MIR) and the Peasant Revolutionary Movement (MCR). An example of the role of the former comes from Winn and Kay (1974, p. 141), who emphasize its role early on: "With the encouragement and assistance of MIR, the revolutionary movement to the left of the Unidad Popular, these tomas [invasions] had assumed powerful proportions by the first months of 1971. To the Allende government, this pressure from below represented both an opportunity for speeding up the rural revolution and a threat to the government's commitment to legality and controlled change."[14] Similarly, Robles-Ortiz (2018, p. 142) emphasize the role of the MCR in triggering some of these early invasions: "Confronting the workers, Governor Hodges argued that the

[13] Some scholars argue that social movements aiming to pressure Allende are one of the explanations behind the social instability and Allende's overthrow. See Goldberg (1975); Sigmund (1977) for a discussion.

[14] The pressure from invasions was not envisioned by Allende: "Another active form of peasant participation in the expropriation process, one not envisioned in the UP program, has been the tomas [...] The tomas were a form of pressure on the government bureaucracy to accelerate the expropriation process [. . .]" (Winn and Kay, 1974, p. 143).

toma [invasion] was illegal, and it would only be prejudicial to President Allende, because the opposition would use it to blame the government for the 'state of chaos' in the countryside. Hodges did not persuade the MCR workers; an MCR 'emissary' went to his office to inform him that they would take over all the cordillera latifundia."

To empirically assess a potential role of the government in driving our results, we performed three empirical exercises. First, we have reestimated our main specification exploiting only the first invasions that occurred within six months of Allende's government. We do this to be conservative and assume that invasions towards the end of 1971 and 1972 could have been orchestrated by the government. Reassuringly, panel (a) of Figure 1.5 shows that results are similar, suggesting that estimates are unlikely to be driven by government actions. Second, Winn and Kay (1974) argue that some invasions were planned by the government at the regional level. At the time Chile was divided into 13 regions. These plans could constitute a threat if we are omitting regional factors driving invasions and expropriations. However, results in panel (b) of Figure 1.5 are again similar when we include region-by-month fixed effects, suggesting unobservables at the regional level are unlikely to be an econometric threat. And third, if the government planned invasions we might expect this to occur in places where they had more political support. However, Appendix Figure A.8 shows that invasions were if anything more likely to have taken place in locations where Allende obtained *fewer* votes in the 1970 presidential election. In sum, the evidence is inconsistent with a role of the government in driving the empirical relationship between invasions and expropriations.

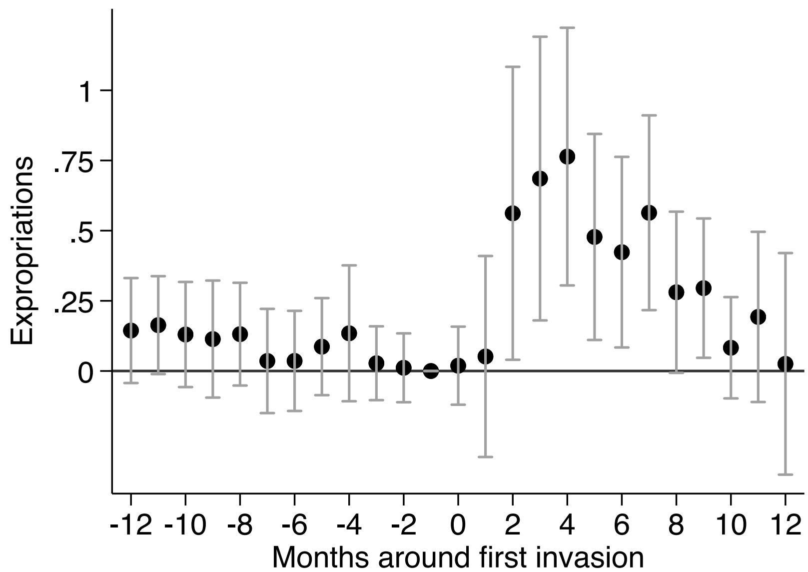
Local public opinion

An additional mechanism through which invasions could have increased the intensity expropriations is by shaping public opinion regarding land inequality and the plight of the poor. Although intuitive, Robles-Ortiz (2018) claims that invasions fostered mixed local opinions and received negative coverage from the opposition-controlled press. Newspapers highlighted the presence of MIR (radical left) collaborators, referring to them as "extremist elements" (*El Correo*, December 1, 1970). A key contributor to the negative press that invasions received was the Christian Democrat Party (PDC), a large party with strong support in the countryside which was publicly against land invasions. Robles-Ortiz (2018) argues that:

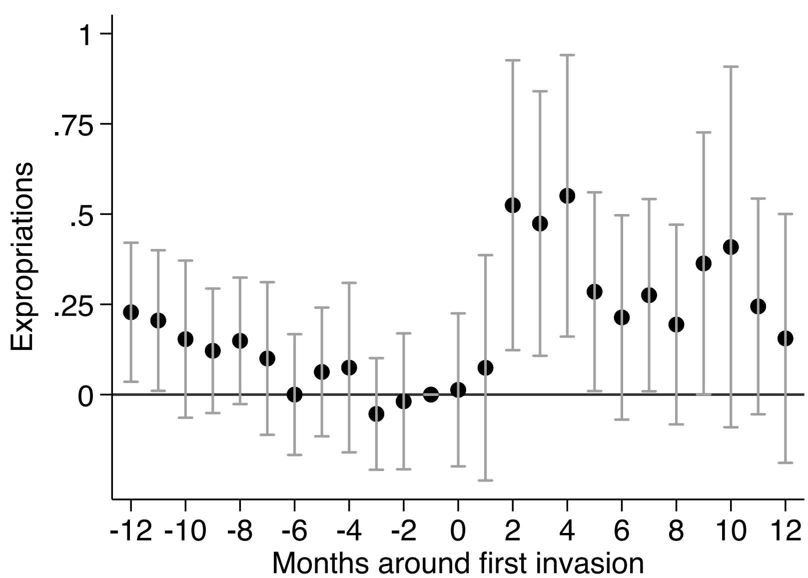
> "The PDC's discourse was politically influential. It grossly exaggerated the 'guerrilla threat,' and was systematically disseminated by the opposition's newspapers. In early February of 1971, providing no source, *El Correo* reported that 'all the cordillera next to Panguipulli is under Comandante Pepe's control, and he is now in the position of mobilizing a mob of no less than five thousand campesinos.' PDC national leaders used the newspapers' vague notes to support their interventions in Congress."

Surveys conducted during the first two years of Allende's government also support the idea that invasions were far from popular among the public. These surveys, conducted

Figure 1.5: Collusion between workers and the government



(a) Using only early invasions



(b) Region-by-month fixed effects

Notes: Panel (a) presents estimates of equation (1.1) with their corresponding 95 percent confidence interval using only invasions within 6 months of Salvador Allende's government (November 1970 - May 1971). According to historical accounts these early invasions are unlikely to be orchestrated by the government. Panel (b) presents estimates using our main specification but augmented with region-by-month fixed effects, administrative unit in which the government appears to have organized some invasions. Both panels constitute evidence against the collusion mechanism.
Source: Land reform data files and police reports of land invasions.

by sociologist Eduardo Hamuy, reveal that 47% of 1800 respondents thought violence had increased when compared to previous governments.[15] Moreover, 60% responded that the left-wing was causing this violence and only 16% perceived it was caused by right-wing groups. Finally, consistent with previous anecdotal evidence and responses in the Hamuy surveys, Appendix Table A.3 presents cross-sectional regression estimates which reveal that the number of invasions before the 1971 local election was unrelated to the local political support obtained by the candidates from the left-wing coalition in power (UP). In sum, anecdotal and empirical evidence suggest that the public opinion was unlikely to be a mechanism connecting invasions and the policy agenda.

The role of displacement

Our estimates represent the impact of first invasions after Allende rose to power using other counties as counterfactuals over time. Without further assumptions, these within-country comparisons prevent us from knowing whether invasions increased the intensity of expropriations or if these would have taken place anyways in a different location or time. This could be the case if, for example, the government had limited capacity and invasions were diverting expropriations from one place to another. Although the potential displacement does not invalidate our analysis, it affects its interpretation. This section explores the importance of displacement in explaining our findings using an estimate of the structure and strength of displacement.

Spatial diversion of expropriations is likely to be the most relevant displacement.[16] Unfortunately we lack a counterfactual for the country, so the best we can do is to explore the importance of displacement using two simple assumptions. First, we use our estimates from previous sections and assume the absence of displacement: there were 176 first invasions causing an increase in hectares expropriated per month over a 12-month period, for a total of 0.6 million hectares expropriated due to invasions, or 10% of Allende's expropriations. Second, we assume that displacement occurred only across *adjacent* counties. In practice, we estimate equation (1.2) using the sum of hectares expropriated in the three nearest counties as the dependent variable. A negative estimate would indicate the presence of displacement. However, after a first invasion we estimate that expropriations *increased* by 300 hectares in nearby counties.[17] We can conservatively use the 95% confidence interval $[-110, 700]$ and

[15] In the design of these probabilistic surveys Hamuy received help from French sociologists Alain Girard and Alain Touraine. More information about these surveys can be found in Hamuy, Salcedo, and Sepúlveda (1958) and Navia and Osorio (2015).

[16] Temporal displacement within counties seems unlikely to be a concern: Figure 1.4 reveals that all point estimates after the first invasion were positive and some should be negative in the presence of this type of spillover. We cannot test for temporal displacement in a longer period of time because of the 1973 coup that ended the Allende government.

[17] One potential explanation for this finding is that plot owners decided to offer the plot in response to the perceived threat of an invasion. In this case an invasion in an adjacent county serves as an informational signal for landowners. If this were the case, we would be underestimating the impact of invasions on expropriations.

reject a displacement rate larger than 38% ($-100/261 = 0.38$).[18] Using this rate, we calculate that invasions increased the number of hectares expropriated during the Allende years by 0.4 million hectares or 6% of expropriations in this period.

All in all, these calculations suggest that land invasions generated 0.4-0.6 million hectares of additional expropriations, equivalent to 6-10% of all area expropriated by Salvador Allende between 1970 and 1973, approximately 0.7% of the Chilean territory or the size of Trinidad and Tobago. Thus the presence of displacement is unlikely to fully explain our findings.

1.7 Conclusion

The role of collective action as a factor that can affect the intensity of a policy has been relatively overlooked empirically. In this paper we have studied Chile's peasant social movement in the beginning of the 1970s and Salvador Allende's land reform program to show how organized groups of agricultural workers affected the location and intensity of expropriations. We find that in the months following the invasion of a plot the number of plots expropriated in the same area increased significantly. After exploring a variety of mechanisms we conclude that the government is likely to be expropriating plots after invasions to avoid an uprising.

The impact of land invasions on the policy agenda can deliver important lessons for the future. Recent waves of protests around the world have sparked a renewed interest in understanding the role of group actions in shaping the policy-making process. Moreover, the increased availability of information technologies has decreased the cost of coordination and hence collective actions are likely to become more common, not only in developed countries but in low-income countries as well (Enikolopov et al., 2020; Manacorda and Tesei, 2020). In our context the unionization law of 1967 acted as a decrease in the cost of coordination and hence land invasions and other collective actions spread throughout the country. We believe this historical context provides a useful case study to understand the interplay between organized groups and the policymaker. Our results highlight the potential radicalization of the policy agenda of an incumbent government in the presence of organized groups that can exert pressure to pursue their goals.

[18] We also used the five and ten closest counties and reject any rate of displacement, i.e. confidence intervals are always positive. Of course, the displacement structure could be more complex than across adjacent counties, as in Dell (2015). One possibility is that invasions took place in clusters of counties and displacement occurred across clusters instead of counties. Although the displacement structure is unknown, we test for the most intuitive one.

Chapter 2

Decomposing Political Favoritism in Kenyan Mass Electrification

2.1 Introduction

Political and ethnic favoritism can harm economic development by diverting spending away from the public optimum (Easterly and Levine, 1997; Posner, 2005; Michalopoulos and Papaioannou, 2016). When citizens vote largely along ethnic-party lines, electoral accountability can be limited and public services may serve as a form of patronage for government supporters (Ferraz and Finan, 2008; Casey, 2015). In Sub-Saharan Africa, where political divisions often mirror ethnic ones, clientelistic allocation of public resources is often believed to have undermined economic performance. Recent research, however, has spurred optimism for accountability mechanisms that could curb favoritism.

Rigorous democratic institutions and a free and transparent press can empower citizens to hold their elected officials accountable (Hodler and Raschky, 2014; Burgess, Jedwab, Miguel, Morjaria, and Padró i Miquel, 2015). International donor agencies increasingly place strict conditions on the use of their funds to restrain corruption (The World Bank, 2007). Decentralization can improve delivery by leveraging local knowledge about constituent needs (Harris and Posner, 2019). However, assessing accountability channels by comparing contexts can confound other differences, posing an empirical challenge.

To investigate how accountability may constrain favoritism, we study multiple potential channels within a single context: Kenya's nationwide electrification project. Launched in 2008, this large-scale and politically high-profile public investment aims to provide universal household electricity access by 2022.

A first major contribution of this paper is to combine a spatially and temporally rich set of electricity infrastructure and construction data with granular electoral data on Kenya's 2013 and 2017 national elections. The unusually disaggregated and detailed nature of the resulting dataset allows us not just to estimate the overall extent of favoritism, but to investigate the mechanisms through which favoritism emerges.

We first analyze political favoritism in the context of the canonical distributive politics framework (Golden and Min (2013)). Wards that voted for the incumbent party in the 2013 elections receive 38% more electricity connections per capita than opposition areas between 2016-2019. Construction follows the electoral cycle, peaking in the 6-12 months preceding the election and slowing down significantly after. Site selection targeted core rather than swing wards, and this appears to have increased the incumbent's vote share in the 2017 election. Given the limited impacts of household electrification on economic development in rural villages (Lee, Miguel, and Wolfram (2020b); Burlig and Preonas (2021)), vote seeking appears to distort the allocation of public investment.

In a second major contribution, we use the detailed panel dataset to decompose the construction process into four distinct stages: (i) transformer installation, (ii) selection of transformers for mass connections, (iii) construction at selected transformers, and (iv) household meter activation. Stages (i) and (iii)—which were hard to track and hidden from the public eye—exhibit significant favoritism, while stages (ii) and (iv)—which were disclosed publicly —do not.

Finally, we explore potential other channels of accountability, and rule out most. Favoritism is overwhelmingly driven by differences in support for the incumbent President, while variation in voters' alignment with their Member of Parliament (MP) does not meaningfully affect outcomes. This suggests that decision-making on large nationwide programs like the electrification initiative we study is still primarily driven by central government officials and considerations, and argues against the view that Kenya's recent move towards greater fiscal decentralization – including through a major constitutional reform – has fundamentally altered the country's political economy. Berkouwer, Hsu, Miguel, and Wolfram (2021) explores how oversight by international aid donors (who largely funded the electrification project) may have helped restrain favoritism in stage (ii). However, stage (iii) diplays significant favoritism despite significant donor involvement, suggesting a limited impact of donor conditionality in restraining favoritism.

This paper contributes to the rich inter-disciplinary literature on political favoritism and its effect on the provision of public goods in Sub-Saharan Africa (Easterly and Levine, 1997; Miguel and Gugerty, 2005; Michalopoulos and Papaioannou, 2016; Hodler and Raschky, 2014), including the provision of electricity services (Min, 2019; Briggs, 2021; MacLean, Gore, Brass, and Baldwin, 2016; De Luca, Hodler, Raschky, and Valsecchi, 2018). In particular, it builds on the recent work of Burgess et al. (2015) and Harris and Posner (2019) on the role of Kenya's electoral democracy in potentially constraining the clientelistic allocation of two other major categories of public goods—roads and local development funds, respectively. In their extensive survey of the political favoritism literature, Bardhan and Mookherjee (2018) observe that the link between clientelism and infrastructure investment remains a largely open question.

In this study, we find evidence of widespread political favoritism in the construction of household electricity connections in Kenya even after the advent of competitive multiparty politics, although the magnitude of the favoritism effects are considerably smaller than those estimated for road construction during earlier non-democratic periods in Kenya (Burgess et al., 2015), which may be due to increased constraints on executive and legislative power (Opalo (2020a)). While much of this prior work has been restricted to studying on- and off-election years, our panel dataset of weekly construction progress allows us to study how public goods provision tracks political developments at a uniquely high frequency, and in the case of the 2017 Kenyan national election, how investments in a high-profile public project accelerate in the weeks immediately before voters head to the polls. These data also contribute to the empirical literature on the timing of fiscal spending around the election cycle (Nordhaus, 1975; Alesina and Roubini, 1992; Baskaran, Min, and Uppal, 2015; Marx, 2018).

The rest of the paper is organized as follows. Section 2.2 explains the institutional background behind nationwide electrification and the Kenyan political context. Section 2.3 describes the data. Section 2.4 presents our main findings on how political favoritism has shaped the nationwide deployment of electricity connections, and decomposes the channels through which any favoritism may emerge. Section 2.6 analyzes how electricity connections influence and in turn are influenced by the electoral cycle, while section 2.7 examines the effect of local politics on shaping electrification. Section 2.8 concludes.

2.2 Background

At the time of the 2009 census, only 20% of Kenyan households had access to electricity, defined as a household using electricity as their primary source of lighting. Access was particularly limited in rural areas, where household electrification rates averaged just 5%. Low electrification rates are common in Sub-Saharan Africa, where around half of the world's 1.2 billion people without electricity live.

Between 2008 and 2020, motivated by a broad objective of achieving universal electricity access, the Government of Kenya (GoK), together with international donors, directed significant funding to several nationwide electrification projects. The nationwide effort to reach universal electricity access consisted of several distinct components. In 2008, the Rural Electrification Authority (REA) launched a Strategic Plan to build transformers in rural areas across the country and connect public facilities. In 2011 the GoK and the World Bank announced the Global Partnership on Output-Based Aid (GPOBA), a slum electrification project designed to connect households living in informal settlement areas. The Last Mile Connectivity Project (LMCP), announced by the Kenyan government in May 2015, targeted primarily rural households, and installed streetlights in cities and major towns. Later, the Kenyan Off-grid Solar Access Project (KOSAP) was added with the goal of using solar and other alternative energy sources to provide access to electricity for households living primarily in the Northern and Eastern parts of the country, which are currently beyond the reach of the bulk of the electricity network.

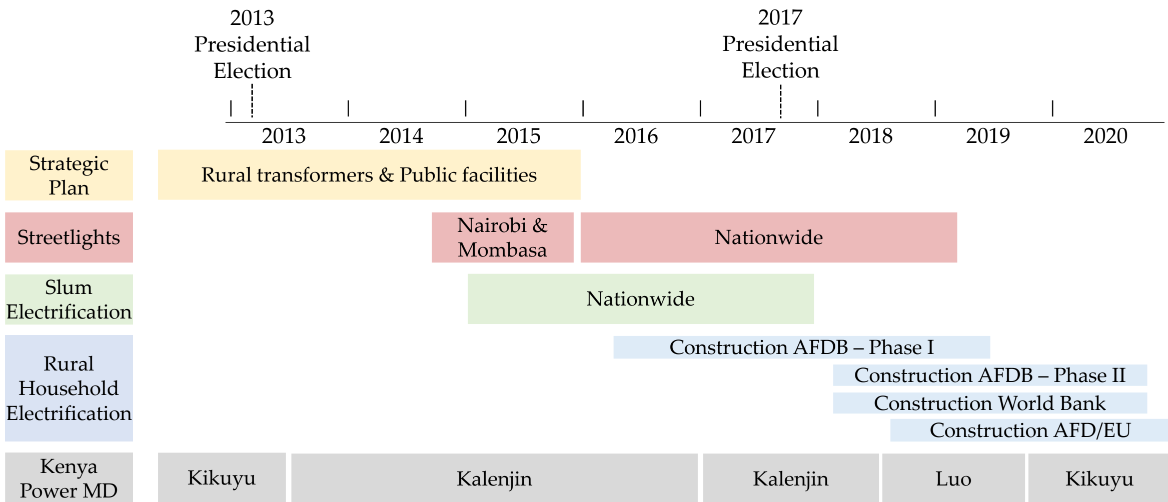
The primary government agency responsible for implementing GPOBA and LMCP was The Kenya Power and Lighting Company (Kenya Power), an electric utility that holds a nationwide monopoly over electricity distribution. Kenya Power is 51% government-owned.

Kenya's nationwide electrification projects were highly politicized, with significant construction progress made in the run-up to a contentious 2017 election. During this time Kenya Power was also affected by a substantial turnover in leadership amid allegations of corruption, driven by a political realignment that disrupted ethnic alliances in national-level politics. Figure 2.1 presents an overview of the various programs and events in the period under study. The sections that follow provide more detail about each component of Kenya's nationwide electrification and the political landscape.

Transformer construction

In 2008, REA launched a nationwide Strategic Plan to connect more than 7,800 rural public facilities to electricity, including secondary schools, trading centers, health and water centers, and administrative facilities (REA, 2008; Berkouwer, Lee, and Walker (2018)). At the time, a key constraint for achieving universal electricity access was the lack of distribution transformers in rural areas, which convert power from 11kV or 33kV medium voltage (MV) long-range transmission cables down to low voltage 0.415kV wiring (LV), which is what rural facilities and households are usually connected to. Many rural public facilities were too far away from the nearest transformer to directly receive a workable connection, as

Figure 2.1: Timeline of political and Kenya Power events, 2013-2020



Note: This timeline shows recent history of politics and mass electrification programs in Kenya. The first four rows show the rollout of electrification programs: the building of rural transformers to connect public facilities under the Rural Electrification Authority's Strategic Plan; the building of streetlights (starting in Nairobi and Mombasa, Kenya's two largest cities); the electrification of urban slum areas; and the programs of rural electrification funded by the African Development Bank (AfDB), the World Bank, the French Development Agency (AfD), and the European Union (EU). The bottom row shows the ethnicity of the Managing Director (MD) of Kenya Power.

the nearest town was often many kilometers away and distribution losses from LV wires increase exponentially with distance. REA's goal therefore required the construction of several thousand transformers across the country, in addition to the final LV connections from the transformer to the public facility. As a result, the number of 11/0.415kV and 33/0.415kV distribution transformers nationwide increased by more than 60%, from 4,200 in 2008 to more than 7,000 in 2015 (Kenya Power (2013, 2017)).

Household electrification

Since 2015, the African Development Bank (AfDB), the World Bank, the French Development Agency (AFD), the European Union, and the European Investment Bank jointly contributed more than USD 770 million to the LMCP, supplemented by internal GoK funding. This paper focuses on the Phase I sections of the LMCP that were funded by the AfDB and the World Bank. Together, these targeted 10,640 transformers for *transformer maximization*: connecting all households (usually between 20 and 100) located within 600 meters of a transformer that was already connected to the national grid, many of which had been built during REA's Strategic Plan. These projects were targeted to generate an additional 550,000 grid connections (Kenya Power (2017)). The list of transformer sites to be included in LMCP Phase I was selected jointly by the funders, Kenya Power, and the MP for the constituency containing each site using a rigorous and public selection progress, and the lists were shared publicly (we discuss media coverage in more detail below).

After the transformer sites had been selected, Kenya Power was responsible for implementing the project according to donor requirements: construction was to be outsourced to private contractors after a competitive bidding process. 35 different contractors were responsible for implementation of the AfDB and World Bank Phase I portions of the LMCP, with each contractor responsible for all sites in the region (a set of counties) for which they had won the bidding process, making this phase substantially harder for the media and the public to track (Berkouwer et al. (2021)).

Construction at each site proceeds as follows. First, a contractor visits the site to understand the existing layout of the local low-voltage network and to determine the number of unconnected households who reside within 600 meters of the selected transformer and are thus eligible for an electricity connection. Second, a contractor uses this information to design a proposed expansion of the local grid that efficiently and cost-effectively reaches eligible households. Third, the materials needed to complete these expansions are procured. Fourth, poles are erected around the village according to the design. Fifth, cables are installed between the transformer and the newly constructed poles, and from the poles to the eligible households, a phase referred to as stringing.[1] Finally, Kenya Power is responsible for installing a meter at each household, and activating their connections so that electricity runs to the household. At this point, the site is considered complete.

Households connected under LMCP faced a subsidized connection fee of KES 15,000 (USD 150), significantly lower than the standard fee of KES 36,000 (USD 360). And, rather than paying the entire sum up-front, customers are able to pay in 36 monthly installments of KES 400 (USD 4) that are automatically charged to each household's meter every month in the three years after connection, with no requirement of a downpayment. A final component of the LMCP was the installation of streetlights in towns throughout Kenya that were seen as having significant potential for economic growth. This was implemented in Nairobi and Mombasa in 2014 and 2015, and in smaller towns across the country from 2016 until 2018. In this paper we focus primarily on household connectivity, and therefore do not present results on streetlight installation.

In addition to electrification in rural areas, Kenya's flagship slum electrification project, GPOBA, was rolled out in cities and towns between 2015 and 2017. The initial rollout was targeted at major urban slum areas in Nairobi and Kisumu, such as Mathare, Mukuru, and Kibera. GPOBA then continued to be implemented in smaller towns across the country and in peri-urban areas. This led to frequent overlap of GPOBA and LMCP implementation, and as such, we combine GPOBA and LMCP meters in parts of the analysis.

The average marginal cost per connection was significantly lower for GPOBA than for LMCP, due to the high population density in GPOBA areas. The price of a household con-

[1] For a given transformer funded by the AfDB, one contractor is responsible for all five stages. At World Bank sites, different contractors are responsible for different stages. We discuss this in more detail in Berkouwer et al. (2021).

nection under GPOBA was Ksh 1,160 (USD 12)—less than 10% the cost of a rural connection under LMCP. This also explains why electricity theft is very uncommon in rural areas. Due to the large physical distances between residential compounds new electricity connections often require at least one additional pole and many meters of low voltage wiring. The high cost of materials prevents most families from acquiring a connection through informal means and without a government subsidy: even the relatively high price of USD 150 for a rural electricity connection still represents only a fraction of the true average cost of construction (Lee et al. (2020b)).

Kenyan politics and Kenya Power leadership

Kenya's electrification projects were in part shaped by political developments surrounding Kenya's 2013 and 2017 presidential elections. In March 2013, Uhuru Kenyatta won his first presidential victory. Three months later, he installed Ben Chumo as Kenya Power's Managing Director (MD), who oversaw many of the electrification initiatives over Kenyatta's first term. In the 12 months prior to the 2017 presidential election, more than a million new household meters were installed.

In a State of the Nation Address in March 2017, five months before the election, Kenyatta stated: "To begin the walk towards industrialisation, we needed to drastically improve and expand our infrastructure, and to increase access to electricity and diversify our energy sources... In 2013, we promised to provide access to electricity for 70% of all households by the end of 2017. Today, we have connected an additional 3.7 million new homes to electricity. We have more than doubled the total number of connections made since independence." (Kenyatta, 2017)

Presidential voting in Kenya frequently aligns with ethnic identity, which can cause increased tensions and violence around elections—most notably in 2007, when over 1,000 people were killed after a disputed result. Kenyatta is ethnically Kikuyu, and his partnership with his ethnic Kalenjin running mate William Ruto gained him significant support in the Rift Valley, home to much of Kenya's Kalenjin population. Raila Odinga—Kenya's main opposition leader in both the 2013 and 2017 elections—is an ethnically Luo Kenyan, whose primary political base is the large Luo population located primarily in Nyanza and Western Kenya.

On August 8, 2017, Kenyatta was reelected to Kenya's Presidency with 54% of the vote, defeating Odinga a second time. Kenyatta's Jubilee Party—the successor to the Jubilee Coalition, an alliance of 11 different political parties formed for Kenyatta's 2013 run—won a plurality (140 out of 290 seats) in the National Assembly, the lower house of Kenya's Parliament. Odinga's Orange Democratic Movement came in second, with 62 MPs elected.

In response to his defeat, Odinga contested the results, and on September 1, the Supreme Court annulled the election, citing widespread irregularities. This decision came largely as a surprise, as it marked the first time in African history that a court had nullified the reelection of an incumbent (de Freytas-Tamura, 2017). The Court called for a repeat election to be held 60 days later. However, on October 10, Odinga announced that he was boycotting the new election, citing a lack of reform in Kenya's electoral commission. When the repeat election was held on October 26, Kenyatta again won re-election, this time with over 98% of the vote.

Kenyatta was sworn in to his second term on November 28, but tensions continued. In January 2018, Odinga supporters gathered in Nairobi to inaugurate him as the "People's President". In response, the government jailed opposition leaders and took Kenya's three biggest television stations off the air (Ombuor, 2018).

Tensions unexpectedly fell on March 9, 2018, when Kenyatta and Odinga held a public meeting at the Presidential residence to announce a truce. Photos of the long-time adversaries shaking hands were widely circulated in domestic and international media, and the subsequent reconciliation between opposition and government became known in Kenyan media as "the handshake".

Figure 2.1 shows the timeline of changes in Kenya Power leadership. When Chumo's four-year term expired in 2017, he was originally replaced as MD by Ken Tarus in January 2018. But in July 2018, four months after the handshake, most members of the Kenya Power Board of Directors were arrested on charges of corruption—this included Chumo and Tarus, who, like Ruto, are both ethnically Kalenjin (Menya, 2018). These leadership changes were interpreted by some as being linked to President Kenyatta's political realignment away from Ruto towards Odinga (Gaitho, 2019; Wilson, 2019). Over this period, Ruto was widely seen as having fallen out of favor, with key allies of Ruto being removed from office on charges of corruption (Wilson, 2019). The new MD appointed by Kenyatta in July 2018 was Jared Othieno, who—like Odinga—is an ethnic Luo. This cooperation was recognized as the signal of a significant realignment in Kenyan politics (Obonyo, 2020; Mwangi, 2019). Kenyatta and Odinga jointly launched a high-profile task force to recommend political reforms, and with Kenyatta's support, Odinga was appointed the African Union's High Representative for Infrastructure Development (Agutu, 2018).

2.3 Data

To analyze how political favoritism shaped Kenya's electrification effort, we combine electoral data with infrastructure data from Kenya Power and construction reports from independent contractors. We match construction progress reports to the infrastructure data at the individual transformer site level using GPS coordinates. We then aggregate the site-level data up to the electoral ward level to match them with electoral results. The following sections explain these data sources and the matching approach in greater detail.

Infrastructure data

We analyze two administrative datasets from Kenya Power containing the universe of the 7.4 million meters and 63,525 transformers located across Kenya as of December 2019.

The meter dataset includes basic meter attributes, such as what tariff the account is on, whether they use post-paid or pre-paid billing, on what date the meter was connected, and whether the meter was connected via LMCP, GPOBA, or a different project. The list of meters includes primarily household meters.

The location coordinates for some transformers are either missing or inaccurate. To merge the meter and transformer data sets with electoral and construction progress data, we develop a matching algorithm that combines GPS coordinates, names of administrative units, and location names. The transformer GPS coordinates also identify the electoral ward where each transformer is located. The number of transformers per ward ranges from 0 to 300 and averages 31. We also observe the list of 10,640 of these transformers that were selected for inclusion in LMCP Phase I, including whether each transformer was funded by the AfDB or the World Bank. The end result of this procedure is a novel, comprehensive dataset of 41,641 geolocated transformers nationwide with granular information about local network construction and voting outcomes, which we believe is one of the major contributions of this research project.[2] To our knowledge, this is the first research project to combine electricity infrastructure and electoral data at such fine geospatial and temporal resolution.

Construction progress data

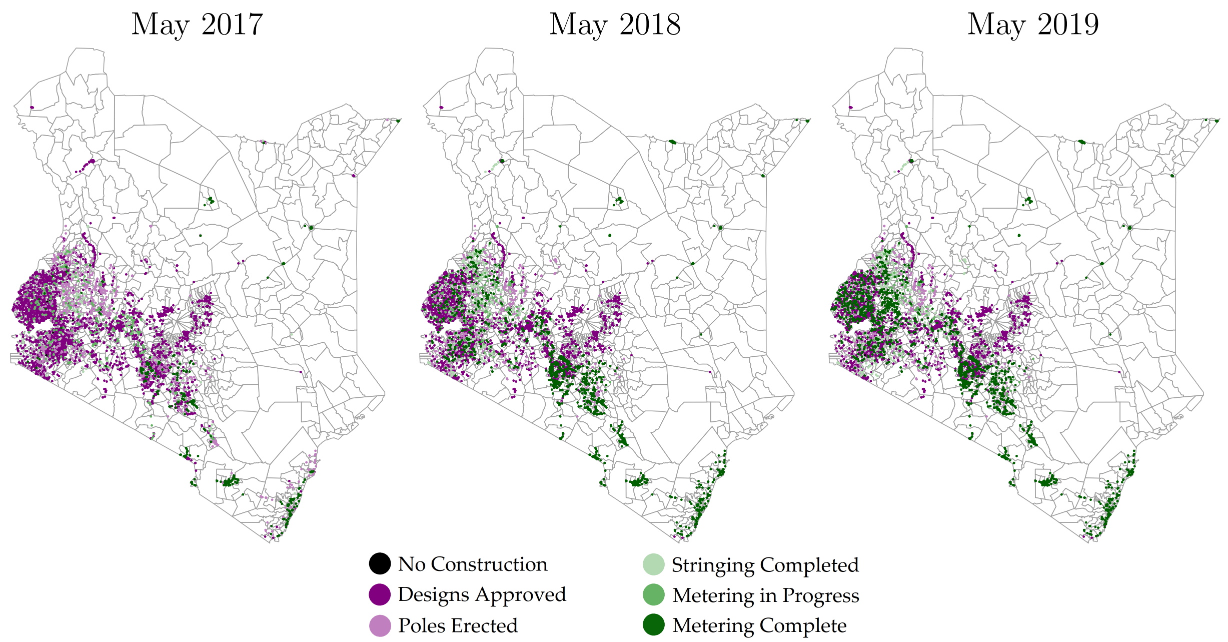
We combine the meter and transformer administrative data with transformer-level panel data on LMCP construction, generated from progress reports by independent contractors. These reports indicate the weekly status of the construction of Phase I transformer sites from May 2016 to June 2019. The stages of construction are: 1) no construction, 2) pole erection in progress, 3) pole erection complete, 4) stringing in progress, 5) stringing complete, and 6) metering complete. A 7th stage, metering activated—when electricity actually begins to flow to households—is completed by Kenya Power and thus outside the purview of the contractor progress reports. Instead, we infer meter activation from Kenya Power's database of meters, which are geolocated across the country. 2.2 presents three snapshots of this construction data. There is a clear increase in the number of completed construction sites between May 2017 and May 2019.

The frequency of reports varies by transformer. To study the timing of different stages of construction, we restrict the primary analysis to a balanced panel of 4,564 LMCP transformer sites spanning 975 wards and 118 weeks (April 2017 to June 2019). This generates a sample of 115,050 ward-week observations.

We convert the transformer reports into a set of binary progress variables. $Construction$ = 1 if a report confirms that at least pole erection is ongoing at the transformer site, and 0 if no construction has begun. $Stringing$ = 1 if a report confirms that at least stringing of wires is ongoing, and 0 if construction has not progressed to stringing. These stages are cumulative: sites where stringing is complete (i.e. $stringing$ = 1) will also have $construction$ set to 1.

[2] We describe this matching process in detail in the Data Appendix.

Figure 2.2: Construction status at transformer sites as listed in progress reports, Over Time



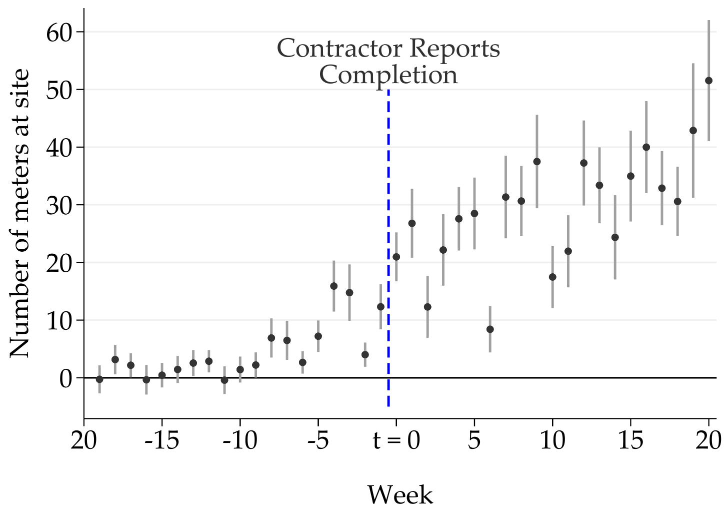
Note: These graphs present three example snapshots from our weekly construction data, indicating the status of construction at each LMCP transformer site. The full weekly panel data set spans from April 2017 to June 2019. Some updates are missing for some weeks—we address this by linearly interpolating between reports—these graphs display progress as of the most recent report.

We linearly interpolate the status of transformers between reports. If a transformer is reported as undergoing stringing 4 weeks after being reported as commencing construction, the 4 weeks in between are interpolated to reflect increases of 0.25 sites in stringing each week. Any sharp increase in construction therefore represents actual progress in construction rather than the idiosyncratic timing of reports.

To verify the accuracy of contractors' progress reports, we compare the construction completion dates listed in contractors' progress reports with meter installation dates recorded in Kenya Power's customer database, as meter installation was supposed to occur soon after construction completion. Figure 2.3 plots stacked difference-in-differences estimates of the number of meters installed in the 20 weeks before and after a contractor reports construction completion, relative to sites that were not yet completed during that period (Deshpande and Li, 2019; Cengiz, Dube, Lindner, and Zipperer, 2019; Goodman-Bacon, 2020). The estimation stacks 31 datasets matching the 31 distinct weeks during which at least one transformer had been recorded complete. To account for possible selection effects, the control group consists of sites where at least some stringing had been reported, but that never reached completion.

The figure shows that the relative number of meters increases significantly after $t = 0$,

Figure 2.3: Meters activated in Kenya Power infrastructure database after contractor reports construction completion



Note: This figure combines Kenya Power's meter data with construction progress data at the transformer level provided by independent contractors. In the weeks after a contractor reports construction at a particular transformer to have been complete, the number of meters that Kenya Power identifies as going on-line increases sharply.

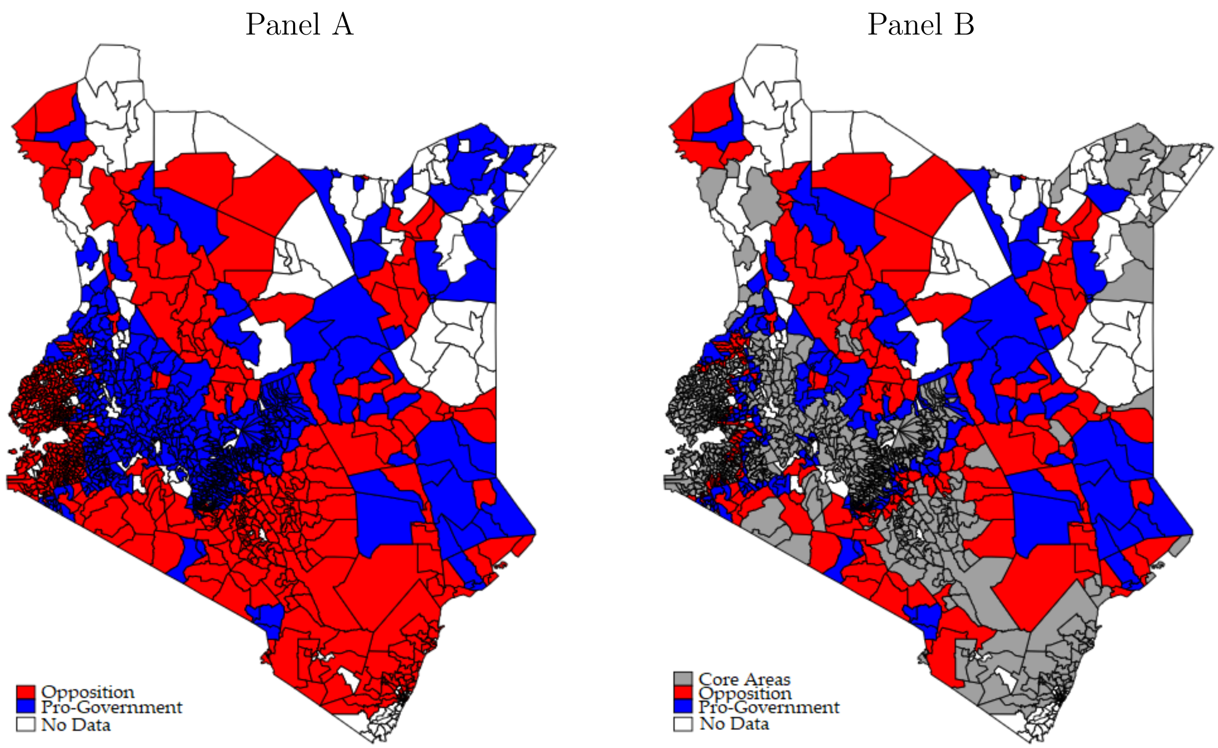
which is when the contractor first reports completion of the site. We take this as supporting evidence that the contractor's reports of construction completion are meaningful. Given that these two datasets come from independent sources—the meter activation dates from the Kenya Power infrastructure system, and the completion reports from contractors' manually compiled project reports—this strong relationship lends confidence to the accuracy of the data.

The result also shows that meters were generally not installed in bulk on the day of construction, but rather, the average number of meters at a completed site grows steadily, eventually reaching around 50 meters per site 20 weeks after the contractor reports the site is complete. It is therefore possible that households at some sites had to wait for four or five months from when pole erection and stringing had been completed until their home had a usable electricity connection. And, at some sites there is a slight increase in the number of meters even *before* the contractor reports completion, most likely due to some inaccuracies in the completion dates. For both of these reasons, we use Kenya Power's meter activation data as our primary final outcome in the analyses that follow.

Electoral and demographic data

Election results are compiled by Kenya's Independent Electoral and Boundaries Commission (IEBC) for the 2013 and 2017 presidential elections, aggregated up to the ward level—the

Figure 2.4: Kenya's 2013 Presidential Election



Note: Panel A 2013 presidential election results at the ward level. Blue wards had vote shares of over 50% for Kenyatta. Red wards had vote shares under 50% for Kenyatta. White wards are missing election data. Panel B shows the same, but 'core' wards—which only border similarly aligned wards—are greyed out.

smallest electoral subdivision in Kenya. Panel A of Figure 2.4 displays Kenya's 2013 election results. Wards in blue are where Kenyatta won over 50% of the vote ('pro-government'), while wards in red are where Kenyatta won under 50% ('opposition').[3]

The 2009 Kenya Population and Housing Census—the most recent national census before the launch of LMCP—reports socio-economic data at the ward level, and is thus straightforward to merge with electoral data.

Omitted variables unrelated to political affiliation may be a cause of differences in electrification between pro-government and opposition wards. This is especially true in Kenya, where political affiliations are geographically segregated, as shown in Panel A of Figure 2.4: government support is concentrated around Nairobi and the former Central Province, while the opposition coalition electorally dominates the coastal and western regions.

To address this concern, we complement the national-level results with an "adjacent wards" empirical strategy, restricting the sample to wards that border at least one ward that voted for the opposing candidate in the 2013 presidential election. The goal of this

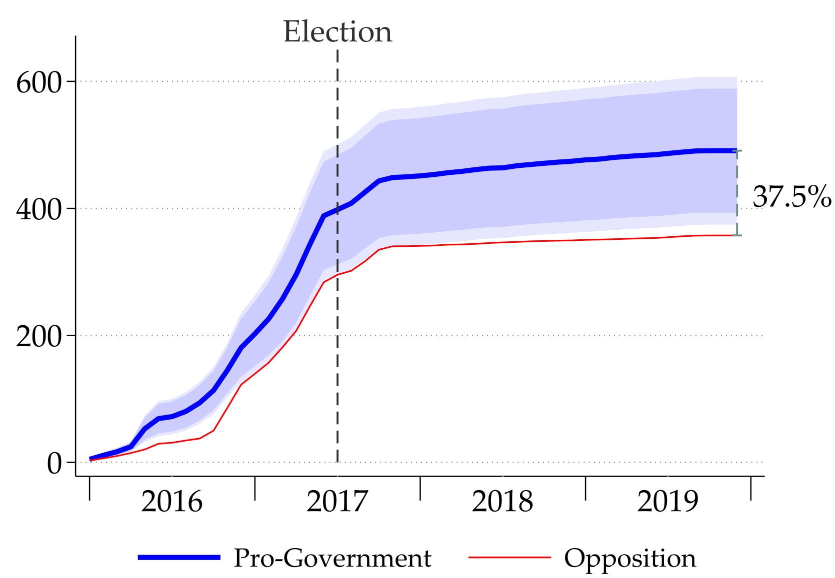
[3]2013 IEBC election data are missing for 156 out of 1,450 wards (11%). These are colored in white.

approach is to only compare wards that are similar in their geography and socio-economic characteristics. Panel B of Figure 2.4 illustrates this sub-sample, with adjacent wards colored in blue or red and non-adjacent wards ("core areas") in white. For example, a ward where Kenyatta obtained over 50% of the votes in 2013 that is completely surrounded by other wards where Kenyatta also won a majority would be excluded from the adjacent-wards sample. Keeping only adjacent wards limits the analysis to a sample size of 456 wards.

Table B.1 tests whether pro-government and opposition wards in the adjacent-wards sample had similar characteristics before the start of the electrification program according to the 2009 Census. Adjacent pro-government and opposition areas are similar in population, initial electrification, education, roofing, and urban make-up. Opposition areas have slightly less rugged terrain (as measured by a satellite-based gradient index) and larger land areas, which may affect the overall costs of construction. To account for these small differences, we control for gradient and land area in the analysis.

2.4 Electrification and National Politics

Figure 2.5: Number of LMCP meters connected per 100,000 residents



Note: The red line plots the γ_k's (LMCP meters per 100,000 people in opposition wards) from Equation 2.5. The blue line plots $\gamma_k + \beta_k$ (LMCP meters per 100,000 people in pro-government wards). The gap between the blue and red lines thus corresponds to the difference in meters per capita between opposition and pro-government wards (β_k's). The darker blue are is the 90% confidence interval, and the light blue are is the 95% confidence interval, of the β_k's, the difference between pro-government and opposition wards. The vertical line represents the August 2017 Presidential election. The national sample has 571 pro-government wards (3,534 transformers) and 672 opposition wards (2,710 transformers).

The most direct measure of household electricity connections under LMCP is the number of meters per capita that were installed in or after January 2016 at LMCP transformer sites. Equation 2.1 estimates the effect of political affiliation on metering progress:

$$y_{it} = \sum_{k=1}^{118} \gamma_k D_{it}^k + \sum_{k=1}^{118} \beta_k D_{it}^k * ProGovernment_i + \epsilon_{it} \tag{2.1}$$

where y_{it} equals the number of meters per 100,000 inhabitants at ward i in week t, the D_{it}^ks are indicator variables which equal 1 when $t = k$ and 0 otherwise, and $ProGovernment_i$=1 if ward i voted pro-government in 2013. Errors ϵ_{it} are allowed to be correlated within ward over time. Observations are weighted by the number of transformers in each ward.

Figure 2.5 presents the results. Wards that voted pro-government in 2013 have a large, persistent, and statistically significant advantage in the number of meters installed per capita. By December 2019, wards that voted pro-government had an average of 491 meters per 100,000 people, compared to just 357 meters per 100,000 in wards that voted for the opposition—a 37.5% advantage. Metering progress accelerates in both pro-government and opposition areas in the run-up to the August 2017 Presidential election and then stagnates—potentially a sign of strategic behavior around the electoral cycle. We discuss the econometric estimates in Section 2.4. Figure B.1 confirms that these results hold when constraining the analysis to adjacent wards, where geographic and economic differences are expected to be minimal.

Installing meters in households at LMCP sites is a multi-stage process. To highlight how political favoritism can influence the final deployment of electricity to households, we decompose this outcome into the following four parts:

$$\frac{\text{\# Meters at LMCP sites}}{\text{100,000 residents}} = \left(\frac{\text{\# transformers}}{\text{100,000 residents}}\right) \cdot \left(\frac{\text{\# LMCP transformers}}{\text{\#transformers}}\right) \cdot \left(\frac{\text{\# LMCP sites with construction}}{\text{\# LMCP transformers}}\right) \cdot \left(\frac{\text{\# Meters at LMCP sites}}{\text{\# LMCP sites with construction}}\right) \tag{2.2}$$

These four terms correspond to the four steps by which a rural household far from the grid gets connected to electricity: (i) electrical transformer installation, (ii) selection of transformers for mass electricity connections, (iii) construction of local networks at the selected transformers, and (iv) final household connection activation. Gaps in LMCP construction progress between pro-government and opposition wards may be caused by differences in one or more of the terms.

The following subsections consider how political favoritism may shape each of these steps. The first subsection discusses the initial numbers of transformers per capita across Kenya's wards (term 1 in equation 2.2). The next one discusses how a subset of the existing transformers were selected to be maximized under the LMCP program (term 2), and the following one discusses construction progress at those transformers (term 3). The one after that discusses the activation of the meters that provide households with electricity once construction has been completed at a site (term 4). Finally, the last subsection in Section 2.4 discusses how these four stages contribute to the aggregate influence of favoritism on household connectivity.

Each section also presents two sets of robustness checks. First, since the LMCP objective was to electrify rural areas, we exclude urban wards, defined as overlapping with one of Kenya's 42 major towns (World Resources Institute, 2007). Second, as mentioned previously, to address concerns about omitted variables, we include only adjacent wards.

As discussed in Section 2.2, the turnover in Kenya Power's leadership after the handshake suggests that the president has significant influence over Kenya Power. But political favoritism may also operate through other channels, such as the MPs who represent Kenya's 290 constituencies. In addition to a ward's political alignment with the winner of the 2013 presidential election, we therefore also consider a second potential channel of favoritism: political alignment of a ward with its constituency-level MP. Since Kenya Power worked directly with local MPs to determine the number and location of transformers to be maximized within each constituencies (subject to budget considerations), MPs may have allocated more resources to wards within their constituencies that voted for them, while disfavoring wards that voted against them. We define 'aligned' wards as those where the first-past-the-post winner of the ward was the same as the first-past-the-post winner of the overall constituency; 'not-aligned' wards are where the winners are different. The following analysis considers how favoritism through both channels—presidential and MP alignment—may have affected ward-level deployment of LMCP. We discuss the MP alignment results in greater detail in Section 2.7.

Transformer construction

This section considers the first term in Equation 2.2: the number of existing transformers per 100,000 residents. LMCP was primarily a program of transformer *maximization*: connecting customers who reside near existing transformers. As discussed in section 2.2, the locations of these transformers largely reflect earlier initiatives to build transformers in rural areas, which may themselves have been shaped by political favoritism. The following equation tests how political favoritism shaped the national deployment of transformers:

$$y_i = \alpha + \beta_1 ProGovernment_i + \beta_2 MP\text{-}Aligned_i + \gamma X_i + \varepsilon_i \tag{2.3}$$

where y_i is the number of transformers in ward i per 100,000 residents as of December 2019, $ProGovernment_i = 1$ if ward i voted pro-government in 2013, $MP\text{-}Aligned_i = 1$ if the winner in ward i's MP race won the constituency's MP race, and X_i is a set of ward-level controls that include socioeconomic status, demographics, and geography.

The top row of column (1) in Table 2.1 shows a significant positive raw correlation between the number of transformers per capita in a ward and pro-government support without controls in the 2013 Presidential election. Pro-government wards have an additional 66 transformers per 100,000 residents, 38% higher than the opposition ward mean of 173. Column (2)

Table 2.1: Number of Transformers per 100,000 inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Pro-Govt in '13	65.7***	31.9***	35.3***	33***	18.3	31.1**	30.6***
	(10.7)	(10)	(10)	(6.8)	(26.2)	(15.5)	(10.9)
Aligned with MP in '13	14.6	-2	-2.4	3.5	19.2	-8	11.4
	(11.5)	(6.9)	(6.8)	(5.3)	(25.5)	(11.9)	(10)
Opposition Mean	[173.2]	[173.2]	[173.2]	[173.2]	[193.7]	[193.7]	[193.7]
Treatment Effect	37.9%	18.4%	20.4%	19%	9.4%	16%	15.8%
Method	OLS Raw	OLS Controls	OLS Controls	Double Lasso	OLS Raw	OLS Controls	Double Lasso
Adjacent Wards Only	No	No	No	No	Yes	Yes	Yes
Rural Only	No	No	Yes	No	No	No	No
Number Observations	1,009	1,009	1,009	1,009	341	341	341
R^2	0.041	0.636	0.507	0.827	0.004	0.748	0.938

Note: Regression at the ward level. Outcome variable: meters per 100k inhabitants. 'Voted Pro-Govt in '13'=1 if Kenyatta obtained over 50% of the Ward votes in 2013. 'Aligned with MP in '13'=1 if the Ward voted for the winning MP in 2013. Controls include shares of adults with primary and secondary education, share of households with electricity, share of adults who work for pay, dependency ratio, share of households with a corrugated iron roof, ward area, household size, and being an urban ward. Controls originally at the transformer level which where averaged at the ward include gradient, granular population density, and meters per person before LMCP. Controls originally at the transformer level which were summed over at the ward level include granular population count, meter count before LMCP, and the number of unconnected households. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

adds socioeconomic and geographical controls—including land gradient, population density, and number of unconnected households—which may correlate not just with political alignment but also with optimal economic targeting, for example by affecting construction cost.[4] Even after the addition of these controls, a strong pro-government bias persists, though the estimated effect size shrinks by roughly half, to 31.9 transformers per 100,000 residents. Column (3) finds that this estimated effect persists when excluding urban areas. By contrast, there is no evidence that MP alignment drives greater transformer construction in any of the regression results.

Column (4) includes all 976 triple interactions between all controls, and applies machine learning methods to control more flexibly for ward characteristics that could be correlated

[4] Economic activity may be endogenous to political favoritism. This exercise employs only baseline socioeconomic characteristics from the 2009 census and focuses on favoritism in post-2009 construction of transformers.

with electrification and political affiliation. In particular, we use the Double Lasso Variable Selection procedure described in Urminsky, Hansen, and Chernozhukov (2016). The first step uses LASSO to estimate a regression of the dependent variable on the full set of controls, excluding the two focal independent variables. The second step repeats this for each focal independent variable. The penalty hyperparameter of each regression is chosen using 5-fold cross validation. The union of the three sets of selected variables is then included in a regression estimated by OLS. Column (4) shows that the estimated coefficient is very similar to Column (2), despite the R^2 of the model increasing substantially, suggesting the interaction of controls was important.

Columns (5-7) include only the adjacent ward sample, where unobservable differences in socioeconomic and geographic characteristics of pro-government and opposition wards are far more muted. The results are robust to the use of this subsample of wards, with the point estimates on the pro-government term nearly unchanged, although the standard errors are slightly larger due to the smaller sample size. The robustness of the results to alternative estimation methods and subsamples provides additional confidence in the relationship.

Together, these results provide strong and consistent evidence—even after addressing concerns regarding omitted variables and selection bias with a battery of empirical approaches—that the overall deployment of transformers in Kenya favored areas that voted for the incumbent president in 2013.

LMCP site selection

This section considers the selection of LMCP transformer sites from the nationwide sample of transformers, represented by the second term in Equation 2.2. To examine how political favoritism may have shaped whether existing transformers were selected for LMCP, we re-estimate Equation 2.3 where this time y_i is the share of transformers in ward i selected for LMCP (Table 2.2) and the number of transformers in ward i selected for LMCP per 100,000 people (Table 2.3).

Table 2.2 considers the number of LMCP transformers as a share of total transformers in a given ward. Column (1) shows that transformers in wards that voted pro-government in 2013 were around 0.9% less likely to be selected for LMCP compared to opposition wards. Because ward and transformer site characteristics could influence selection into LMCP while being unrelated to political bias, column (2) includes socioeconomic and geographic controls, and column (3) restricts the analysis to rural areas. The results show that transformers in pro-government wards were modestly less likely, by 1-2%, to be selected for LMCP. Column (4) applies the same Double LASSO Variable Selection procedure, and finds no significant evidence that either pro-government or opposition areas are favored in selection. Restricting the sample to just adjacent wards, columns (5-7) similarly show mixed evidence for a small negative anti-government bias in the share of transformers selected for LMCP. Finally, these results show small and generally insignificant effects of being aligned with the local MP, as discussed further in Section 2.7. The limited degree of favoritism in the selection of LMCP

Table 2.2: Probability of transformer being selected for LMCP, conditional on transformer installation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Pro-Govt in '13	-0.009***	-0.015***	-0.016***	-0.003	0.014***	-0.006	-0.015*
	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.009)
Aligned with MP in '13	-0.006*	0.008**	0.006	0.004	-0.017***	0.008	0.007
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.008)
Opposition Mean	[0.104]	[0.104]	[0.104]	[0.104]	[0.104]	[0.104]	[0.104]
Treatment Effect	-9%	-15%	-16%	-3%	14%	-6%	-15%
Method	OLS Raw	OLS Controls	OLS Controls	Double Lasso	OLS Raw	OLS Controls	Double Lasso
Adjacent Wards Only	No	No	No	No	Yes	Yes	Yes
Rural Only	No	No	Yes	No	No	No	No
Number Observations	48,301	48,301	36,839	48,301	16,331	16,331	16,331
R^2	0.000	0.045	0.027	0.079	0.001	0.072	0.137

Note: Regression at the transformer level. 'Selected for LMCP'=1 if the transformer was selected for LMCP. 'Voted Pro-Govt in '13'=1 if Kenyatta obtained over 50% of the Ward votes in 2013. 'Aligned with MP in '13'=1 if the Ward voted for the winning MP in 2013. Ward level controls: shares of adults with primary and secondary education, share of households with electricity, share of adults who work for pay, dependency ratio, share of households with a corrugated iron roof, ward area, household size, and ward population. Controls at the transformer level include gradient, granular population count, granular population density, meter count before LMCP, number of unconnected households, meters per person before LMCP. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

transformers may have been due to the large amount of donor oversight and public scrutiny, which we discuss in more detail in Section 2.4.

Table 2.1 thus shows strong pro-government bias in the number of transformers per capita (term 1 in Equation 2.2), while Table 2.2 shows slight opposition bias in the share of transformers selected for LMCP (term 2 in Equation 2.2). Combining these results determines the overall effect of a ward's political affiliation on LMCP sites per 100,000 people. Table 2.3 estimates Equation 2.3, where the outcome is the number of transformers selected for LMCP per 100,000 people in each ward. Column (1), which shows the raw nationwide correlation without controls, finds a strong and significant effect of having voted pro-government in the 2013 presidential election. The estimated effect—four additional transformers selected for LMCP per 100,000 people—is large and economically significant, at 20% of the baseline of 20 transformers in opposition areas. This effect remains positive and large when controlling for ward characteristics (column 2) and restricting the sample to only rural areas (column 3), though it is no longer statistically significant in these specifications. Applying the Double LASSO Variable Selection procedure, Column (4) shows a strong and significant effect of

Table 2.3: Number of Transformers Selected for LMCP per 100,000 inhabitants

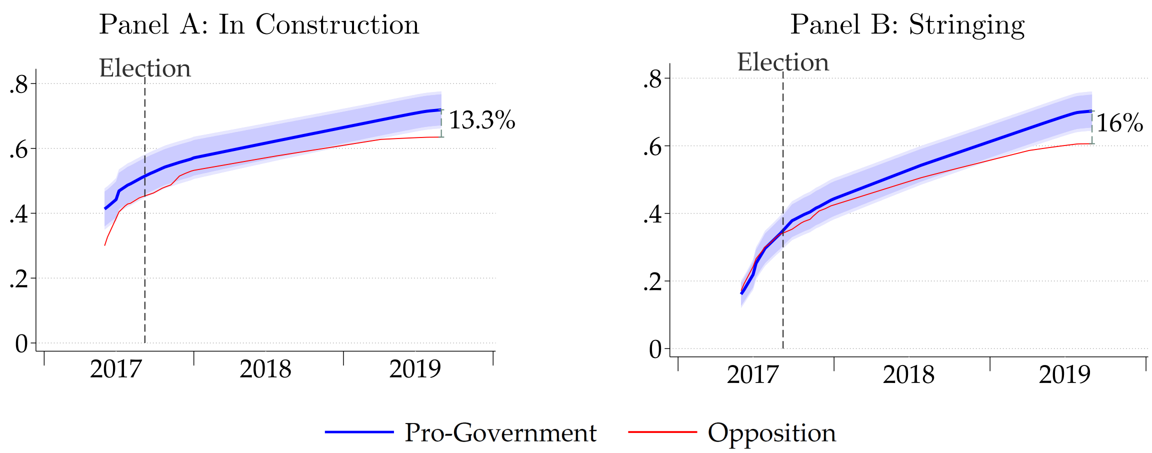
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Voted Pro-Govt in '13	4***	1.9	2.5	4.1***	3.9	1.3	4.3*
	(1.3)	(1.4)	(1.7)	(1.3)	(2.6)	(2.2)	(2.2)
Aligned with MP in '13	0.7	2.2*	1.9	1.8	-2.3	0.4	1.4
	(1.3)	(1.2)	(1.3)	(1.2)	(2.7)	(2.4)	(2.4)
Opposition Mean	[19.9]	[19.9]	[19.9]	[19.9]	[18.7]	[18.7]	[18.7]
Treatment Effect	19.6%	9.5%	12.6%	20.1%	20.1%	7%	23%
Method	OLS Raw	OLS Controls	OLS Controls	Double Lasso	OLS Raw	OLS Controls	Double Lasso
Adjacent Wards Only	No	No	No	No	Yes	Yes	Yes
Rural Only	No	No	Yes	No	No	No	No
Number Observations	1,009	1,009	872	1,009	341	341	341
R^2	0.010	0.228	0.159	0.323	0.007	0.262	0.452

Note: Regression at the ward level. The outcome is the number of LMCP transformers per 100,000 inhabitants. 'Voted Pro-Govt in '13'=1 if Kenyatta obtained over 50% of the Ward votes in 2013. 'Aligned with MP in '13'=1 if the Ward voted for the winning MP in 2013. Ward level controls: shares of adults with primary and secondary education, share of households with electricity, share of adults who work for pay, dependency ratio, share of households with a corrugated iron roof, ward area, household size, and being an urban ward. Controls originally at the transformer level which where averaged at the ward include gradient, granular population density, and meters per person before LMCP. Controls originally at the transformer level which were summed over at the ward level include granular population count, meter count before LMCP, and the number of unconnected households. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

having voted pro-government in the 2013 presidential race, with a similar effect size of four transformers per 100,000 people as in column (1). Finally, columns (5) - (7) confirm that the finding of favoritism in areas that voted for the current president holds when restricting the sample to adjacent wards, although the standard errors are larger due to the smaller sample size. By contrast, these results show mixed and inconclusive evidence of the effect on a ward of being aligned with the local MP.

This favoritism is largely driven by a large and significant bias in the initial distribution of transformers towards pro-government areas. By contrast, in most specifications, transformers in pro-government wards are slightly (0.9% - 1.6%) less likely to be selected for LMCP. However, given the large initial number of existing transformers in pro-government areas, the combined effect is still sizeable favoritism for pro-government wards. Pro-government areas had 16% to 19% more transformers per capita, and between 7% and 23% more LMCP sites per capita, than opposition wards, even after using alternative estimation strategies and accounting for potential unobservables.

Figure 2.6: Share of LMCP sites in each stage of construction



This figure plots coefficients from equation 2.4. Outcome variable: share of sites that reached each construction stage. The red line plots the γ_k's, which are the share of sites that reached each stage in opposition wards. The blue line plots the γ_k's + β_k's, which are the share of sites that reached construction or stringing in pro-government wards. The darker blue is the 90% confidence interval, and the light blue is the 95% confidence interval, of the β_k's, the difference between pro-government and opposition wards. The dashed vertical line represents the August 2017 Presidential election. The national sample has 434 pro-government wards (2,406 transformers) and 509 opposition wards (2,158 transformers).

Construction progress at LMCP sites

Once a transformer has been selected for LMCP, independent contractors are responsible for the design, procurement, and construction of an expanded local low-voltage electricity network at the transformer site. This section investigates the pace of LMCP construction, conditional on having been selected for the program—the third term in equation 2.2. Is the progress of LMCP construction in a ward influenced by how residents voted in the 2013 election?

We answer this question using a nationwide panel of construction data, detailed in Section 2.3. We compare the pace of LMCP's deployment between pro-government and opposition areas using two ward-level measures of construction progress. The first is the share of LMCP sites in a ward that reached each stage of construction—*construction* or *stringing*. The second measure is the total number of LMCP sites that reached a given stage per 100,000 people in a ward—this is the product of the first three terms in the decomposition in equation 2.2. In equation 2.4, these outcomes are represented by y_{it}:

$$y_{it} = \sum_{k=1}^{118} \gamma_k D_{it}^k + \sum_{k=1}^{118} \beta_k D_{it}^k * ProGovernment_i + \delta X_i + \eta_{it} \quad (2.4)$$

where D_{it}^ks are indicator variables which equal 1 when $t = k$ and 0 otherwise; $ProGovernment_i$ is an indicator for whether a majority of ward i voted pro-government in 2013; and X_i is a set of ward-level controls that include socioeconomic status, demographics, and geography. The error term η_{it} is allowed to be correlated within wards over time.

Table 2.4: Percent of a Ward's LMCP transformers in progress, by 2013 Ward election result

	(1)	Construction Started (2)	(3)	(4)	Stringing in Progress (5)	(6)
Ward voted pro-govt in 2013	2.54	2.54	1.65	0.47	0.47	-4.56
	(2.96)	(2.96)	(3.33)	(2.74)	(2.74)	(2.87)
Sample week [0-1]		24.81***	23.89***		40.18***	34.91***
		(1.08)	(1.34)		(1.25)	(1.49)
Interaction (pro-govt X week)			1.76			10.05***
			(2.15)			(2.42)
Observations	109268	109268	109268	109268	109268	109268
Control Mean	55.44	55.44	55.44	46.79	46.79	46.79
Treatment Effect	4.6%	4.6%		1%	1%	
Week Control	FE	Continuous	Continuous	FE	Continuous	Continuous
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The outcome variable is the percentage of LMCP sites in each ward that have progressed to at least the indicated stage of construction (either construction started or stringing in progress). Standard errors are clustered by ward and are shown in parentheses. Observations are weighted by ward population. Versions of this table are available on a per capita basis (Table B.2), by number of transformers (Table B.3), or as a share of LMCP transformers in the ward (Table B.4). When Week Control="FE" the specification includes week fixed effects. The variable 'Sample week' equals 0 in the first week of the sample and 1 in the last week of the sample, increasing in linear increments over the interval. Controls include share of adults with primary education, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, and being an urban ward. * ≤ 0.10,** ≤ .05,*** ≤ .01.

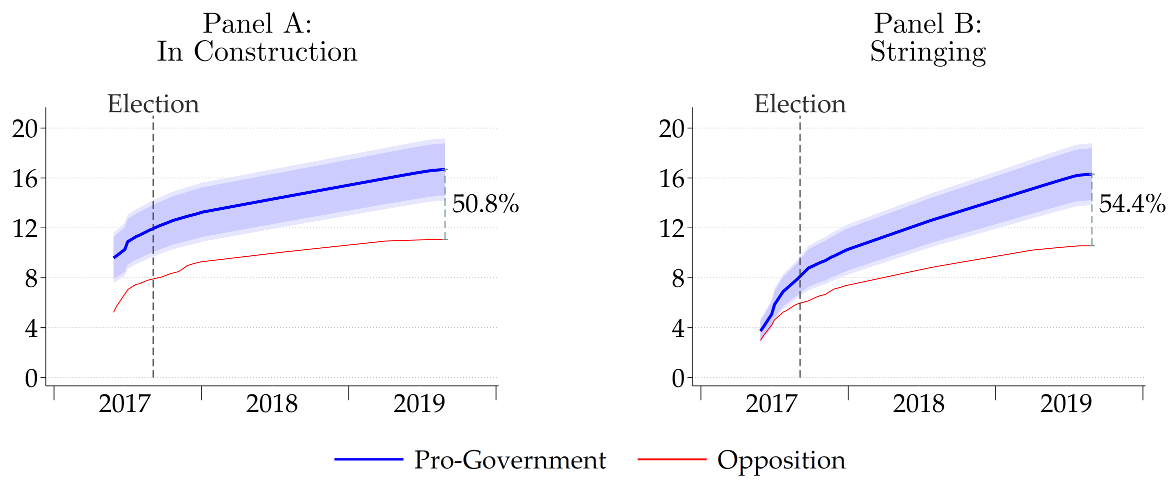
Figure 2.6 presents estimates for the share of LMCP sites in each stage of construction in graphical form, omitting the controls X_i to have a visual representation of the actual gap. Conditioning on being selected for LMCP, Panel A shows pro-government bias in construction progress, and Panel B shows pro-government bias in stringing progress. Although the difference in shares of LMCP sites is not significant at the beginning of the sample, this gap widens over time. Panel A of Figure B.2 confirms that this pattern holds when limiting the sample to adjacent wards, although the smaller sample does increase the standard errors.

Table 2.4 estimates equation 2.4 in continuous form. For each progress outcome (construction started or stringing in progress), columns 1 and 4 consider a regression only on a 2013 pro-government majority indicator. Columns 2 and 5 include a linear time trend. And columns 3 and 6 add an interaction between the time trend and the pro-government indicator. All regressions include socio-economic and geographic controls.

The pro-government indicator variables are small and insignificant in all regressions, suggesting no baseline level difference. However, stringing occurs significantly more rapidly in pro-government wards—at the end of the construction period, 5% more LMCP sites had at least begun stringing in pro-government wards than in opposition wards (column 6).

Figure 2.7 combines the first three components of equation 2.2 to compare the aggregate

Figure 2.7: Construction progress per 100,000 residents



Coefficients from equation 2.5 for the nationwide sample. The red line plots the γ_k's (sites per 100,000 people in construction or stringing in opposition wards). The blue line plots $\gamma_k + \beta_k$ (meters per 100,000 people in pro-government wards). The blue shaded area is the confidence interval of the β_k's, the difference between pro-government and opposition wards. The darker blue is the 90% confidence interval, and the light blue is the 95% confidence interval. The dashed vertical line represents the August 2017 Presidential election. The national sample has 344 pro-government wards (1,735 transformers) and 386 opposition wards (1,496 transformers).

impact of political favoritism on the number of sites that started construction (Panel A) and stringing (Panel B) per 100,000 people. There are large and persistent gaps in the number of sites under construction between pro-government and opposition wards. At the start of the LMCP, pro-government wards have almost twice the number of sites under construction—9.6 sites on average in pro-government wards, compared to just 5.2 in opposition wards. This gap remains significant throughout the sample period. A similar gap exists in the number of sites that reached stringing, but with a different evolution over time. The gap between pro-government and opposition wards is statistically insignificant to start, but grows steadily. By December 2019, 16.3 sites per 100,000 people had reached stringing in pro-government wards, compared to 10.6 sites in opposition ones. Panel B of Figure B.2 confirms that this pattern holds when limiting the sample to adjacent wards, although the smaller sample does increase the standard errors.

Despite a small degree of convergence in the selection of LMCP sites, the large combined degree of favoritism in the existing distribution of transformers and in the commencement of construction and stringing progress results in a large construction progress gap between pro-government and opposition areas.

Meter completion

The final stage of connecting a household to electricity is connecting a household meter to the grid. Households need a meter to access electricity from the grid. This can be thought of as the end product of the four parts of construction progress in equation 2.2. While private contractors were responsible for site construction, Kenya Power was responsible for connecting household meters. Table 2.5 estimates the following equation:

$$y_{it} = \sum_{k=1}^{118} \gamma_k D_{it}^k + \sum_{k=1}^{118} \beta_k D_{it}^k * ProGovernment_i + \delta X_i + \eta_{it} \tag{2.5}$$

where y_{it} is the number of meters at transformer i in week t, restricting the sample to transformers where construction had started by the end of the sample period. The D_{it}^ks are indicator variables which equal 1 when $t = k$ and 0 otherwise; $ProGovernment_i$ is an indicator for if a majority of ward i voted pro-government in 2013; and X_i is a set of ward-level controls that include socioeconomic status, demographics, and geography. As before, the error term η_{it} is allowed to be correlated within wards over time.

In Table 2.5, columns 1-3 estimate equation 2.5 without ward-level controls, while columns 4-6 includes them. Columns 1 and 4 consider a regression on just the voting pro-government in 2013 indicator, columns 2 and 5 add a linear time trend, and columns 3 and 6 include interaction between the time trend and the voting pro-government indicator. Overall, these results provide little evidence that transformers with construction progress in pro-government areas received more meter installations.

Table B.5 conducts a placebo test that examines political bias in the number of meters installed at these transformer sites in 2015 or earlier. During this period, residential meters were issued only when individual households apply for a connection and paid the full connection fee of approximately USD 350, without a government subsidy for the connection cost. In a pattern that lends support to the assumptions of the econometric identification strategy, once observables are controlled for, there are no significant differences in meters per LMCP transformer between pro-government and opposition areas. In other words, there is no evidence of additional private demand for residential electricity connections in pro-government areas, suggesting limited differences in underlying fundamentals.

Aggregate favoritism in household connectivity

Table 2.6 combines all four components of electrification and investigates the overall influence of favoritism on the aggregate number of meters per capita in each ward. The results provide strong evidence of favoritism in the deployment of LMCP towards residents of wards that voted pro-government in 2013. In December 2019, wards that voted pro-government had 37.5% more meters per 100,000 people than wards that voted for the opposition. Column (1) presents the raw correlation. Columns (2) through (6) confirm that these results hold in alternative specifications, including when socioeconomic characteristics of the ward are controlled for.

Table 2.5: Meters per transformer that saw LMCP construction, by 2013 Ward election result

	(1)	(2)	(3)	(4)	(5)	(6)
Ward voted pro-govt in 2013	-1.55	-1.55	0.27	0.07	0.11	2.20*
	(1.69)	(1.69)	(0.52)	(2.26)	(2.26)	(1.18)
Sample month [0-1]		21.70***	23.75***		25.25***	28.07***
		(1.32)	(1.74)		(1.84)	(2.36)
Interaction (pro-govt X month)			-3.63			-4.71
			(2.58)			(3.37)
Observations	54144	54144	54144	38524	38524	38524
Control Mean	14.33	14.33	14.33	14.33	14.33	14.33
Treatment Effect	-10.8%	-10.8%		0.5%	0.8%	
Month Control	FE	Continuous	Continuous	FE	Continuous	Continuous
Controls	No	No	No	Yes	Yes	Yes

The outcome variable is the number of meters installed within 1000 meters of each LMCP transformer since January 2016. Standard errors are clustered by ward and are shown in parentheses. Observations are weighted by number of transformers. When Month Control="FE" the specification includes month fixed effects. The variable 'Sample month' equals 0 in the first month of the sample and equals 1 in the last month of the sample, increasing in linear increments over the interval. Controls include share of adults with primary education, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, being an urban ward, and ward population. $^{*} \leq 0.10,^{**} \leq .05,^{***} \leq .01$.

These magnitudes of favoritism in 2008-2020 may be compared to the related findings from Burgess et al. (2015) regarding favoritism in road construction in Kenya from 1963-2011. During periods of autocratic rule, they find that road expenditures were 100-200% higher and kilometers of paved road constructed 200-400% greater in districts aligned with the president, compared to the national average. By contrast, during periods of democratic rule, they find positive but small and not statistically significant levels of favoritism in road construction. The extent of political favoritism in mass electrification under LMCP during the last decade appears to be an order of magnitude smaller than historical levels of favoritism for large infrastructure investment in Kenya under autocratic rule, and more similar to magnitudes of favoritism in roads investment during more democratic periods. Given the observed patterns of favoritism, several forces are likely important in constraining the use of executive power to favor political supporters.

2.5 Mechanisms

The aggregate favoritism in meter connections is driven by two of the four construction stages: construction of transformers between 2008-2015, and construction progress among

Table 2.6: Meters per 100,000 residents, by 2013 Ward election result

	(1)	(2)	(3)	(4)	(5)	(6)
Ward voted pro-govt in 2013	94.74**	94.74**	33.17***	124.14**	124.73**	54.43*
	(42.40)	(42.38)	(12.83)	(60.66)	(60.64)	(28.43)
Sample month [0-1]		456.74***	400.30***		603.79***	527.08***
		(30.88)	(33.82)		(48.06)	(53.10)
Interaction (pro-govt X month)			123.14*			159.65*
			(63.80)			(93.28)
Observations	59424	59424	59424	41027	41027	41027
Control Mean	274.42	274.42	274.42	274.42	274.42	274.42
Treatment Effect	34.5%	34.5%		45.2%	45.5%	
Month Control	FE	Continuous	Continuous	FE	Continuous	Continuous
Controls	No	No	No	Yes	Yes	Yes

The outcome variable is the number of meters installed at an LMCP site per 100,000 inhabitants in a ward. Standard errors are clustered by ward and are shown in parentheses. Observations weighted by ward population. When Month Control="FE" the specification includes month fixed effects. The variable 'Sample month' equals 0 in the first month of the sample and 1 in the last month of the sample, increasing in linear increments over the interval. Controls include shares of adults with primary, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, being an urban ward, and ward population. $^{*} \leq 0.10,^{**} \leq .05,^{***} \leq .01$.

LMCP sites. This difference was not overturned by the modest favoritism toward opposition areas in LMCP site selection from among the set of transformers. And, conditional on stringing progress there was notably little evidence of bias in metering. In this section we explore the mechanisms through which these two construction stages enabled favoritism, while the other two constrained it.

Media scrutiny

LMCP site selection was highly visible, with sites publicly announced in national and local news, and the entire LMCP project highly publicized as a cohesive government endeavor. With the rapid rise in on-line media, citizens had direct access to the lists of sites that had been selected for LMCP. Flagrant favoritism along these highly visible margins therefore became politically infeasible. In contrast, on-the-ground construction progress is much less visible and difficult to track. While media accounts appear to report on LMCP construction progress sporadically in certain areas, systematic tracking of construction progress nationwide is much more difficult—even for government officials—with sites scattered across the country in rural areas, and actual site visits and construction being completed by a numerous private contractors and sub-contractors. Our finding of faster construction progress at LMCP sites in pro-government areas—but not biased selection in the share of LMCP sites—is consistent with this narrative of imperfect visibility. Indeed, one contribution of this paper is to unify construction progress data from disparate sources, to obtain for the first time a complete picture of the status of LMCP construction across Kenya over time.

Finally, we explore why there was less favoritism in metering conditional on construction progress. One explanation—that households benefit from construction progress in the absence of installed meters through illegal connections—can be ruled out, as there is little qualitative evidence of this as a widespread phenomenon outside of informal settlements in urban areas. Instead, visible construction progress—erecting poles and stringing low-voltage lines are highly visible in local areas—may have been used as a form of electoral inducement, and there may have been an implicit exchange of votes for further completion of the site. Partial construction demonstrates a government's potential capacity to provide public goods, but also creates space for a campaign promise, that construction can only be completed if recipients vote for the incumbent.

International donor oversight

The changing involvement of international financing agencies may explain why favoritism influence site selection in the 2008-2015 push for transformer construction, but not in site selection for inclusion in the LMCP program. Exploiting rich primary data, Berkouwer et al. (2018) document that the site selection process for transformer construction between 2008 and 2010 was conducted with very little involvement from international donors. Instead, REA engaged directly with MPs of the relevant constituencies to solicit their commentary and suggestions for site selection. The REA Strategic Plan 2008-2012 (2008) lists "development partners" as "stakeholders" providing "financial support," but otherwise makes no mention of any specific international donors who contributed financing to the project, nor of their involvement in any part of the construction progress.

The household connectivity portion on the other hand was launched a decade later, and donor oversight increased significantly over the intervening period. When Paul Wolfowitz was appointed as President of the World Bank in 2005, he emphasized its role in cracking down on corruption and clientelism. World Bank-funded electricity construction in Kenya was subject to strict guidelines to *"ensure that the proceeds of any loan are used only for the purposes for which the loan was granted... without regard to political or other noneconomic influences or considerations"* (World Bank 2004). To this end, *"established detailed procedures..., laws, regulations, policies, and implementing rules must promote fairness and thus discourage discrimination and favoritism"* (World Bank 2007). To achieve this in practice, the World Bank separated the various construction stages of the LMCP—designing LV networks at transformer sites, procuring materials, constructing the local network, and installing meters—into independent competitive bidding processes, often further segmented into regional construction groupings. These procedures substantially delayed the construction process with the goal of minimising opportunities for corruption. But in line with the findings above that favoritism can be observed in some parts of the electrification process and not others, the report acknowledges that *"even with competitive bidding, opportunities for corruption abound in procurement activities."*

As a result, the launch of the LMCP was publicly tied to AfDB and World Bank financing and contracting. A 2016 Kenya Power press release informing the public of the LMCP—clarifying, for example, who is eligible and what the fees will be—includes a detailed description of the contracting process: *"This phase will be carried out in semi-turnkey basis where major materials (conductors, poles and transformers) will be procured as goods and will be given out to the contractors who will be awarded the works contracts. The contractors will be expected to buy other small items like stays, fittings and insulators to execute the works. The designs in this phase will be carried out separately by design contractors and once completed, tenders will be floated for works contracts"* (Kenya Power (2016)). Political favoritism in the LMCP is likely to have been constrained by oversight from donors such as the World Bank and AfDB[5] who newly implemented additional regulations to avoid political capture of development projects that they finance.

Checks on executive power

Democratic institutions are also likely to have played a key role in restraining political favoritism. Recent multi-party elections have been seen as fairly competitive, and the balance of power has trended toward decentralization, allowing for better checks on executive power. Along these lines, Opalo (2020b) finds that the advent of multiparty elections decreased the incidence of unilateral executive actions—evidence of increased legislative checks on executive authority. Reforms in the 2000s and 2010s can be seen reflected in measures of democracy in Kenya, such as its Polity IV score, which has increased from 4 in 2001 (an "anocracy") to 7 in 2007 (a "democracy") to 9 in 2013 (where 10 represents a "consolidated democracy"). This may partly explain patterns of favoritism toward pro-government areas in the earlier transformer construction, but a move toward a more equitable distribution of electrification projects later when LMCP sites were selected. Furthermore, the establishment of the Constituency Development Fund in 2003 and the passing of the 2010 Constitution partially shifted political power away from the national government toward members of the legislature and local politicians. Decentralization may provide for some enhanced accountability for the president and central government leaders, even if the local authorities themselves have limited power over large-scale nationwide projects such as LMCP. Indeed, public statements by government officials have emphasized the need for equitable distribution of electrification projects under LMCP, and Kenya Power has stated that LMCP site allocations would be spread throughout the country in a similar fashion as the allocation of Constituency Development Funds.

[5]The AfDB—which funded around half of all LMCP sites—employs a more streamlined 'turn-key' approach, where a single contractor is responsible for the design, procurement, and construction phases, and instead seeks accountability through auditing and monitoring activities. We further explore how donor conditionality affects the quality and timing of infrastructure construction in Berkouwer et al. (2021).

Management

Alternatively, due to numerous missteps in the management of the construction process, the provision of meters may have been inadvertently delayed, making targeting of meter installations an impractical lever. Media accounts support this story: despite electoral pressures to report large numbers of installed meters before the 2017 elections, the installation of many meters was mismanaged.[6]

2.6 Electrification as Political Targeting

Why have pro-government wards received significantly more electricity connections per capita? Recent research in similar settings has found limited short and medium-run impacts of electrification on socio-economic outcomes (Lee et al., 2020b; Burlig and Preonas, 2021). Instead, electrification projects may have electoral benefits. Visible local construction progress in LMCP could have electoral benefits for the current president, as well as other politically aligned leaders, even if it does not significantly impact welfare. In Lee, Miguel, and Wolfram (2020a), for example, households who received an electricity connection had a more positive opinion of the government. In the following section, we examine the electoral impacts of electrification under LMCP, and whether the deployment of the program is consistent with strategic behavior to win more votes.

Impacts of construction progress on votes

Greater LMCP construction progress is associated with more votes for the incumbent, Uhuru Kenyatta, in the 2017 Presidential election. Table 2.7 regresses the incumbent's 2017 vote share in each ward on the number of LMCP sites under construction per capita, controlling for his previous vote share in the 2013 election. An additional 10 sites in construction per 100,000 people is associated with a 0.4 to 1.3 percentage point increase in the incumbent vote share. Similarly, an additional 10 sites undergoing stringing per 100,000 people is associated with a 0.35 percentage point increase in the incumbent vote share. Construction completion, as reported by contractors, does not appear to affect electoral outcomes. While these patterns are somewhat speculative—as shown in previous sections, the placement of infrastructure is highly non-random and selected—they are consistent with the view that politicians may win more votes by increasing provision of visible public goods.

Timing of construction around elections

Patterns in the pace of construction are consistent with strategic behavior around the election cycle. Figure 2.5 shows that metering progress accelerated in the run-up to the August 2017

[6] For example: https://www.standardmedia.co.ke/business/article/2001233193/shocking-kenya-power-details-of-fake-meter-activations-to-please-president-uhuru

Table 2.7: Regressions of 2017 Elections on Progress to the date of Election

	(1) Pro-govt % '17	(2) Pro-govt % '17	(3) Pro-govt % '17	(4) Pro-govt % '17
Sites in Construction	0.043**			0.13**
	(0.020)			(0.061)
Sites in Stringing		0.035*		-0.094
		(0.021)		(0.085)
Sites Completed			0.011	-0.044
			(0.026)	(0.054)
Pro-govt % '13	0.80***	0.80***	0.80***	0.80***
	(0.029)	(0.029)	(0.029)	(0.029)
N	939	939	939	939
R2	0.97	0.97	0.97	0.97
County FE	Yes	Yes	Yes	Yes
Std Errors	Robust	Robust	Robust	Robust

Regressions at the ward level. Outcome: percentage of votes obtained by the incumbent pro-government candidate in 2017. Construction progress variables are sites in each stage by the time of the election, per 100,000 inhabitants. The average number of sites in construction is 10.2, 7.4 in stringing, and 4.4 construction completed. Regressions are weighted by ward population. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

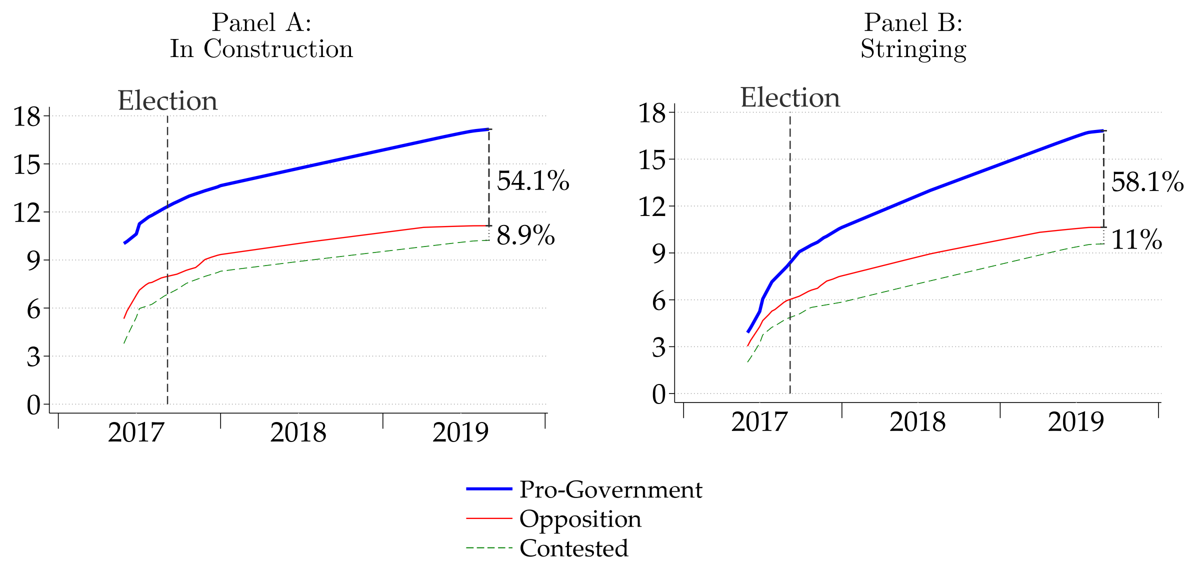
Presidential election in both pro-government and opposition wards, with an average of 350 meters per 100,000 residents added across all wards by August 2017. However, after the election, the pace of construction slowed dramatically, resulting in just an additional 68 meters per 100,000 being added across all wards by the end of our sample in December 2019.

Figure 2.6 shows a similar pattern of acceleration when examining construction progress at transformer sites. Panel B shows that, among LMCP transformer sites, approximately 16% had reached at least the stringing stage between the start of LMCP and April 2017. By the time of the August 2017 elections, in just four months, the total share of sites undergoing stringing had more than doubled, reaching around 36%. However, following the elections, the pace slowed considerably. Over the following year, by August 2018, the share of sites that had reached the stringing stage had risen to just 55%—a similarly sized increase over one year to the one in just 4 months prior to the election. These patterns of acceleration immediately before the 2017 elections hold for both pro-government and opposition areas.

Core Wards and Contested Wards

Figure 2.8 examines construction progress in 'contested' wards, where the 2013 vote share for Kenyatta was between 40% and 60%. Governments may target contested areas if they believe they can gain a higher absolute number of votes in those areas. Instead, differences between pro-government and opposition areas in construction progress are driven by core support areas, rather than contested areas. Core supporters were targeted with greater construction

Figure 2.8: Number of LMCP sites with construction progress per 100,000 residents



This figure compares the number of LMCP sites per 100,000 inhabitants in each ward by how the ward voted in the 2013 presidential election. Pro-government wards (blue) are where Uhuru Kenyatta won greater than 60% of the vote in the 2013 Presidential election, contested wards (green) are wards where his vote share was between 40% and 60%, and opposition wards (red) are where his vote share was less than 40%.

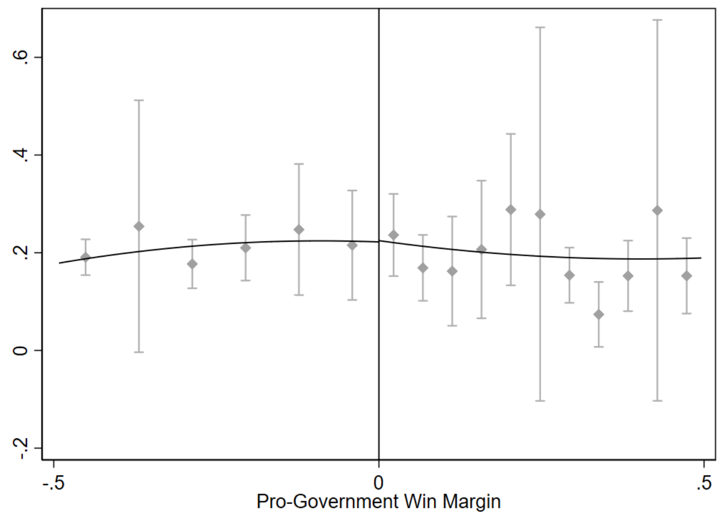
before the election, and rewarded with completed sites after the election. By contrast, swing voters in contested areas were not targeted with faster construction progress. In line with Kenya's nationwide first-past-the-post (FPTP) presidential electoral system—with electoral votes aggregated nationwide rather than first being aggregated FPTP locally—favoritism towards core areas may indicate these have a larger number of marginal votes.

2.7 Local politics

In addition to presidential influence, political favoritism may also operate through other political levels, such as the Members of Parliament (MPs) who represent Kenya's 290 constituencies. Each constituency contains on average four to five wards—the main unit of analysis in the previous sections. Since Kenya Power worked with local MPs to determine the number and location of transformers to be maximized within each constituency, MPs may have been able to exert favoritism in the allocation and construction of sites. In the following section, we consider two political mechanisms by which MPs may have affected local electrification: first, the alignment of each MP with the central government; and second, the electoral alignment of each ward with their constituency's MP.

We first consider whether MPs aligned with the pro-government Jubilee coalition were able to increase the share of their constituency's transformers selected for LMCP. Figure 2.9

Figure 2.9: Share of constituency's transformers selected for LMCP

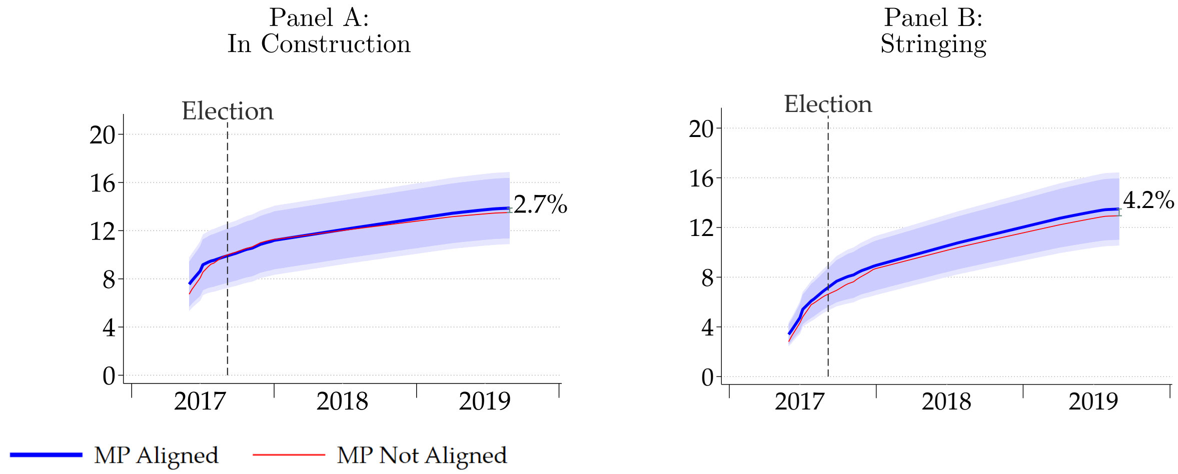


Note: The running variable—pro-government win margin—represents the difference between the vote share of the best performing candidate in a race for Member of Parliament who was in the Jubilee coalition in the 2013 general elections and the winner (if that candidate lost) or the best-performing candidate not in the Jubilee coalition (if that candidate won). Each observation is a constituency. We control for a quadratic trend. Lines represent 95 confidence intervals.

implements a regression discontinuity design which estimates how the vote share difference between the best-performing Jubilee coalition candidate and the best-performing opposing candidate (the running variable) affects the share of selected transformers. Figure B.4 shows the distribution of win margins, which indicates a relatively smooth distribution, with little evidence of bunching and notable mass of electoral outcomes near the 0 mark. We do not find evidence for a discontinuity around 0 as seen in Figure 2.9, suggesting that electing an MP aligned with the central government did not increase a constituency's share of transformers that were selected for LMCP. Similarly, Figure B.3 shows that electing a pro-government MP did not result in greater construction progress by March 2019.

We next consider whether a ward's electoral alignment with its MP shaped the amount of LMCP construction in that ward. Since each constituency consists of multiple wards, wards that voted for the winning MP ("aligned" wards) may have been favored in the deployment of LMCP compared to wards that did not. Figure 2.10 compares construction progress in wards that are aligned with their constituency-level MP to those that are not. Panel A plots the coefficients from equation 2.5 estimated on the national sample of 939 wards, while panel B restricts the sample to the set of constituencies where there is variation in alignment (i.e., at least one ward is aligned and at least one ward is not aligned). There is no evidence that the political alignment of a ward with its MP affects the pace or level of LMCP construction.

Figure 2.10: Construction progress per 100,000 people by Ward's Alignment with MP



Coefficients from equation 2.5. Outcome variable: number of sites in each construction stage per 100,000 inhabitants. The red line plots the γ_k's, which are the number of sites in construction or stringing each week in non-aligned wards. The blue line plots the γ_k's + β_k's, which are the number of sites in construction or stringing each week in aligned wards. The darker blue is the 90% confidence interval, and the light blue is the 95% confidence interval, of the β_k's, the difference between aligned and non-aligned wards. The dashed vertical line represents the August 2017 Presidential election. The national sample has 569 aligned wards (2,884 transformers) and 207 non-aligned wards (980 transformers).

2.8 Conclusion

We find evidence of substantial political favoritism in the Last Mile Connectivity Project, currently Kenya's largest public works program. Wards that voted pro-government in the 2013 Presidential election have 491 meters per 100,000 people, compared to just 357 per 100,000 in opposition wards, a gap of 37.4%. We analyze the sources of this bias by decomposing the connection process into distinct stages of construction, starting with the initial stock of transformers. Pro-government wards started with 19%-38% more transformers per capita than wards that voted for the opposition, potentially due to the fact that this stage was funded by the government of Kenya and therefore experienced little donor oversight. We find a small degree convergence between pro-government and opposition wards in the share of transformers selected for maximization under LMCP, which may have been due to the fact that this process was subject to strict donor requirements and that the lists of sites to be included was scrutinized and publicized in Kenyan media. However, this effect is not large enough to offset the inequality of the initial distribution: the aggregate result is that 16-19% more LMCP transformers are selected per capita in pro-government wards than in opposition wards. This gap in the number of sites selected is further exacerbated by greater construction and stringing progress in pro-government wards, which was difficult to track in part because construction was implemented by dozens of independent private contractors, and is not closed in the provision of meters. The end result is a sizable gap in the number of households connected to electricity between pro-government and opposition wards.

The aggregate degree of political favoritism we estimate is substantially lower—roughly by an order of magnitude—than historical levels of favoritism in Kenyan infrastructure allocation. The continued development of democratic norms, recent political reforms, and oversight from other sources such as foreign donors or local media may have been effective in reducing political favoritism and eliminating the most visible forms of favoritism. Still, the patterns of construction progress that we document suggest that favoritism persists in stages of construction that are difficult to monitor by the public and by international donors, and concerns remain about the inequitable distribution of new infrastructure investments.

While the patterns we document are consistent with construction progress motivated by the election cycle, we also observe relatively low levels of finished construction—with stringing completed and meters installed to allow residents to consume electricity in their homes—in both pro-government and opposition areas. This suggests that existing oversight from different sources to constrain presidential power may not be sufficient to reduce the social costs of unfinished development projects.

Chapter 3

Urban Transit Infrastructure: Spatial Mismatch and Labor Market Power

3.1 Introduction

Urban transit infrastructure projects involve large investments, but their total benefits are hard to measure. How much of the change seen in neighborhoods targeted by them is due to a causal effect on the incumbents rather than by a change in neighborhood composition? Beyond direct efficiency gains from reduced commuting costs, are there indirect benefits coming from reduced labor market power now that workers can substitute more easily between jobs? Lack of individual-level panel data at the time when most subway networks around the world were built has posed a challenge when investigating these questions. This paper aims to answer them. We use a unique employer-employee dataset from Santiago, Chile, that allows us to track workers over time. We circumvent the principal challenge faced by the urban economics literature that has assessed the effect of transit infrastructure on wages: worker sorting vs. efficiency gains.[1]

First, we test if transit infrastructure gives workers better job opportunities and more bargaining power, due to the improved access to labor markets in the city. We compare areas affected by the network expansion to areas that were not affected through a panel event-study leveraging the opening of 84 new subway stations. By combining administrative data on monthly earnings from an unemployment insurance database, and data on the residence location of each worker and the business location of each firm, we obtain reduced-form estimates of the effects of improving market access on wages and work locations. Because we include worker and firm fixed effects in our event-study regressions, our estimates of the effects of infrastructure are net of any sorting caused by the treatment. We answer how much of the change seen in neighborhoods targeted by the infrastructure is due to a causal effect on the incumbents rather than by a change in neighborhood composition.

Second, we build a quantitative spatial equilibrium model in which workers commute and firms exert labor market power over workers. The model serves two purposes: One, it allows us to disentangle the channels behind the reduced-form estimates, and two, it provides a tool to quantify the infrastructure expansion's effect on welfare. The model is based on Monte et al. (2018) and Berger et al. (2019). Its main assumption is that firms behave as oligopsonies in the labor market.[2] With model estimates at hand, we quantify the economic impact of transit improvements considering two channels. First, we measure the efficiency gains from the infrastructure expansion, accounting for the direct benefits of reducing commuting costs and the indirect effects from changing labor market power. Second, given that one of the biggest concerns of economists is the rise of market power and inequality, we measure the effects on the distribution of welfare between firms and workers. The aggregate impact of

[1] The challenge faced by previous work in the urban economics literature is similar to the one faced by the literature that has aimed to understand the gap in labor productivity between the agricultural and non-agricultural sector in low-income countries. In this literature, estimates with individual panel data lead to substantially different policy conclusions. For instance, Hicks, Kleemans, Li, and Miguel (2017) show that including individual fixed effects reduces the estimated urban - rural productivity gaps by as much as 92%.

[2] The assumption of oligopsonies is similar to assuming different Nash-Bargaining parameters in search models (Manning, 2021).

the infrastructure expansion on firm's labor market power can go in either direction. On the one hand, as labor markets become more integrated, more competition among firms for workers reduces labor market power. On the other hand, larger firms may become bigger, increasing their wage-setting ability.

Our reduced-form estimates reveal four facts that then motivate our model: 1) After the subway network expands to connect an additional district, workers that experience an improvement in market access commute longer distances and earn higher wages. 2) After the subway network expands to a district, even workers who live in that district and do not switch jobs start earning more. 3) After the subway network expands to a district, firms in that district start hiring workers from farther away, but pay the same wages on average. 4) Expansions of the subway network lead earnings to converge across space. Specifically, firms start paying workers wages closer to their sector-education-age group average wage after the subway connects the district where the firm is in.

The convergence of earnings across space that we find in fact 4) suggests that there is heterogeneity in the firm's responses to new infrastructure that we find in fact 3). Firms that had little access to workers were paying higher wages to attract them. After being connected, these firms can pay lower wages closer to the city average. In contrast, firms that had access to a lot of workers who in turn were unconnected to other places, were able to pay lower wages and now have to increase them to bring them closer to the city average. The wage equalization we observe is equivalent to the convergence of tradeable-goods prices after trade costs decrease. It also suggests a differentiation of jobs by commuting costs, as theorized by job differentiation models like in Card, Cardoso, Heining, and Kline (2018), which leads to labor market power. Therefore, the previous facts suggest that there are potential winners and losers of labor market integration in a city, and that a model is necessary to assess the overall gains. They also indicate that incorporating labor market power is important to rationalize the gains seen by workers who do not switch jobs, and to account for what seems to be differentiation of jobs due to commuting costs.

Our model with oligopsonistic firms incorporates this firm heterogeneity. The model's main predictions on the welfare impact of new infrastructure depend on two structural parameters: a labor supply elasticity across sectors, and a commuting supply elasticity specific to firms. We describe the limit cases of the model, show that some of its key features are reflected in the data, estimate the key parameters, and simulate it to show that welfare gains from reducing commuting costs are predicted to be significantly larger when accounting for imperfect labor markets.

This paper is closely related to the literature measuring the impact of transportation infrastructure on economic activity. Part of this literature has studied the integration of different regions through railroads, highways, and administrative unification (Faber, 2014; Redding and Sturm, 2008; Donaldson, 2018; Bartelme, 2015; Donaldson and Hornbeck, 2016; Alder, 2016). Other papers have studied property prices and population in cities as a response to various transportation infrastructure improvements (Baum-Snow, 2007; Gonzalez-Navarro and Turner, 2018; Baum-Snow, Brandt, Henderson, Turner, and Zhang, 2017; Gibbons and Machin, 2005; Kahn, Glaeser, and Rappaport, 2008; Billings, 2011; Gupta, Van Nieuwerburgh, and Kontokosta, 2020; Tsivanidis, 2018; Zárate, 2019). The closest paper to ours is Tsivanidis (2018), which measures the welfare gains from Bogotá's bus rapid transit system using rich data at the census tract level. He shows that when taking into account general equilibrium effects and reallocation, welfare gains are 20-40% larger than usual estimates based on time savings alone. Our paper contributes to this literature in two ways. First, we use linked employer-employee data, which allows us to obtain more credible reduced-form estimates. Second, we incorporate labor market power into a quantitative spatial equilibrium model, allowing us to analyze the welfare gains from infrastructure-induced labor market power changes.

The paper also contributes to the spatial mismatch literature. This strand of work started with Kain (1968), who formalized the idea that low black employment was in part due to residential segregation. Hsieh and Moretti (2019) estimate that mismatch across cities in the U.S. due to housing constraints lowered growth by 36% between 1964 and 2009. On the other hand, other papers (Hellerstein, Neumark, and McInerney, 2008; Marinescu and Rathelot, 2018) suggest local mismatch does not play an important role on employment. Closer to the Chilean context, Meneses (2021) studies how the subway network in Santiago expanded educational choices for students, and Carrera and Rojas (2021) finds that having access to the network reduced the negative effects of displacement to the outskirts from camps near the city center. Our paper uses a shock to commuting costs to test how important mismatch is within a city. We find that mismatch plays a role in work decisions, as workers change work locations and earn more when labor market access expands.

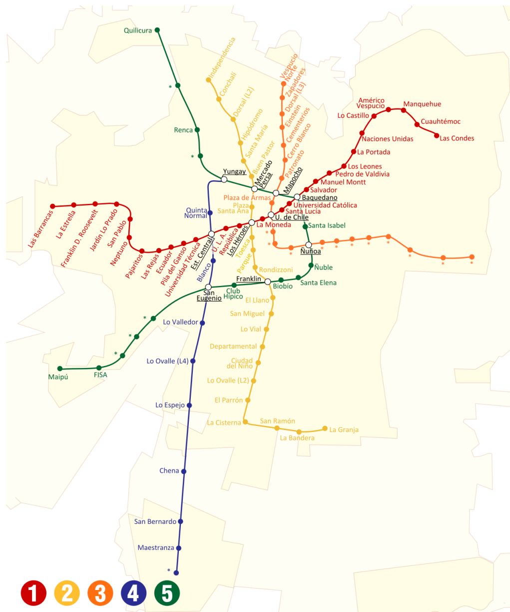
Last, we also build on the growing labor market power literature, which has received increasing attention in the last decade (Staiger, Spetz, and Phibbs, 2010; Dube, Jacobs, Naidu, and Suri, 2020; Naidu, Nyarko, and Wang, 2016; Azar, Marinescu, and Steinbaum, 2017; Azar, Berry, and Marinescu, 2019; Berger et al., 2019; Bhaskar, Manning, and To, 2002; Lamadon, Mogstad, and Setzler, 2019; Hershbein, Macaluso, and Yeh, 2021). We contribute to this literature by quantifying responses to additional labor market integration in a city. Our findings on reduced labor market power due to transportation infrastructure expansion are consistent with those of Brooks, Kaboski, Kondo, Li, and Qian (2021), who find reduced labor markdowns after an infrastructure expansion in India.

The rest of the paper is structured as follows. Section 3.2 narrates the process behind the subway expansion, Section 3.3 describes the different data sources used, Section 3.4 presents the reduced-form empirical strategy and results, section 3.5 lays out the model, and section 3.6 concludes.

3.2 Context About Santiago's Subway Expansion

Santiago is Chile's capital. With a population of 5.6 million, it is the home of 30% of the country's inhabitants. Like many other Latin American cities, it has a central business district (CBD), and other than for a few high-income suburbs, income tends to fall as one moves away from the CBD. Connecting people from the peripheries to jobs downtown has

Figure 3.1: 1968's Subway Network Master Plan



Notes: Santiago's subway Master Plan drawn in 1968 under President Eduardo Frei Montalva.

been the main advertised reason behind the creation of new subway lines, since the initial project was devised in 1968. That year, President Eduardo Frei Montalva signed a decree to begin constructing a subway network in Santiago. Figure 3.1 shows the master plan that was approved, which included five lines covering a large part of Santiago. The first line was inaugurated in 1975, stretching from East to West. During the 80's and 90's construction continued, and by the year 2000, the network had 3 lines, shown in Figure 3.2, panel (a). The network had 52 stations, covering 40 kms, and transported almost 1 million passengers each day. Our analysis starts after this, so the network's extent up to this point is our baseline.

President Ricardo Lagos took office in March 2000, and quickly expressed his intentions of expanding the subway network. His first announcements were on the short extensions of two of the existing lines, but a big addition to the network was yet to be decided. The two most populated districts in Santiago were Puente Alto and Maipú, located in the southeast and southwest areas of the city, respectively. Both district majors were lobbying for their districts to be the next areas connected to the network (Cooperativa, 2001), and finally in 2001, the President announced Line 4 would be constructed, connecting the downtown to the Southeast. Between 2004 and 2006, the extensions to the previous lines and line 4 were inaugurated, making the network extend over 70 kms. Figure 3.2, panel (b) shows what the network looked like after this wave of construction. We refer to this expansion the first wave of expansion.

It is important to note that in early 2007, soon after the first wave of expansions was finished, the bus system in Santiago changed with the creation of Transantiago. A high-profile project, Transantiago completely changed the logic and functioning of Santiago's public transit system. The previous system consisted of 8000 buses (serving 380 routes) owned by competing individual firms who on average owned 2 buses each (Muñoz and Gschwender, 2008). This meant routes where extremely long, sometimes crossing the entire city, and there was on-street competition between buses. Drivers were paid as a share of fares collected. This meant that drivers frequently skipped less busy stops and did not respect student fares (30% of the adult fare). Transantiago reduced the number of firms to 10, each operating in an area and with buses that fed into main "trunk lines" and the Metro. The bus and metro fares were integrated and allowed multiple transitions within 90 minutes, and drivers were not paid based on the fares collected. The purpose was to make the Metro the backbone of the system, and have lower-income groups increase their use of it, which was quite low before the implementation of Transantiago. This turned out to be exactly what happened, with Metro exceeding 1.6 million daily trips. However, the launch of Transantiago was riddled with problems (Muñoz and Gschwender, 2008) which led to initial years of low frequency, crowded buses, and overall longer travel times. For this paper's perspective, this means that we might expect different effects to be found in the first wave of expansions relative to the second and third waves. The initial failure of Transantiago suggests that travel times might not have decreased despiste the subway line expansions, however relative to other neighborhoods without an expansion, residents of places with a new line should indeed have lower commuting costs. Therefore it is not clear if we should expect a smaller or larger effect, but we will show results separately for each wave in the appendix to see if any differences are evident between the waves.

The second wave of expansion was announced in 2005, and it included a sizable extension of one of the existing lines to serve the previous contender district, Maipú, in the southeast. The other extension was shorter and was aimed at reaching the East of the city, an affluent area where many people work. The stations from this second wave were inaugurated between 2010 and 2011; the layout of the extended network can be seen in Figure 3.2, panel (c).

The third and most recent expansion was announced in October 2010. New Lines 3 and 6 would be constructed to serve the North and the West. They were inaugurated between late 2017 and 2019. Figure 3.2, panel (d) shows what the network looks like today. After these three expansion waves, the network has grown from 52 to 136 stations, 40 to 140 kms, and now carries over 2.5 million passengers each day. Figure 3.3, panel (a) shows the current location of the subway stations in the city's districts.

3.3 Data

Data sources. We use data from three sources. Our main data source is an 8% sample of the Unemployment Insurance Database (UID), an employer-employee dataset which records monthly earnings for all private sector formal employees starting in October 2002. It also has information on each worker's date of birth, gender, education, district of residence, and

Figure 3.2: Subway Expansions after 2001

(a) Jan 2001 (b) Jan 2007

(c) Jan 2002 (d) Jan 2020

Notes: Evolution of Santiago's subway network.

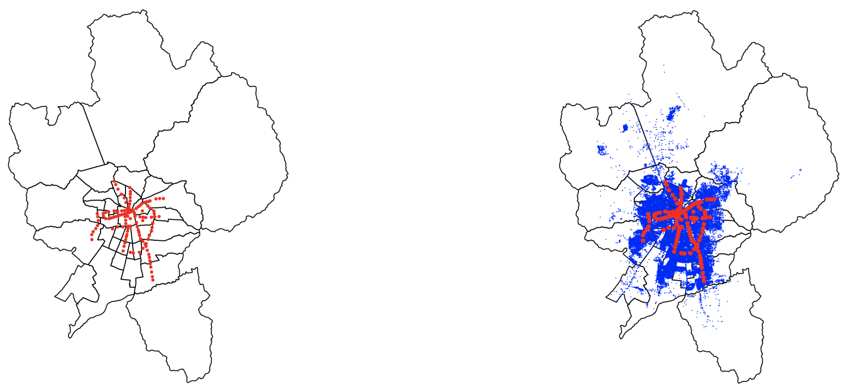
each firm's sector and the district where it's registered.[3]

Table 3.1 shows descriptive statistics on workers from the UID from a cross-section of September 2012. We can see that average monthly earnings at this time were $1,406 USD. This is underestimating average earnings since earnings are top-coded at $4,860 USD. Only 14% of workers work in their district of residence, and almost 50% of them work in firms located in one of the 3 districts with most jobs, which we refer to as "Downtown" from now on. About 15% of workers have any type of college degree (2 year technical or 5 year university). Despite this low percentage, there has been a large increase in college attainment in the younger cohorts.

Our second data source are the 2001 and 2012 Origin-Destination Surveys (OD surveys

[3]We have been granted access to the full dataset, but are in the process of signing the data agreement. Unfortunately, the database does not have establishment level identifiers, so our analysis is at the firm level. Reassuringly, 60% of firms in Chile are categorized as small-medium (up to 4 million USD in annual sales), and these make up 70% of employment. These firms are less likely to be multi-establishment.

Figure 3.3: Districts in Sample



(a) Districts and Subway Stations (b) OD Surveys Origin Points

Notes: Maps of the 38 districts included in our sample. Panel A shows the subway stations up to date, and Panel B also shows the Origin-Destination Survey points used to create representative work trips sample.

Table 3.1: UID Descriptive Statistics - September 2012

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Monthly Earnings (USD)	1,406	1,206	66	4,860
Age	37.4	11.17	16	93
Female	0.39	0.49	0	1
HS Complete	0.82	0.38	0	1
College Complete	0.15	0.36	0	1
Works in District of Residence	0.13	0.33	0	1
Works Downtown	0.5	0.5	0	1
N			**108,889**	

Notes: Descriptive statistics from the Unemployment Insurance Dataset. A cross-section of September 2012.

from now on). These surveys collect the exact coordinates of origin and destination, time, purpose, and transportation mode for thousands of trips in Chile's Metropolitan Region. They are representative at the district level. We restrict our analysis to the 38 districts included in the 2001 OD Survey. Since the surveys' purpose is to characterize commuting in Santiago, the included districts should represent an adequate sample to study the effects of the subway expansion. Figure 3.3, panel (a), shows the 38 districts and the current subway network. Figure 3.3 panel (b), maps all the origin points of trips from both surveys, which we use in Section 3.4.

Our third dataset contains the coordinates of each subway station, along with their opening date.

Commuting statistics and infrastructure effects. Table 3.2 shows statistics on commuting and its evolution between 2001 and 2012. We can see that commutes increased in time and distance, and exhibit large differences across districts. The share of commutes using public transportation slightly increased, but as can be expected, the usage of the subway increased by 200%. By 2012, in some districts, as much as half of all commutes were through the subway. This increase in subway use is in part due to the expansions, but it can also be attributed to the change in the bus system detailed in the previous section.

Table 3.2: Commuting in Santiago

	Mean		**District-level Min–Max**	
Variable	*2001*	*2012*	*2001*	*2012*
Commuting Time (min)	36.67 (25.3)	47.92 (29.5)	22.1–51	27.8–68.6
Commuting Distance (km)	7.27 (6.2)	8.5 (7)	3.5–13.3	4.1–14.4
Used Public Transport	0.49 (0.5)	0.54 (0.5)	0.19–0.67	0.19–0.81
Used Subway	0.08 (0.27)	0.25 (0.43)	0.01–0.22	0.05–0.51
N	18,143	17,331	38	38

Notes: This table shows evolution in commuting patterns in the 38 included districts using the Origin Destination Surveys of 2001 and 2012. Columns 3 and 4 show the minimum and maximum district-level averages

If the subway expansion is to have had any effect on the labor market, it should have reduced commuting times. We use the 2001 and 2012 Origin Destination Surveys to evaluate this. Table 3.3 shows trip-level regressions of commuting time on a dummy equal to 1 if the district or zone where the trip started saw its average distance to the closest subway station reduced by more than 50%[4], controlling for distance to work. Column 1 analyzes these

[4]Appendix Table C.1 performs a similar analysis but using the distance of the trip origins and destinations as a continuous variable.

effects at the district level, defining treatment by district of origin and using 'district of origin-district of destination' fixed effects. Column 2 replicates the analysis at the zone level, dividing Santiago into approximately 400 rectangular zones. Both regressions compare similar trips in 2001 and 2012 and look at how a change in the distance to subway stations affected commuting times, controlling for distance and for overall increases in commuting time. We see that in both specifications, trips in places that got better access to the subway network, experienced a 6% reduction in commuting times, relative to places which did not get better access to the subway network. These results are not surprising, but nevertheless fundamental to believe that the expansion of the subway network could have affected the labor market. We explore the labor market effects in the next section.

Table 3.3: Relationship between distance to subway and work-commuting times

	(1) ln(Trip Duration)	(2) ln(Trip Duration)
Improved Access to Subway	-0.057**	-0.065***
	(0.025)	(0.019)
N	17455	10898
R2	0.53	0.62
OD District FE	Yes	No
OD Zone FE	No	Yes
Distance Control	Yes	Yes
Year FE	Yes	Yes
Std Errors	Cl at OD-District	Cl at OD-Zone

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Notes: These regressions use data from the 2001 and 2012 Origin-Destination surveys. OD District FE are fixed effects for each pair of origin-destination districts. OD Zone FE divides Santiago into 400 rectangular zones, and is a fixed effect for each pair of origin-destination zones. Only work trips that use public transportation at some stage are included in this sample. "Improved Access to Subway" is a dummy equal to 1 if the district or zone saw its average distance to the closest subway station reduced by more than 50%. Results are robust to using a different cutoffs or the continuous measure of the reduction in distance to the closest subway station.

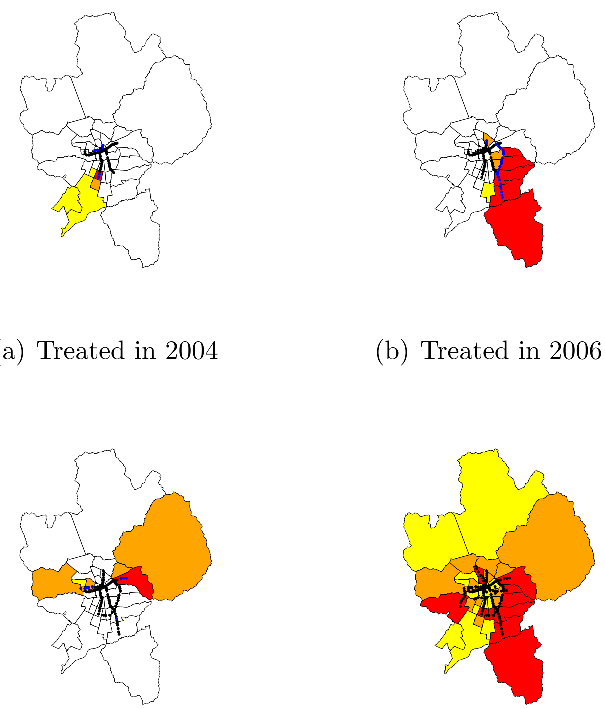
3.4 Reduced-form Evidence

Empirical Strategy

We first present reduced-form evidence on the impact the subway expansion had on affected workers and firms. Thanks to the UID, we can control for worker fixed effects in our estimations, avoiding the problem of worker sorting that has been present in most previous attempts at estimating the effects of transit infrastructure.

We first combine the 2001 and 2012 OD surveys to have a representative sample of work trips from each district. We take the origin coordinates of these trips, shown in Figure 3.3, panel (b), and calculate the distance to the closest subway station that has been opened so

Figure 3.4: Treatments Visualization



(a) Treated in 2004 (b) Treated in 2006

(c) Treated in 2010 (d) All Treatments

Notes: Black dots are existing subway stations. Blue dots are new subway stations. Districts in yellow reduced their distance to the subway by less than 25%, those in orange by 25-50%, and those in red by over 50%.

far, for each month. We then take a district-level average of these distances for each month, obtaining an average minimum distance to the subway for each month, a measure of access to the subway network. We consider the month with the greatest percentage reduction in average minimum distance as the event period for each district. For simplicity of exposition and precision of our estimates, we group all 198 months in our sample into groups of 6 months, and therefore the semester in which the event-month is in is the event-semester[5]. All thirty-eight districts experience some reduction in distance to the subway between 2002 and today, with different intensities. Figure 3.4 shows which districts were treated, and with what intensity, for three of the event-semesters, and overall.

We estimate the following specification relating outcomes to the subway expansion:

[5]Although we refer to them as semesters, they are not calendar semesters because the sample does not begin in January 2002.

$$y_{idt} = \alpha + \beta_{-5-} T_{dt}^{-5-} \times I_d + \sum_{k=-4}^{8} \beta_k T_{dt}^{k} \times I_d + \beta_{9+} T_{dt}^{9+} \times I_d + \lambda_i + \delta_t + \varepsilon_{idt}, \tag{3.1}$$

where y_{idt} is the outcome of worker i, who lives in district d, in month t. The coefficients λ_i are worker fixed effects, and δ_t are month fixed effects. The variables T_{dt}^{k} are district-level event-time dummies, which range from 4 semesters prior to 8 semesters after each event. We exclude the semester prior to the semester of the event to have as baseline. Following the literature on panel event study estimation (Freyaldenhoven, Hansen, and Shapiro, 2019; Schmidheiny and Siegloch, 2019), we bin the event-time dummies beyond this range in T_{dt}^{-5-} and T_{dt}^{9+}, and estimate β_{-5-} and β_{9+} but do not present them in the results. Each event-time dummy is interacted with I_d, which is the percentage reduction in average minimum distance to the subway that took place in the event.[6] This scales each event by the intensity of its treatment, and the interpretation of each coefficient is the effect of a 100% reduction in the distance to the subway. Last, we allow the error terms ε_{idt} to be correlated within district.

Recent work has highlighted the problems that event-study designs have in the presence of dynamic and heterogeneous treatment effects (Abraham and Sun, 2018; Borusyak and Jaravel, 2017). If the first wave of expansions (2004-2006) caused a change in trend in an outcome rather than a jump in levels, this could lead to an implicit estimation of a negative effect on the subsequent waves (because the first wave is used as a control for the following waves), leading to a net zero effect. To deal with this, we estimate equation (3.1) interacting all event-time dummies and month fixed effects with a wave categorical variable. This means we estimate the effects separately (but in the same regression to estimate covariances between coefficients) for each wave, and then combine them according to the share of workers affected by each wave. Since the third wave happened late in our sample, we exclude it from our analysis. Another advantage of estimating the effect for each wave separately, is that we are only exploiting variation in timing within each wave. If the decision of where to expand in each wave is endogenous, but the timing of openings within each wave is orthogonal to the trends in the outcome variables, this reduces the concern of endogeneity.

Results

We summarize the reduced-form evidence into four main results that serve as motivation for our model. Recall that the coefficients from the event study are interpreted as the effect from a 100% decrease in the distance to a subway station. The weighted average reduction of distance in our sample is 42%, and therefore the following figures and discussion of results are scaling the coefficients by 0.42, to represent the effect for the average worker:

Fact 1: *After the subway network expands to a district, workers who live in that district start working farther away and earning more.*

[6] Each district-month has an average minimum distance to the subway network $MD_{d,t}$, so $I_d = \max_t \{ \frac{MD_{d,t} - MD_{d,t-1}}{MD_{d,t-1}} \}$

Figure 3.5, panel (a) shows the coefficients estimated in equation (3.1) using the log of the time to work as the outcome. This measure is the average commuting time by public transportation between districts. We compute these times using the 2001 OD survey, so they are measured before the subway expansion. This means the positive coefficients estimated after the subway expansion do not necessarily mean workers started commuting longer, only that they started commuting to districts where it used to take longer to commute to. We see flat pre-trends and a persistent increase of almost 1% in this outcome. Panel (b) looks at the distance to work by measuring the euclidean distance between the centroids of the district of residence and the district in which the firm is registered. In the case of workers who work in their district of residence, we use the average distance inside each district calculated using the OD surveys. This measure is more coarse, but we see similar results. It is important to note that both of these measures are very noisy, since we only see a change in the outcome for a worker if they switch jobs to an entirely different district. The dynamic effect in both panels is not surprising, since not all workers search for jobs each period. As natural turnover happens, more workers in affected districts start considering jobs further away, and the average distance to work in the district starts increasing. This suggests that the subway expansion did influence workplace decisions.

Figure 3.6, panel (a) shows the effect on log monthly earnings. We see that workers' earnings increase slightly over 1% 4 years after the subway expansion. The inclusion of worker fixed effects allows us to rule out the possibility that this effect is driven by workers with higher earnings moving to the affected districts. An interpretation of these results is that before the subway expansion, there were jobs that paid more but were not taken due to high commuting costs. With the new infrastructure reducing these costs, workers can now take those jobs and experience higher earnings.

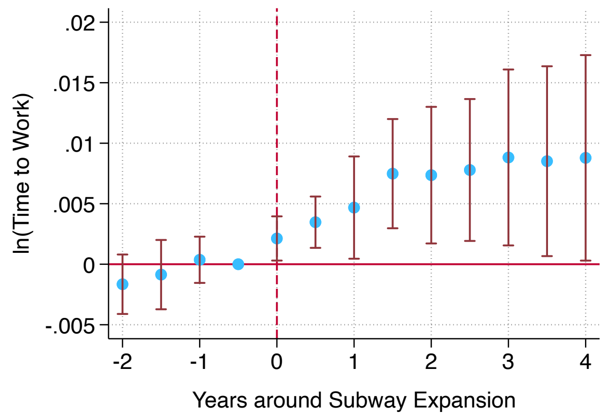
Fact 2: *After the subway expands to a district, workers who live in that district and do not switch jobs start earning more.*

The results in Figure 3.6, panel(a), and in Figure 3.5 show that workers affected by the infrastructure expansion start earning more, and that this effect may be coming from changes in worker's place of work. Nevertheless, we expect that workers who do not change their place of work should benefit from the infrastructure expansion as well. Because the substitutability between jobs of different locations has increased, models of job differentiation such as Card et al. (2018) would predict a decrease in the labor market power of firms. Recent work by Caldwell and Harmon (2019) suggests that an increase in the value of outside options can be enough to cause an increase in earnings, without the worker having to actually change jobs. Both of these suggest that reducing commuting times could increase earnings for workers who do not change jobs.

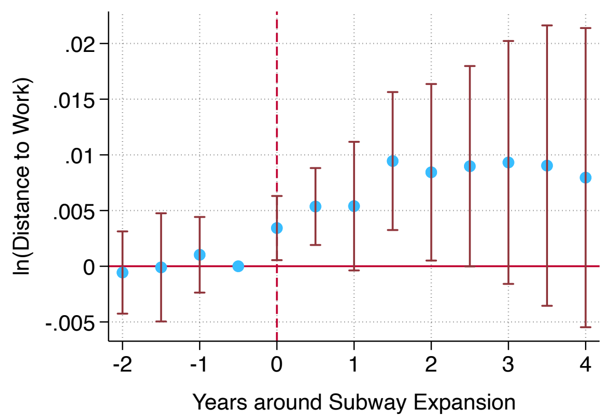
To test this prediction, we estimate equation (3.1) with worker-firm fixed effects instead of worker fixed effects. In practice, this specification estimates the changes in earnings for "stayers", since it exploits changes in earnings within each worker-firm pair. Panel (b) in Figure 3.6 shows the results. We see a similar effect as the one in panel (a). These results are consistent with wage effects from reduced labor market power.

There are, however, alternative explanations for these wage increases that may not be

Figure 3.5: The Effect of Subway Expansion on Workers: Where to work



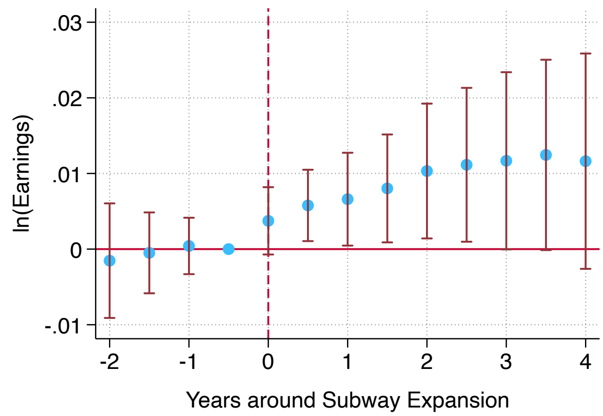
(a) Time to Work



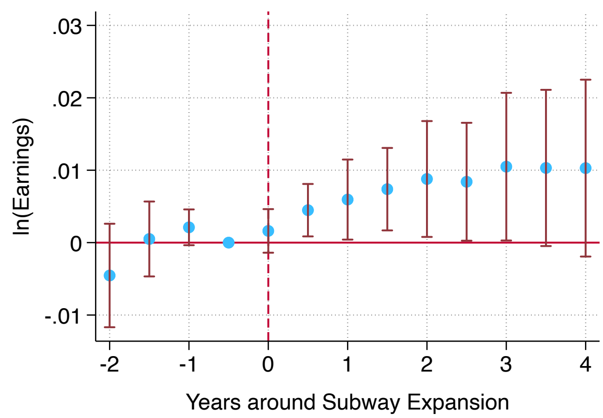
(b) Distance to Work

Notes: Event Study results on distance and time to work. Time to work is estimated before any subway expansion, and therefore is just another measure of distance, does not necessarily imply longer commutes. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure 3.6: The Effect of Subway Expansion on Workers: Earnings



(a) Earnings



(b) Earnings - Stayers

Notes: Event Study results on earnings. Panel A using worker fixed effects, Panel B using worker-firm fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

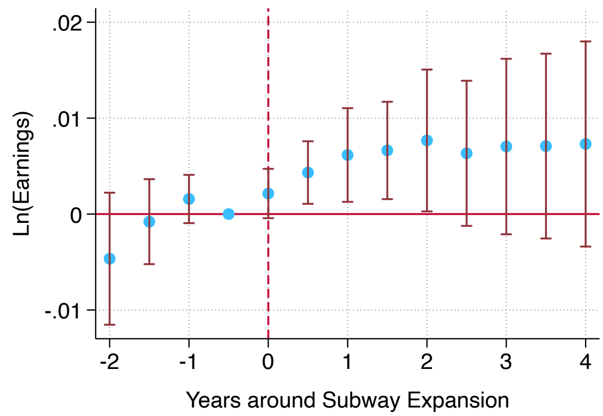
associated with the changes in outside options from reduced commuting costs. For example, the infrastructure expansion may be inducing changes in local labor supply and the composition of the labor force. To the extent that the expanded infrastructure increases the labor supply of high-productivity workers, wages could increase as a result. The reduced commuting and trade costs could also boost the strength of agglomeration externalities and would be consistent with the increased earnings. Last, the infrastructure expansion may be boosting local economic activity at station construction sites, boosting local wages. While in the model we can distinguish between these mechanisms, we only attempt to rule them out in the reduced-form analysis . Figure 3.7 presents estimates that include both worker-firm fixed effects and 'district of firm-sector-month' fixed effects. This means that stayers are only compared to other stayers who work in the same district and sector, but who did not experience a subway expansion in their district of residence yet. We see an effect of a similar magnitude to Figure 3.6, panel (b). If changes in wages were driven by a shift in the labor supply curve, we would expect wages for all workers in a district-sector to change, not only for those who experienced an increase in connectivity to other jobs. Moreover, if the effects were driven by local economic activity changes or agglomeration effects, we would expect to see them for all the workers in the same district of work and sector, and not only for those whose commuting costs decreased.

Another alternative explanation is that reduced commuting times either increased the productivity or hours of work of stayers. Both of these could translate into higher earnings. To test this, we first simulate commuting times between each pair of districts for each moment in time, updating the subway network as it evolves. This allows us to look at each district the semester it is treated, and rank the rest of the districts according to how much the commuting time to each one of them was reduced. To validate this ranking, we divide each ranking into above and below the median, and run an event-study on the probability of working in a district with above-median commuting time reductions. Figure 3.8, panel (a) shows the results. After the subway expands to a district, workers are more likely to start working in one of the districts that saw a greater reduction in commuting times. Having validated the ranking, we then keep only those districts below the median in commuting time reduction for each district. We estimate the event study with worker-firm fixed effects for this subsample. If the effect was driven by workers who experience a sizable reduction in commuting time to their jobs, we should expect this subsample to show smaller or null effects. Figure 3.8, panel (b) shows that this is not the case, suggesting that the reduction in commuting times to their current job is not what is driving the results for stayers.

Fact 3: *After the subway network expands to a district, firms in that district start hiring from further way, but they pay the same in average.*

We estimate equation (3.1) but including firm fixed effects, and defining the event using the distance to the subway in the firms' districts. Figure 3.9, panel (a), shows that firms start employing workers from further away after the event. Four years after their access to subways improves, firms are employing workers who on average live 3% further away than before the subway arrived to their district. Panel (b) looks at the effects on how much firms pay their workers. Although there does not seem to be a change in average wages, the large

Figure 3.7: The Effect of Subway Expansion on Workers: Ruling out Labor Supply



(a) Earnings - Stayers - District of Firm x Sector Fixed Effects

Notes: This event study includes Worker x Firm fixed effects, and Month X District of Firm x Sector fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

standard errors suggest the presence of heterogeneity.

Fact 4: *Earnings converge across space.*

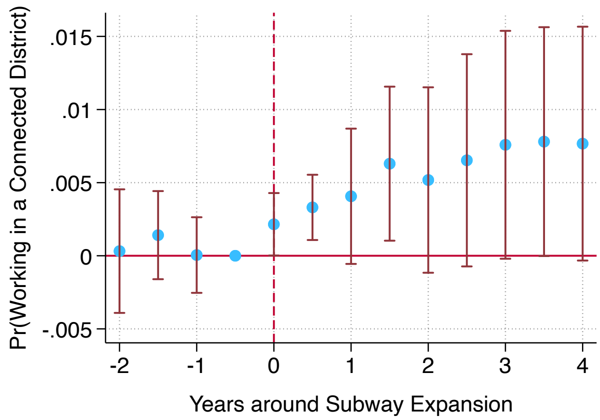
Firms start paying workers wages closer to their sector-education-age group average wage after the subway connects the district where the firm is in. To see this, we compute the average earnings for each sector-education-age group every month, and take each worker's monthly earnings difference with this group average.[7] We estimate equation (3.1) on the log of the absolute value of that difference. The benefit of this specification is the following: firms that have little access to workers are potentially paying higher wages to attract them, but after being connected to more workers, they can pay lower wages, closer to the average. On the other hand, firms that have access to a lot of workers who are unconnected to other places, are able to pay lower wages but after their district is connected need to pay higher wages, closer to the average. This means that in both cases we would expect the gap between a firm's workers' earnings and their group average to decrease unambiguously. Figure 3.10 shows that this is the case. The gap is reduced by approximately 4% two years after the subway arrives at a firm's district.

Robustness

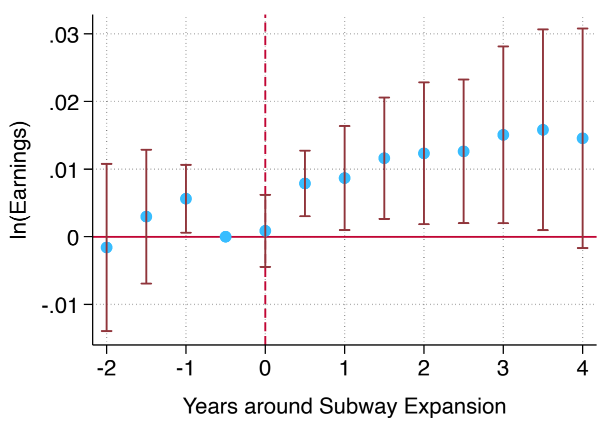
Time Aggregation: To have a better visualization of the pre-trends, Appendix Figures C.1 - C.4 show the main results aggregating months in groups of 3 instead of 6. Our results

[7]We divide education in four categories: no high school, high school, tertiary technical degree, and tertiary university degree. We classify age in 5-year bins.

Figure 3.8: The Effect of Subway Expansion on Workers: Worker Flows and Earnings of Unconnected



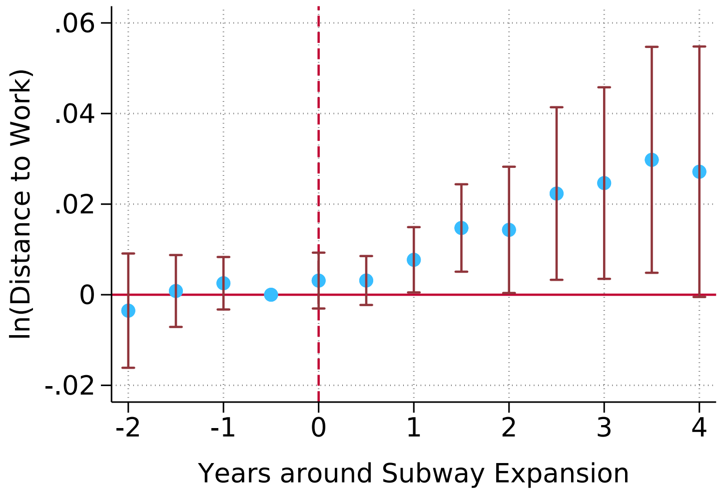
(a) Worker Flows



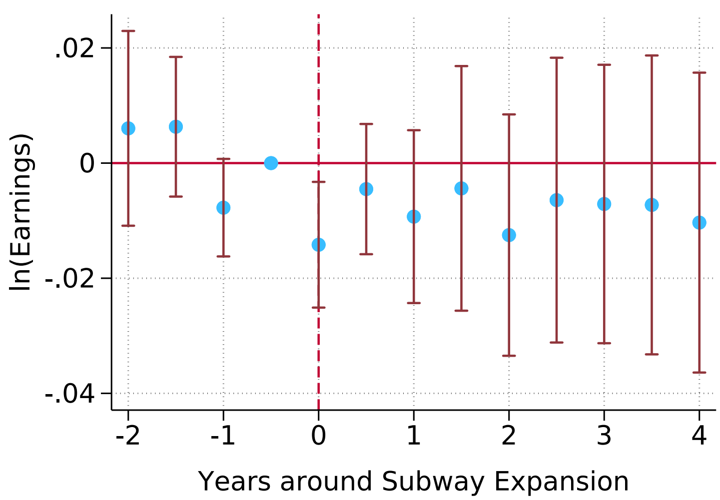
(b) Earnings - Stayers - Unconnected

Notes: For each treated district, we simulate commuting times before and after the treatment to all other districts. Then divide destination districts into above and below the median for each treated district. Above the median districts are referred to as connected districts, below the median as unconnected. Panel A shows that workers are more likely to work in connected districts, and Panel B shows that results on earnings using worker-firm fixed effects hold even for workers who are working in districts that did not get connected. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure 3.9: The Effect of Subway Expansion on Firms



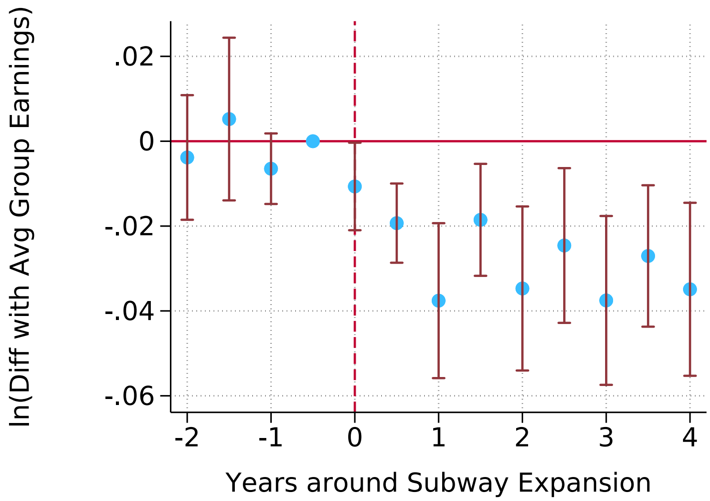
(a) Distance to Work



(b) Earnings

Notes: Event Study results on firms. So the treatment is when the district where the firm is located gets the subway expansion, and regressions are estimated with firm fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure 3.10: Convergence of earnings



Notes: Each month we calculate sector-education-age average earnings. Then we calculte the difference of each worker's monthly earnings with this group average, and run the event study on the log of the absolute value of this difference, from the firms' perspective with firm fixed effects. We see that when the subway reaches the district of a firm, the gap between it's worker's earnings and each worker's group average decreases. Coefficients are scaled by 0.42 to represent the effect on the average worker.

display the same patterns as the main results using 6-month aggregations.

Stacked difference-in-differences: There could still be the concern that within-wave, we are using early-treated districts as controls for later-treated ones, a problem highlighted by Abraham and Sun (2018). To address this, we re-estimate the effects using a stacked difference-in-differences specification. For each wave, we consider districts with a treatment intensity below 30% as "pure" controls. Then, we estimate event studies for each treatment cohort against the corresponding controls, and then aggregate the results according to the number of workers in each regression.[8] This guarantees that the comparisons are always done between districts treated with high intensity vs low intensity, and not between districts with similar intensity but differences in timing. With this specification we can control for differential pre-trends in the regressions on earnings by estimating pre-treatment trends for each treatment-cohort directly and partialling them out of the full panel, as suggested in Bhuller, Havnes, Leuven, and Mogstad (2013); Goodman-Bacon (2021b,a). This specification has 13 treated districts and 5 controls in the first wave, and 6 treated districts and 4 controls in the second wave. Appendix Figures C.5 - C.7 show the main results from this estimation. They suggest that our results are not being driven by problematic definitions of control groups.

[8] In practice, we generate a dataset for each comparison, stack the datasets, and then estimate an event-study regression interacting the worker fixed effects and the month fixed effects with a dataset categorical variable.

Using another region as a control: Another possible concern with our main estimates is the possibility if spillovers across space. The districts we use as controls are likely to be benefiting from the subway expansion as well. For example, even though a new subway line might not reduce the minimum distance from a particular district to the subway (which would imply that the district is not treated), it may still create a faster route to a specific part of the city from that district, reducing commuting times for some workers. With this spillovers pattern, our main regression may underestimate the benefits from receiving access to the subway network.

To tackle this concern, we compare the districts in Santiago to 33 districts from the Bio Bio region, where Concepción, Chile's third largest city is located.[9] We estimate event studies for each treatment cohort against all of the control districts and then aggregate them according to the share of workers in each treatment cohort, again interacting the treatment intensities with the event-time dummies and partialling out pre-treatment linear trends on the earnings regressions. Unfortunately, we do not have travel times between districts for any region outside of Santiago, and therefore we can only look at the Euclideian distance between districts.

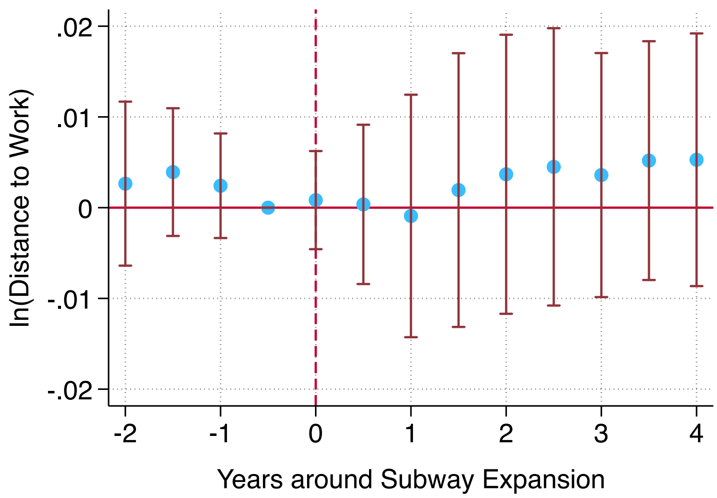
Figures 3.11 - 3.13 shows the results from this analysis. The regression with worker fixed effects and the regression with worker by firm fixed effects both show effects that are larger than those from the within-Santiago analysis. This suggests that the main analysis may underestimate the effects of the infrastructure expansion due to spillover effects.

Randomization inference: We cluster standard errors at the district level in the main analysis. With a small number of districts, out hypotheses test may not have the correct size Bertrand, Duflo, and Mullainathan (2004b); Cameron and Miller (2015). For robustness, we probe the main results using randomization inference. We take the 38 districts, randomize the 38 timing-intensity pairs across them, and estimate the same specification on log wages. Appendix Figure C.8 shows the results. Following Abadie, Diamond, and Hainmueller (2010), we square each coefficient, and compute the average squared coefficient pre and post event. Finally, we calculate the ratio between this post and pre measures. The actual estimates have the 3rd largest ratio out of 60 permutations, putting in the top 5%. This means that if the treatment were meaningless, there would be less than a 5 percent chance of seeing a trend break of the magnitude we are seeing.

Overall, our reduced-form results are quite intuitive. A reduction in commuting costs appears to integrate labor markets, leading to new worker-firm matches and the convergence of earnings across space. When a worker gains access to the subway network, they are more likely to take a job further away, at a higher wage because these locations may be more productive than the locations nearby. On the other hand, our results also suggest that even workers who do not switch jobs obtain positive gains in earnings, and this is not driven by higher productivity or working more hours. There are different mechanisms that can explain this result: larger agglomeration forces, shifts in the labor supply curves, and changes in the

[9]Valparaíso, Chile's second largest city, also built new railway stations during the period of analysis, and therefore is not an ideal control.

Figure 3.11: Another region as control: Where to Work



(a) Distance to Work

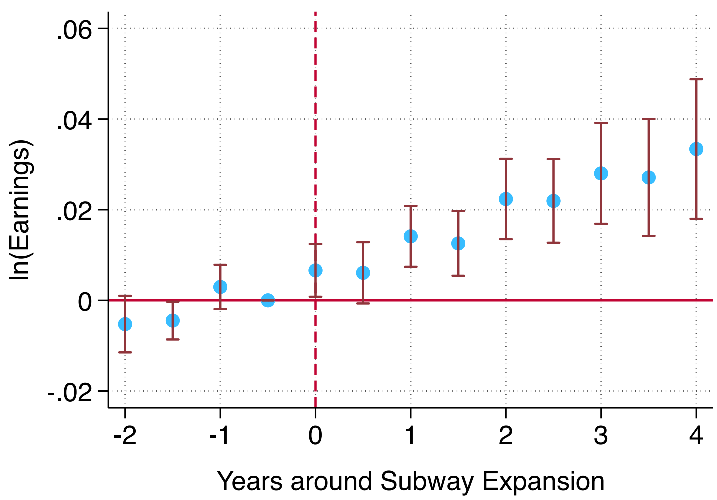
Notes: Stacked Dif-in-Dif using districts from the Bio-Bio region as controls. Coefficients are scaled by 0.42 to represent the effect on the average worker.

bargaining power of firms and workers. In addition, our complementary analysis suggests that there are spillover effects. To disentangle these channels, and to be able to estimate the overall welfare gains of transit infrastructure, we develop a model of oligopsonistic firms that considers all these mechanisms in the next section.

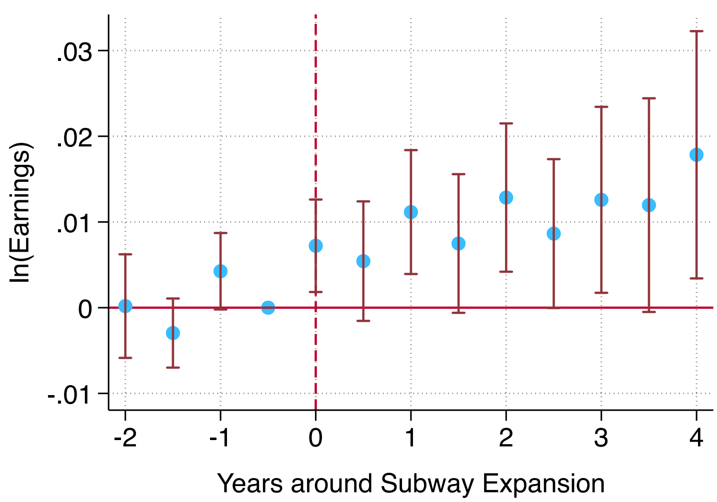
3.5 The Model

In this section, we develop a quantitative spatial equilibrium model with oligopsonistic labor markets. The model has two objectives. First, it provides a framework to explain the economic forces driving the reduced-form results. Second, it allows us to compute the welfare effects of transit improvements through different margins in the labor market. We focus on the effect on wages and rent-sharing parameters between firms and workers. We split the welfare effects of infrastructure expansion into i) the efficiency gains of transit improvements through improved matching between firms and workers and ii) the gains from reduced factor misallocation across firms. We also use the model to measure how the new infrastructure modifies the distribution of surpluses between firms and workers due to the changes in labor market power. The model can be easily extended for different types of workers and to allow migration across locations within the city that for now we have assumed fixed.

Figure 3.12: Another region as control: Earnings



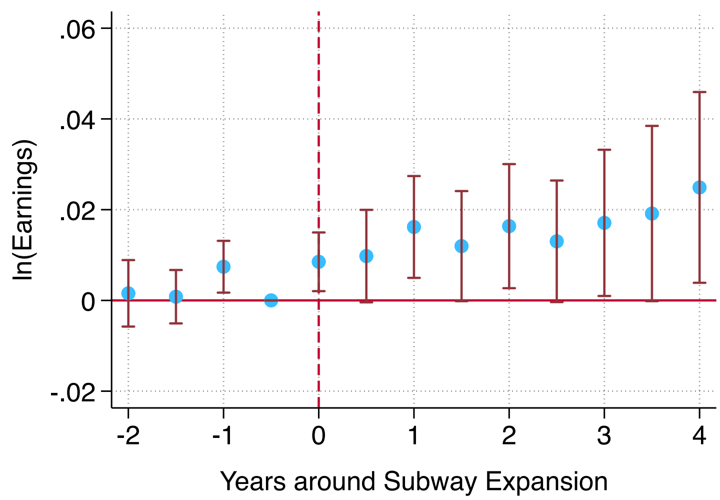
(a) Earnings



(b) Earnings - Stayers

Notes: Stacked Dif-in-Dif using districts from the Bio-Bio region as controls. Panel A uses worker fixed effects, while Panel B includes worker-firm fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure 3.13: Another region as control: Earnings (Robustness)



(a) Ruling out hours/productivity

Notes: Stacked Dif-in-Dif using districts from the Bio-Bio region as controls. The regression includes worker-firm fixed effects, and only includes workers who work in districts that were not connected by the new subway line. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Labor supply

There is a mass of locations $\mathcal{I}$ within a closed city, $\mathcal{S}$ sectors and $\mathcal{F}$ firms where $\mathcal{F} = \cup_s \mathcal{F}_s$ and $\mathcal{F}_s$ represents the set of firms in each sector. The utility of a worker ω who lives in i, works in location j, sector s, and firm f is:

$$U_{\omega isjf} = u_i \epsilon_{\omega ij(f)s(f)f} \left(\frac{C_\omega}{\alpha}\right)^\alpha \left(\frac{H_\omega}{1-\alpha}\right)^{1-\alpha},$$

where C_ω is a consumption aggregator, H_ω is the amount of housing, the parameter $1-\alpha$ represents the expenditure share in housing, u_i is an amenity parameter, and the variable $\epsilon_{\omega ij(f)s(f)}$ is an idiosyncratic shock. Workers allocate their wages $w_{j(f)s(f)f}$, net of commuting costs, towards consumption at price P and housing at price r_i. Given these preferences, indirect utility is given by:

$$V_{\omega ijsf} = \frac{u_i w_{j(f)s(f)f} d_{ij(f)}^{-1} \epsilon_{\omega ij(f)s(f)f}}{P^\alpha r_i^{1-\alpha}}. \tag{3.2}$$

Here, the wages $w_{js(f)f}$ are wage paid per efficiency unit. The parameters $d_{ij} \geq 1$ are iceberg commuting costs, and represent the decrease in efficiency units of labor from commuting. We assume that the idiosyncratic shock $\epsilon_{\omega ij(f)s(f)f}$ affects efficiency units and is drawn from a Nested Fréchet distribution with two nests: i) sector and ii) firm. Conditional on this shock, each agent makes two decisions: the sector to work and the firm within each sector-location. Letting ϵ denote the vector of all the shocks $\epsilon_{\omega ij(f)s(f)f}$, the CDF distribution of ϵ follows an extreme value type II (Fréchet) distribution and is given by:

$$H(\epsilon) = \exp\left[-\sum_s B_{is(f)}\left(\sum_f B_{j(f)s(f)}\epsilon_{ij(f)s(f)f}^{-\beta}\right)^{\frac{\kappa}{\beta}}\right], \quad \text{with} \quad \kappa \leq \beta \tag{3.3}$$

where the parameters β, and κ capture the dispersion of the shocks in each nest.[10] These parameters capture how substitutable jobs are in the two nests. The parameter β represents how easy it is for a worker to substitute jobs across firms within each sector, and the parameter κ measures how easy it is to substitute between jobs across sectors.[11] On the other hand, the parameters $B_{is(f)}$ and $B_{j(f)s(f)}$ are scale parameters that capture amenity or productivity shocks in sector s and firm f. For simplicity, we assume that there is no migration within the city. [12] Given the properties of the Fréchet distribution, it can be shown following McFadden (1978) that the share of workers that are living in i, who decide to work in firm f from sector s at district j is:

$$\lambda_{ij(f)s(f)f} = \underbrace{\frac{B_{is}W_{is}^{\kappa}}{\sum_{s'} B_{is'}W_{is'}^{\kappa}}}_{\text{Prob. of working in sector } s} \quad \underbrace{\frac{B_{j(f)s(f)f}w_{js(f)f}^{\beta}d_{ij}^{-\beta}}{\sum_{f'\in\mathcal{F}_s} B_{j'(f')s'(f')}w_{j'(f')s'(f')f'}^{\beta}d_{ij'(f')}^{-\beta}}}_{\text{Prob of working in } jf \text{ conditional on working in } s}, \tag{3.4}$$

where $W_{is} \equiv \left(\sum_f B_{j(f)s(f)}w_{j(f)s(f)}^{\beta}d_{ij}^{-\beta}\right)^{\frac{1}{\beta}}$ is a wage index for each combination of sector and residence location. It also represents the expected wage conditional on choosing a sector to work in each location i. We drop the dependency of j and s on the firm index f for simplicity from now on.

Labor demand

We assume a wage posting model as in Card et al. (2018), where firms post wages per efficiency units and workers decide where to provide labor depending on the idiosyncratic shock and commuting costs. However, while Card et al. (2018) assume a monopsonistic market structure, we follow Berger et al. (2019) assuming an oligopsonistic market structure.[13] Although it would be hard to provide evidence for this assumption from our current dataset because we do not observe evidence of strategic interactions, the literature has shown that

[10] We could also assume that these parameters vary within each nest as in Zárate (2019).

[11] We will show that this parameter also governs the labor supply elasticity when firm when firms behave like oligopsonies in the labor market.

[12] This can be easily extended adding an additional nest that depends on a migration elasticity.

[13] Berger et al. (2019) follow Atkeson and Burstein (2008) and Edmond, Midrigan, and Xu (2015) and assume this market structure to have a tractable framework to analyze market power responses.

this may be the case in several labor markets. Work from Staiger et al. (2010) showed strategic interaction in the nurses labor market, and recent work by Arnold (2019) shows evidence of these interactions in the US.

We assume that there are several potential entrants M_{js} into each sector and location that draw their productivity from a Pareto distribution $G(A)$. Then, the production function for each firm f in j, s will be given by:

$$Y_{jsf} = A_{jsf} L^{\gamma} \tag{3.5}$$

where A_{jsf} is a productivity parameter that is specific to each firm f that operates in location j and sector s.[14] The parameter $0 < \gamma < 1$ represents decreasing returns to labor. To simplify things we will assume that all firms produce a homogeneous good, and there are no trade costs within the city, which means that the good is freely tradeable. This assumption implies that the price index for the consumption aggregator does not vary across locations. We normalize the price of this good to 1. Then, the problem of firm f is:

$$\max_{w} \pi_{jsf} = Y_{jsf} - w_{jsf} L_{jsf}, \tag{3.6}$$

where $L_{jsf} = \sum_i L_{ijsf}$. Each firm posts a wage assuming that its wage affects wages in the entire city but only within each sector s, or in other words that there is a infinite mass of sectors. Maximizing profits, we obtain that the wage posted by firm f is:

$$w_{jsf} = \left(\frac{\epsilon_{jsf}}{1 + \epsilon_{jsf}} \right) MRPL_{jsf} \tag{3.7}$$

this means that the wage of each firm is a function of the labor supply elasticity (LSE) and the marginal revenue product of labor. The LSE varies across firms and is given by the following expression:

$$\epsilon_{jsf} = \sum_i \theta_{ijsf} \left[\lambda_{ijsf|s} \kappa + (1 - \lambda_{ijsf|s}) \beta \right], \tag{3.8}$$

where θ_{ijsf} corresponds to the share of workers from firm f that live in i. This parameter corresponds to a rent sharing parameter, that captures how much of the marginal revenue product of labor is shared between firms and workers. On the other hand, the parameter $\lambda_{ijsf|s}$ represents the share of workers from location i who work in firm f, conditional on working in sector s. The share of the MRPL that is given to the worker is $\frac{\epsilon_{jsf}}{1+\epsilon_{jsf}}$.

To interpret the model intuitively, it is useful to compare it with the case of Berger et al. (2019). In our case, there are different local labor markets that are represented by the residence location i. All firms in the city compete for workers in each market and the LSE to each firm is a linear combination of the elasticities from each market. For instance, notice

[14] So far, we are abstracting from external economies of scale, which the urban literature has shown to be important. The reason for this is that we want to identify the pro-competitive effects of transit improvements on labor market power in the model.

that the model replicates the case of Berger et al. (2019) when $d_{ij} \to \infty$ for all $i \neq j$, or the case in which the local labor market is the entire city and $d_{ij} = 1$ for all $i, j \in \mathcal{I}$. We now proceed to analyze extreme cases.

Extreme Cases

In this section, we analyze market power and the effects of reducing commuting costs –as expected from the subway expansion– under extreme case such as $\beta \to \infty$, $d_{ij} = 1$ $\forall i, j$; and $d_{ij} \to \infty \forall i \neq j$.

Lemma 1: *Assume that firm f' has higher productivity than firm f'' within sector s and location j, $A_{jsf'} > A_{jsf''}$. Then firm f' has more labor market power than firm f'' in each local labor market i.*

The result follows from the fact that in each local labor market i, more productive firms within each sector s and location j have a higher share of workers, that is, $\lambda_{ijsf'|s} > \lambda_{ijsf''|s}$. For all local labor markets i, $\frac{\partial \epsilon_{ijsf}}{\partial \lambda_{ijsf|s}} \leq 0$ given the assumption that $\kappa \leq \beta$. On the other hand, it is easy to show that all firms within the same j, s have the same share of workers living in i θ_{ijsf} for all local labor markets i since this parameter is only a function of commuting costs. Combining these two results, we obtain that more productive firms face lower LSEs and as a consequence, exert more labor market power.

Lemma 2: *If there is more that one firm in sector s and $\beta \to \infty$, firms do not have labor market power and the model behaves as a model of perfect competition.*

This result follows from Card et al. (2018). If there is more than one firm in sector s, given that firms are differentiated, in each local labor market i, firm f has a share of workers lower than one. Then the LSE ϵ_{ijsf} goes to infinity, implying that the markdown goes to 1. Thus, in this case, the model replicates the perfectly competitive equilibrium in the labor market.

Lemma 3: *In the case in which $d_{ij} \to \infty$ firms only operate in the local labor market in which $i = j$ and exert the highest level of market power.*

This result follows from the fact that firm f will have the largest labor share $\lambda_{ijsf|s}$ when $i = j$. Then, the lowest LSE is obtained when $\theta_{jsf} = 1$ which is exactly the case in which $d_{ij} \to \infty$. Because there is a one to one correspondence between the LSE and the markdown, firm f exerts the highest level of market power in this case.

Lemma 4: *Reductions in commuting costs d_{ij} decrease labor market power for all firms.*

This result is a consequence from the previous lemmas. There are two effects. On the one hand, the increase in commuting costs d_{ij} reduces λ_{ijsf} and θ_{ijsf} in the locations in which firms have more market power, because some workers of local labor market i reallocate to other areas of the city. On the other hand, there is an increase in $\lambda_{i'jsf}$ and $\omega_{i'jsf)}$ in the local labor markets i' in which firms have less market power. Combining the two results we obtain that there is an increase in the LSE ϵ_{jsf} and as a consequence labor market power for all firms decreases.

In the next section, we proceed to decompose the equilibrium welfare in the model to components attributed to different model mechanisms.

Welfare Decomposition

From the properties of the extreme value type shocks, the average wage or workers' welfare in each location i is given by:

$$W_i = \left(\sum_s W_{is}^{\kappa}\right)^{\frac{1}{\kappa}}$$

Following Holmes et al. (2016), with simple algebra we can decompose the welfare in each location i using the following formula:

$$U_i = \underbrace{W_i^{PC}}_{\text{Efficiency term}} \times \underbrace{MD_i}_{\text{Average Markdown}} \times \underbrace{\left(\frac{W_i}{W_i^{PC} MD_i}\right)}_{\text{Allocative efficiency}}, \tag{3.9}$$

where W_i^{PC} is the wage index in location i under perfect competition, MD_i is a term that captures the average markdown that workers from location i face, and W_i is the wage index if firms behave as oligopsonies.

Key Predictions in the Data

There are some key predictions from the model that we can test in the data.

First of all, one of the key features of our model is the functional form of the LSE to the firm, which we repeat for convenience

$$\epsilon_{jsf} = \sum_i \theta_{ijsf} \left[\lambda_{ijsf|s}\kappa + (1 - \lambda_{ijsf|s})\beta\right].$$

It is a standard result that the markdown on wages is higher when the LSE to the firm is low. Our model, through the incorporation of multiple labor markets and oligopsonistic competition between firms, provides a specific conjecture on what this elasticity depends on: the shares θ_{ijsf} and $\lambda_{ijsf|s}$. Recall that θ_{ijsf} is the share of workers within firm f who live in location i, and $\lambda_{ijsf|s}$ is share of workers from location i who work in firm f, conditional on working in sector s. We take values for $\kappa = 1.5$ and $\beta = 7$ from the literature, and attempt to replicate hypothetical scenarios to compare the wages paid by two identical firms with different composition of shares.[15] Intuitively, a firm that hires from many different labor markets will have a higher LSE than a firm that employs a larger share of an specific labor market, since those workers represents a larger share of the firms' employment.

[15]Zárate (2019) and Galle, Rodríguez-Clare, and Yi (2017) for κ and Kline, Petkova, Williams, and Zidar (2019) for β.

Table 3.4 presents results showing that the data is consistent with this model prediction. Column 1 shows the results of a regression relating monthly earnings on the monthly measure of the LSE derived from our model, controlling for firm size, firm fixed effects, and month fixed effects. It is especially important to control for firm size since it is likely to be correlated with our measure of LSE. We can see that within a same firm, a more elastic LSE is associated with higher wages. Column 2 uses worker fixed effects instead of firm fixed effects. It shows that the same worker tends to earn more in a firm with a higher LSE, controlling for its size.

Table 3.4: Relationship between LSE and earnings

	(1) ln(Earnings)	(2) ln(Earnings)
ln(LSE)	0.27**	0.69***
	(0.12)	(0.2)
ln(Firm Size)	-0.06***	0.02***
	(0.003)	(0.004)
N	52,308,062	52,315,973
R2	0.51	0.63
Firm FE	Yes	No
Worker FE	No	Yes
Time FE	Yes	Yes
Std Errors	Cl at Firm	Cl at Firm

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Notes: Two-way fixed effects regressions of monthly earnings on the model's measure of labor supply elasticity of the corresponding firm.

Another way to test this prediction from the model is using a decomposition a la Abowd, Kramarz, and Margolis (1999). This way we obtain a firm effect for each firm, net of worker and time effects. This firm effect can represent higher productivity, but also higher rent-sharing. The first column of table 3.5 presents the results of a regression (employment weighted) relating the firm fixed effects on the LSE, controlling for a fixed effect for the average firm size (in bins of 5). This means this specification compares firms of the same size, but with a different average composition of workers. We see that firms with a more elastic LSE have larger firm effects, which is consistent with them having less labor market power and therefore sharing a larger share of the rents.

The model predicts that the reduction in commuting costs should decrease the labor market power of firms and the dispersion of markdowns. Columns 2 and 3 of Table 3.5 are a first test of this prediction. Both columns shows the results of a regression identical to column 1, but after estimating two separate AKM's, column 2 for 2002-2006, and column 3 for 2012-2016. We can see that in the 2002-2006 period, so mostly before all the new subway expansions happened, LSE is highly correlated with firm effects, however the relationship is lost in 2012-2016. This is consistent when we look at what happened to the actual distribution of LSE's in time. In 02-06, the average LSE (weighted by employment) was 6.83, with a 0.35 standard deviation, and an interquartile range (0.25-0.75) of 6.86-6.99. In

12-16, the average LSE was 6.88, with a standard deviation of 0.28, and an interquartile range of 6.9-6.99. So a lack of correlation in the post period might be due to the fact that there is considerably less variation in LSE's (The standard deviation is 20% smaller despite the mean being larger). A lower dispersion of LSE's is also a prediction of the model and will be a source of efficiency gains through better allocative efficiency since the dispersion of wedges across firms decrease.

Table 3.5: Relationship between LSE and firm effects

	(1) Firm Effect	(2) Firm Effect	(3) Firm Effect
ln(LSE)	0.16**	0.5***	-0.1
	(0.07)	(0.07)	(0.11)
N	54,435,127	6,937,539	21,189,593
R2	0.12	0.1	0.1
Firm Size FE	Yes	Yes	Yes
Std Errors	Cl at Firm	Cl at Firm	Cl at Firm
Time Period	02-16	02-06	12-16

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

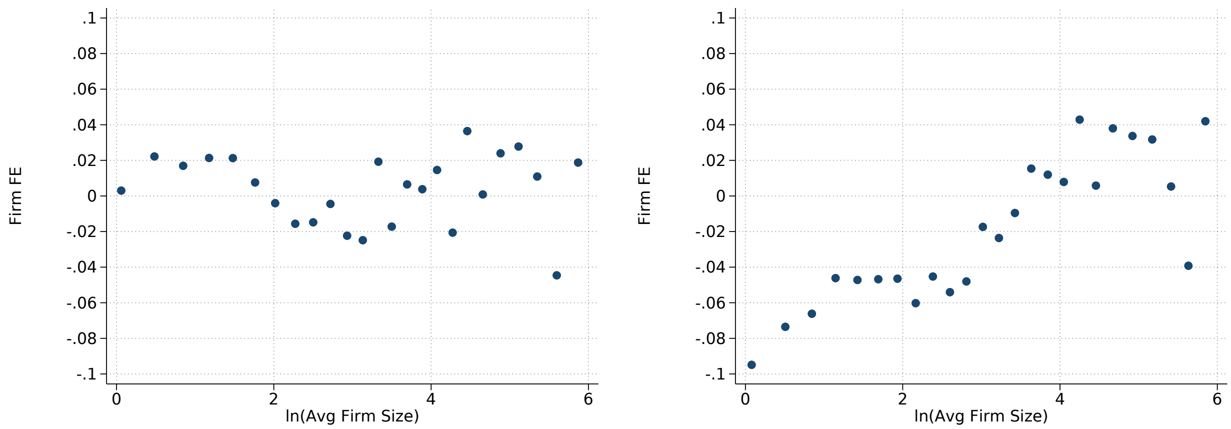
Notes: Regressions of firm effects on the model's measure of labor supply elasticity to the firm. Firm effects are estimated from using an AKM-style regression. Column 1 is from an AKM of the entire sample. Column 2 from an AKM of 2002-2006, and column 3 an AKM of 2012-2016.

Finally, Figure 3.14 shows the correlation between firm size and the firm fixed effects in both periods. The pre-period's relationship is much flatter, consistent with more labor market power. Larger firms don't pay much higher wages (net of worker characteristics). In the post-period, we see a much steeper relationship between firm size and firm effects, consistent with a smaller markdown and a higher pass-through of the firms rents to workers' wages.

Model Quantification

In this section, we quantify and analyze the welfare implications of reducing commuting costs across the different locations within Santiago. Consistent with the model, we show that i) there is an important reduction in labor market power and ii) the welfare implications of transit improvements are larger when we take into account the effect of market power in the labor market. For instance, welfare increases between 20%-50% relative to models with no heterogeneous labor market power across firms such as Tsivanidis (2018) and Ahlfeldt, Redding, Sturm, and Wolf (2015). We also show that workers gain more after the transit shock in detriment of firm owners. These results are robust to different values of the main parameters.

Figure 3.14: Relationship between firm size and firm fixed effect



(a) 02-06 (b) 12-16

Notes: We estimate an AKM model for 2002-2006 and 2012-2016. Then do an employment-weighted binscatter of firm effects on ln(average firm size) during each period, controlling for firm's district fixed effects.

Model Inversion

In the first part of the quantification, we invert the model to recover amenity and productivity parameters. Specifically, with data on wages, the number of workers in each firm, and the number of residents in each location, we can solve the labor supply and labor demand to recover the scale parameters.

Amenity parameters: We recover firm amenity parameters using the total number of workers that work in each firm. Conditional on working in sector s, and using data on wages only explained by the firm, we match the number of workers in firm f with the labor supply and recover the amenity parameters:

$$L_{jsf}^{\text{Data}} = \sum_i \frac{B_{jsf} w_{jsf}^{\beta} d_{ij}^{-\beta}}{\sum_{f' \in \mathcal{F}} B_{j'(f')s'(f')f} w_{j's'(f')f}^{\beta} d_{ij'}^{-\beta}},$$

this equation has a unique solution for the vector of amenity parameters B_{jsf}. After knowing these scale parameters we construct the wage indices for each sector and residence location, $W_{is} = \left(\sum_{f \in \mathcal{F}_i} B_{jsf} w_{jsf}^{\beta} d_{ij^{-\beta}}\right)^{\frac{1}{\beta}}$. Then we can match a sector amenity parameter specific to each residence location. We solve the following system of equations for the amenity parameters B_{is}:

$$L_{is}^{\text{data}} = \frac{B_{is} W_{is}^{\kappa}}{\sum_{s'} B_{is'} W_{is'}^{\kappa}},$$

these amenity parameters represent labor supply shifts that are not explained by the wage indices.[16]

Productivity parameters: We follow a similar procedure to recover the productivity parameters using the labor demand. In particular, we match the number of workers in the data with the ones implied by the model. First, we use the share of workers in the data to construct the labor supply elasticities, and then, we find the productivity parameters solving for the following system of equations:

$$L_{jsf}^{\text{Data}} = \left[\gamma\left(\frac{\epsilon_{jsf}}{1+\epsilon_{jsf}}\right)\frac{A_{jsf}}{w_{jsf}}\right]^{\frac{1}{1-\gamma}}.$$

After knowing the scale parameters we can run the counterfactuals of the transit infrastructure by varying the iceberg commuting costs in the model.

Calibration of Commuting Costs

To calibrate the commuting costs, we follow the standard method from Ahlfeldt et al. (2015). In particular, we parametrize the iceberg commuting costs as a function of travel times using the following equation:

$$d_{ij} = \exp(\delta t_{ij}),$$

where δ is a parameter that transforms travel times into commuting costs and t_{ij} is the travel time from location i to location j. For δ, we use a value of 0.01, which is a standard value used in the literature. On the other hand, to calculate travel times across locations, we use the network analysis of ArcMap and a 1000 random sample of points in the city taking the average across areas. We calculate travel times before and after the subway expansion and run a counterfactual varying the commuting costs.[17]

For the other parameters, we use values from the literature. Table **??** shows the values that we use for the counterfactuals.

Estimation of the Main Parameters

According to the model, there are two main parameters to estimate to understand the effects of transit improvements on labor market power. First, we need to estimate β, which captures how easy is for workers to substitute jobs across firms in the city, and second, we need to estimate κ, which captures how good is to substitute sectors within each residential area. We estimate these parameters through a GMM approach using two moment conditions.

[16] In both equations the parameters are only identified up to a constant (Ahlfeldt et al., 2015), so we need to normalize one of the scale parameters B_{jsf} and B_{is}.

[17] In terms of transportation modes, since we are taking a weighted average across the different transportation modes, the assumption is that preferences for transportation modes follow a Cobb-Douglas structure.

To estimate β, we follow Ahlfeldt et al. (2015) and use the standard deviation of the log wage distribution in the pre-period. One of the properties of the Frechét distributions is that as β increases the dispersion of the idiosyncratic shock is lower and as a result the wage dispersion is lower. The opposite occurs when β decreases. Then, we can match the standard deviation predicted by the data and the model to estimate β. To estimate the variation in wages that is coming from the model we proceed in three different steps. First, we estimate a gravity equation for different transportation modes regressing commuting flows with travel times in each location:

$$\ln \lambda_{ijm} = \underbrace{\mu}_{=\beta\times\delta} t_{ijm} + \gamma_{ij} + \gamma_{im} + \gamma_{jm},$$

where λ_{ijm} is the population share in location i that commute to location j using transportation mode m, and t_{ijm} is the travel time from location i to location j using mode m. The coefficient of interest is β and to include the zeros, we estimate the gravity equation through PPML. The parameter μ is the interaction between two relevant elasticities. First, the commuting elasticity β that captures how easy is to substitute jobs across firms in the city, and second a parameter δ that transforms travel times to commuting costs $d_{ij} = \exp(\delta t_{ij})$. When we invert the model, we just need to know β to invert it and recover and adjusted wage distribution that we use to recover the standard deviation of the log wage distribution predicted by the model. In particular, using the employment measure in each sector we can recover and adjusted measure ω by:

$$L_{jsf}^{\text{Data}} = \sum_i \frac{B_{jsf}\omega_{jsf}\exp(-\mu t_{ij})}{\sum_{f'\in\mathcal{F}} B_{j'(f')s'(f')f}\omega_{j's'f'}\exp(-\mu t_{ij})},$$

Once we recover the adjusted wage distribution ω, we minimize the following moment

$$E\left[\frac{1}{\beta^2}(\ln\omega^2)\right] - \sigma_w^2 = 0, \tag{3.10}$$

Table 3.6 reports the coefficients of the gravity equation. Overall, we find a value of μ of around 0.042. For the second parameter, we use the structure of the model. According to equation 3.4 , we can estimate κ by running the following regression:

$$\Delta\ln\lambda_{is|i} - \Delta\ln\lambda_{is_0|i} = \kappa\left(\Delta\ln W_{is} - \Delta\ln W_{is_0}\right) + \Delta\ln B_{is} - \Delta\ln B_{is0}, \tag{3.11}$$

where s_0 represents a reference sector, and B_{is} an amenity parameter that captures how attractable is a sector within location. Since we do not observe the changes in the amenities parameters we use the following moment condition:

$$\Delta\ln\lambda_{is|i} - \Delta\ln\lambda_{is_0|i} = \beta\left(\Delta\ln W_{is} - \Delta\ln W_{is_0}\right) + \beta X_{is} + \epsilon_{is}, \tag{3.12}$$

$$E\left[\epsilon_{is}Z_{is}\right] = 0,$$

where Z_{is} is the change in the commuter market access across time and taking the difference with respect to a reference sector s_0. This measure only considers changes in commuting costs, and not changes in the spatial distribution of employment and the number of residents. We also include a set of covariates that may capture some of the changes in amenities due to the transit shock.

Figure 3.15 plots the objective function of the GMM. Overall, we find that a value of $\beta \approx 8$ and $\kappa \approx 3$ minimizes the objective function, which are the values that we will use in our analysis.

Table 3.6: Commuting gravity equations

VARIABLES	(1) $\ln \lambda_{ijm}$	(2) $\ln \lambda_{ijm}$	(3) $\ln \lambda_{ijm}$	(4) $\ln \lambda_{ijm}$
time_{ij}	-0.035***	-0.038***	-0.039***	-0.042***
	(0.002)	(0.003)	(0.003)	(0.003)
Transportation mode fe	X			
Origin fe	X		X	
Destination fe	X	X		
Origin-mode fe		X		X
Destination-mode fe			X	X
Observations	3,328	3,328	3,328	3,328
Pseudo R-squared	0.148	0.157	0.163	0.171

Standard errors in parentheses

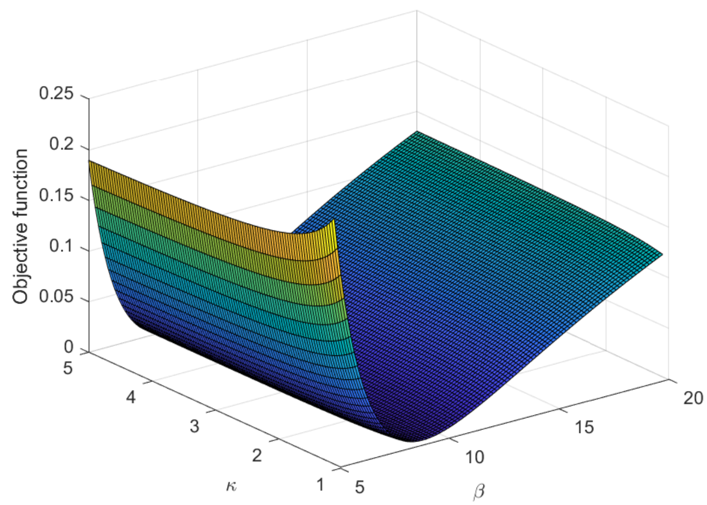
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the results of a gravity equation in which we relate commuting flows in the city with travel times for different transportation modes. We estimate this equation through PPML to include the zeros.

Counterfactual Results

Welfare Analysis: In the first part, we analyze the welfare effects of the subway expansion. Figure 3.16 presents the main result. In panel (a), we plot the distribution of markdowns before and after the shock to study markdown heterogeneity, which is the main determinant for the effect on welfare (Hsieh and Klenow, 2009). In panel (b), we plot the welfare effects under perfect competition and considering markdown responses for different values of β holding κ fixed at 3. In general, the counterfactuals imply that the subway expansion generated welfare gains between 2% and 7% of real income depending on the parameter values. The welfare gains are amplified when we consider markdown responses. For example, in the case of $\beta = 8$, our preferred value, the welfare gains increased by around 55%. Under perfect competition, the welfare gains are 1.9%, but when we consider markdown responses, the welfare gains are 3.1%. This increase is higher as the difference between κ and β increases. The reason behind this amplification in the welfare gains is that the dispersion of markdowns

Figure 3.15: Objective Function-GMM



(a) Objective function

Notes: This figure plots the objective function of the GMM approach to estimate the main parameters of the model.

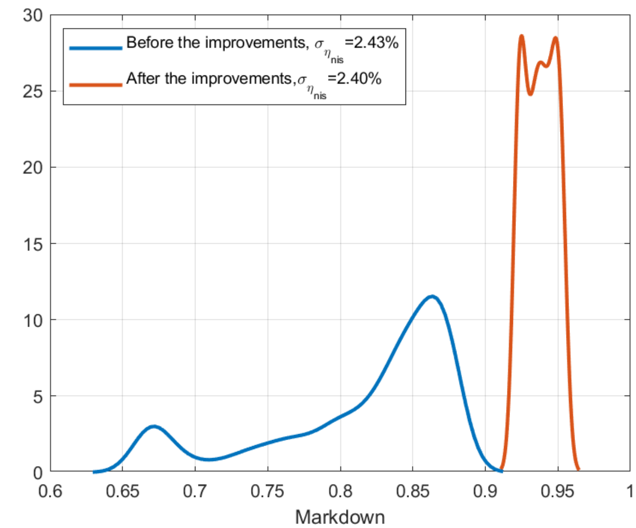
decreases with the subway expansion. For instance, in panel (a) in the pre-period, the standard deviation of the markdown distribution was 2.44%, while in the post-period it is 2.40%.

Distributional Effects: Figure 3.17 plots the main effects in terms of redistribution between firm-owners and workers. In panel (a), we plot the effect on the average markdown for different values of β. The markdown coefficient increases between 2% with a $\beta = 5$ to 10% with a $\beta = 10$, so worker's wages increase and the markdown decreases. This implies that firms' rent sharing parameters are affected significantly by the transit infrastructure. In the base line case with a $\beta = 8$, the average fraction of the MRPL that is redistributed to the worker is 0.80, and the transit shock increases this fraction to 0.85. This means that the transit shock increased the bargaining power of workers by a substantial amount.

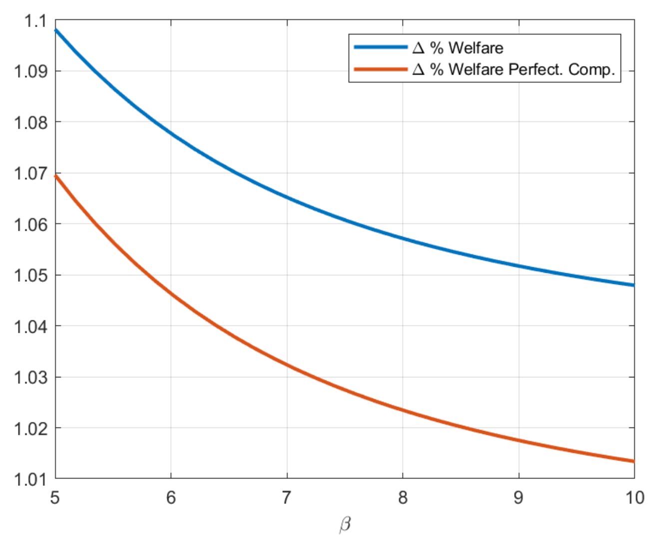
Similarly, in panel (b) we plot the effects for the aggregate variables of income: the aggregate wage bill, the firms' operational profits, and the total income (the sum of the two). In general, we observe that workers gain more from the transit improvements in detriment of firms as the commuting elasticity specific to each firm, β, increases. For example, with a $\beta = 8$ aggregate labor income increases by around 4%, while firm-owners lose 6% of operational profits. Nevertheless, the aggregate effect on total income is positive and it is around 2%.

Overall, the counterfactual results suggest two main conclusions. First, in terms of efficiency, the results imply that considering markdown responses amplify the welfare gains by a considerable proportion, and this result is robust to different values of the commuting

Figure 3.16: Simulation of the subway expansion: Welfare



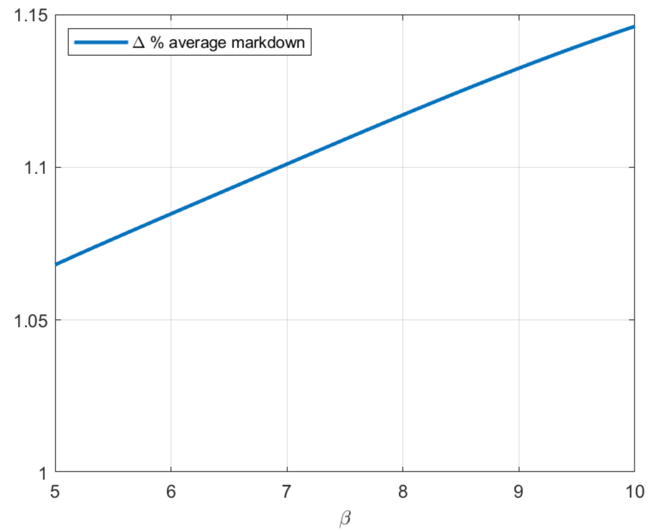
(a) Distribution of Markdowns



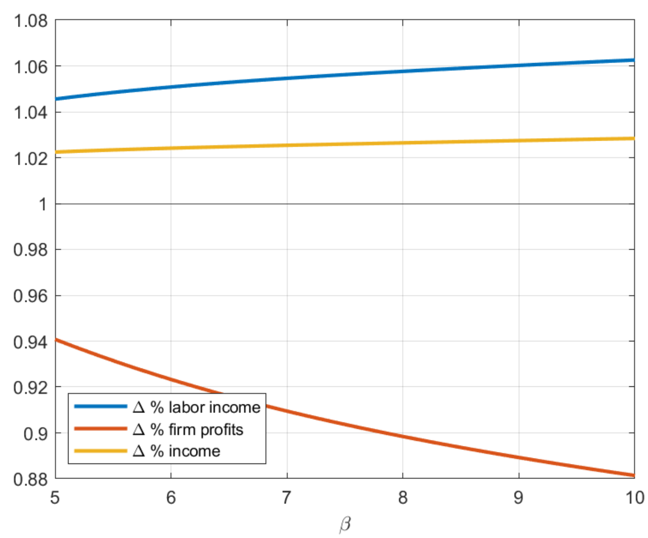
(b) Change in Welfare

Notes: In panel A we simulate how the distribution of markdowns before and after the shock with $\kappa = 3$, and $\beta = 10$. In Panel B we show the change in welfare for different values of β.

Figure 3.17: Simulation of the subway expansion: Distributional Effects



(a) % Change markdown



(b) % Change in income

Notes: In Panel A we simulate how welfare changes when reducing commuting costs. In Panel B we simulate what % of the change in welfare is due to reduced variation in markdowns.

elasticity, β. For instance, in the most conservative case, the welfare gains increase by 16%. Second, in terms of redistribution, the results suggest that workers gain more from the transit shock to the detriment of firms since average labor market power decreases with the transit shock.

3.6 Conclusions

This paper studies a large subway expansion in Chile using linked employer-employee data with geographical information on workers and employers. Using an event study framework, we show four effects on the labor market of a subway expansion: 1) After a subway expands to a district, workers from that district start working further away, and earning more; 2) After a subway expands to a district, workers from that district who do not switch jobs start earning more; 3) After a subway expands to a district, firms in that district start hiring from farther way, but pay the same wages on average; and 4) Earnings converge across space: specifically, firms start paying workers closer to their sector-education-age average after the subway connects the district where the firm is in.

These facts suggest an integration of the labor market which should yield efficiency gains, but also reduced labor market power from reduced differentiation between employers. We develop a commuting model with oligopsonistic firms where the labor market power of each firm depends on the composition of its workforce, where firms who dominate specific labor markets can apply a higher markdown if that labor market represents a large share of its employees. The model also predicts that reductions in commuting costs should reduce this measure of labor market power, and additionally reduce the dispersion of markdowns, yielding indirect efficiency gains through better labor allocation. We provide evidence that the model's measure of labor supply elasticity to the firm is correlated with markdowns, and that after the subway expansions the average markdown and dispersion of markdowns decreased. Finally, we simulate the model to show that incorporating labor market power suggests gains in the order of 20-50% larger than models without it.

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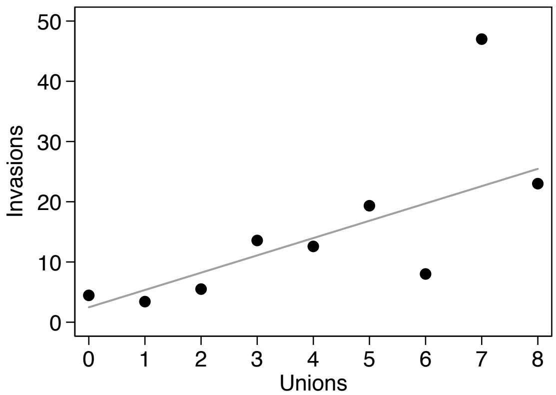
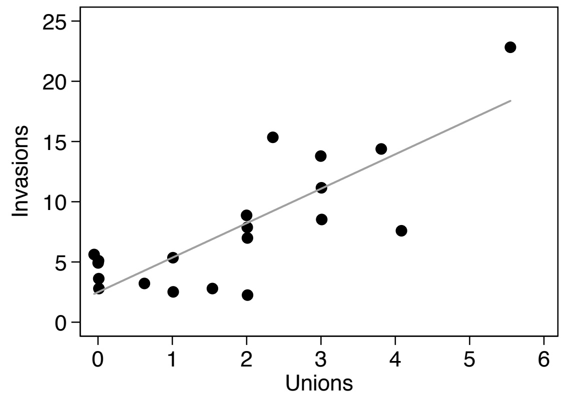
Appendix A

Appendix: Collective Action and Policy Implementation: Evidence from Salvador Allende's Expropriations

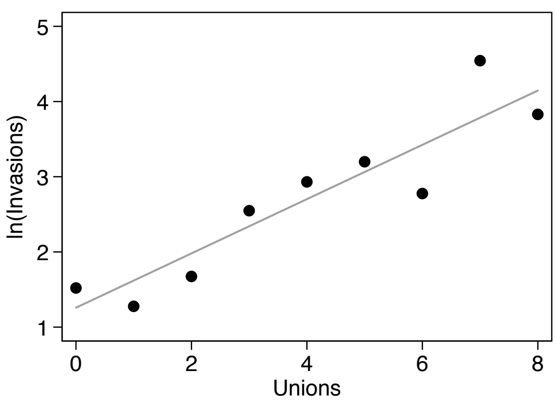
Figure A.1: More results on unions and invasions



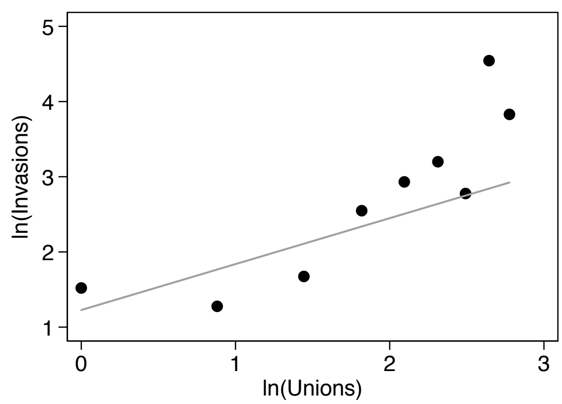
(a) Number of invasions



(b) Population control



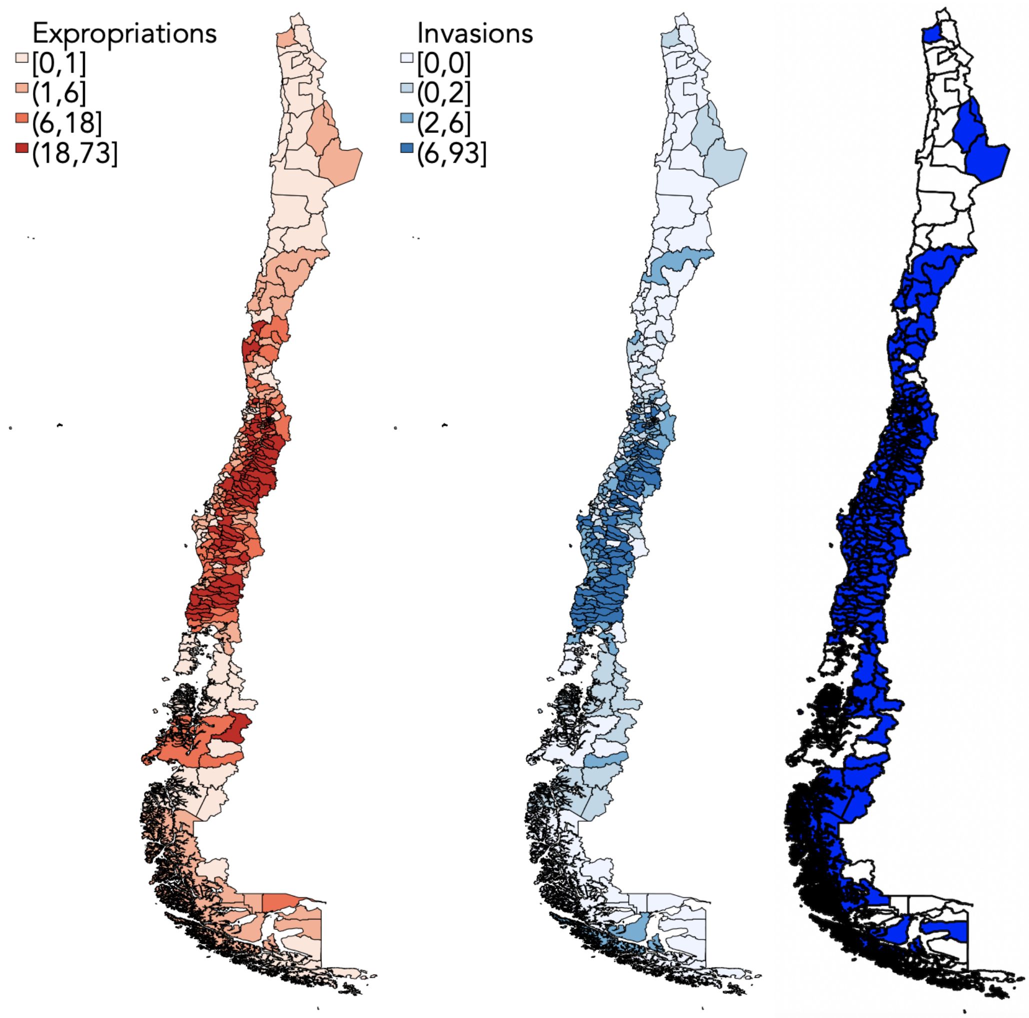
(c) Semi-elasticity



(d) Elasticity

Notes: Binscatter plots representing the cross-sectional relationship between the total number of plots invaded between 1970-1973 (y-axis) and the total number of unions using different functional forms. Straight lines denote linear fits.

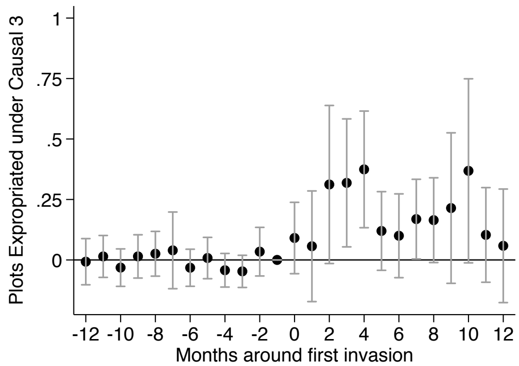
Figure A.2: Maps



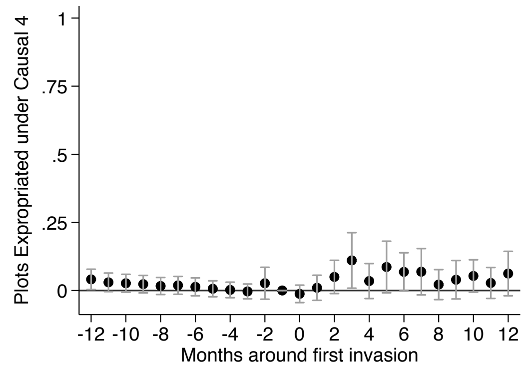
(a) Expropriations (b) Invasions (c) Sample

Notes: Maps of Chile showing the number of expropriations per county during Salvador Allende's government (panel A), the number of invasions per county in the same (panel B), and the counties in our estimation sample.

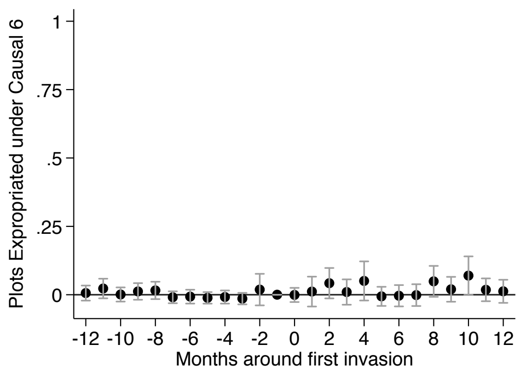
Figure A.3: Legal reasons and plots' outcomes



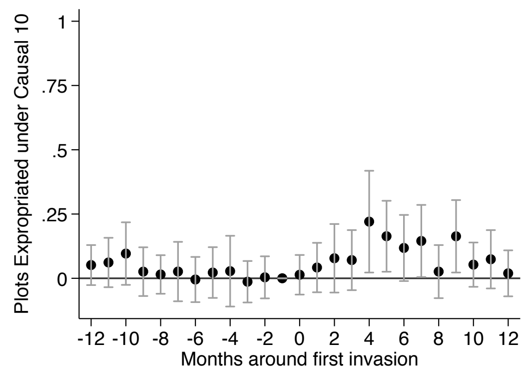
(a) Large plot



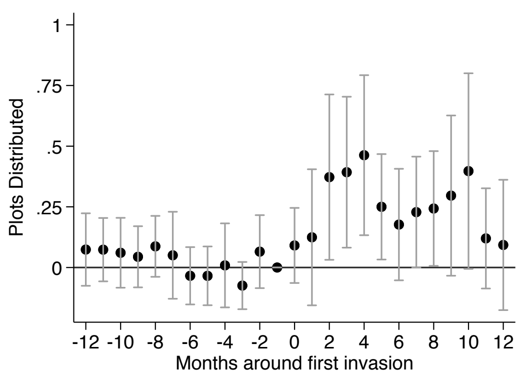
(b) Plot was inefficient



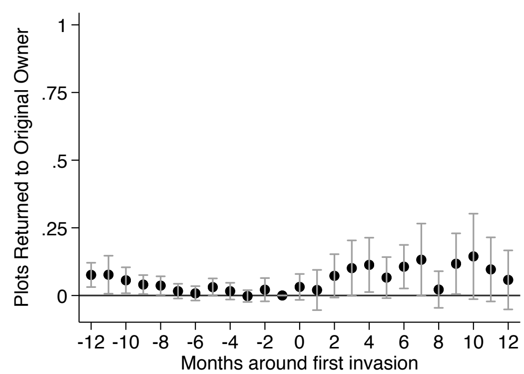
(c) Plot owner is legal person



(d) Plot owner offered plot



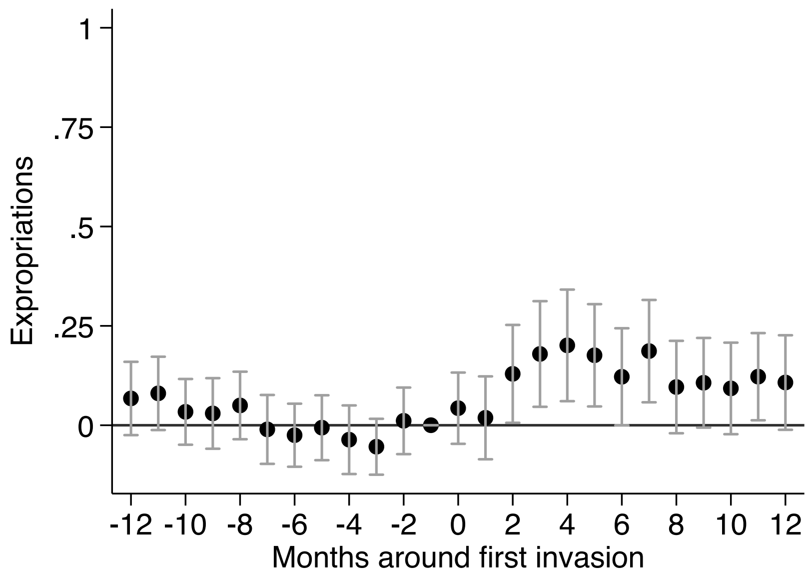
(e) Plot was redistributed



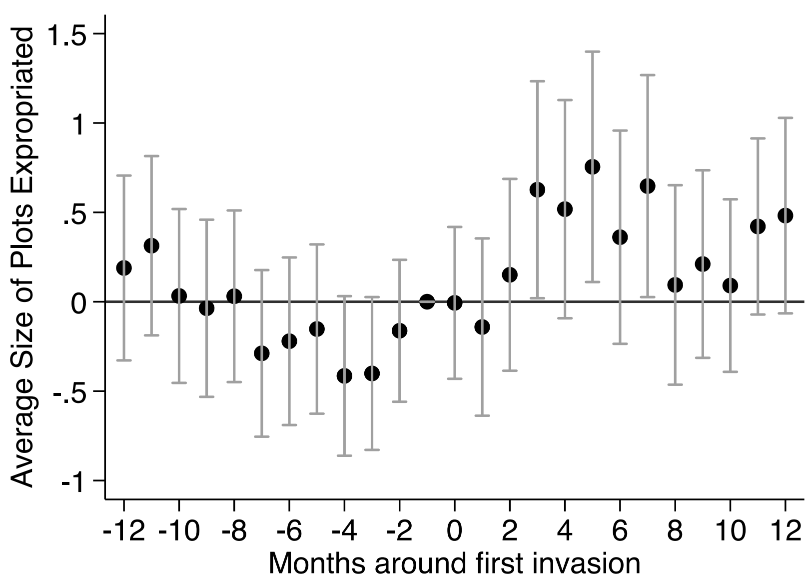
(f) Plot was returned

Notes: These figures present estimates of equation (1.1) with their corresponding 95 percent confidence interval. Each panel uses a different dependent variable. Each dependent variable in panels (a)-(d) corresponds to the number of expropriations using a different legal cause. Panels (e) and (f) use the number of plots redistributed or returned to the original owner – two possible and mutually exclusive outcomes after expropriating a plot – as dependent variable.

Figure A.4: Additional semi-parametric results



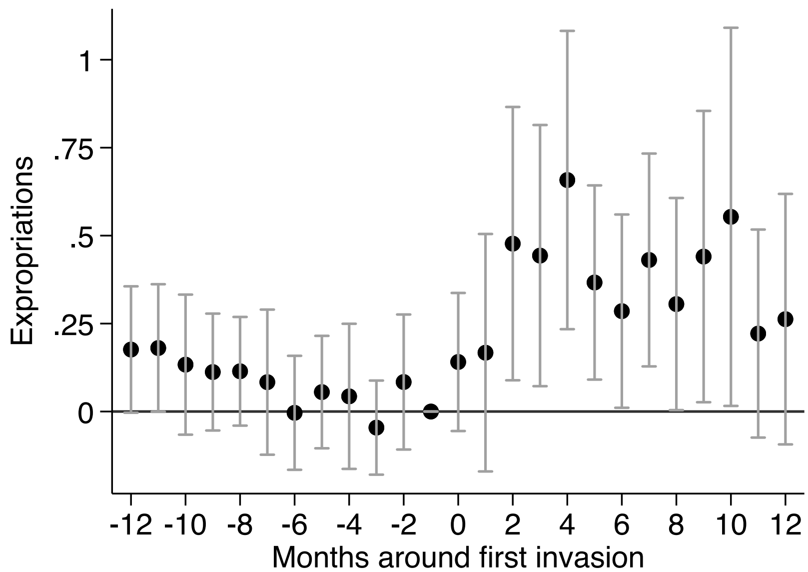
(a) Log plots expropriated



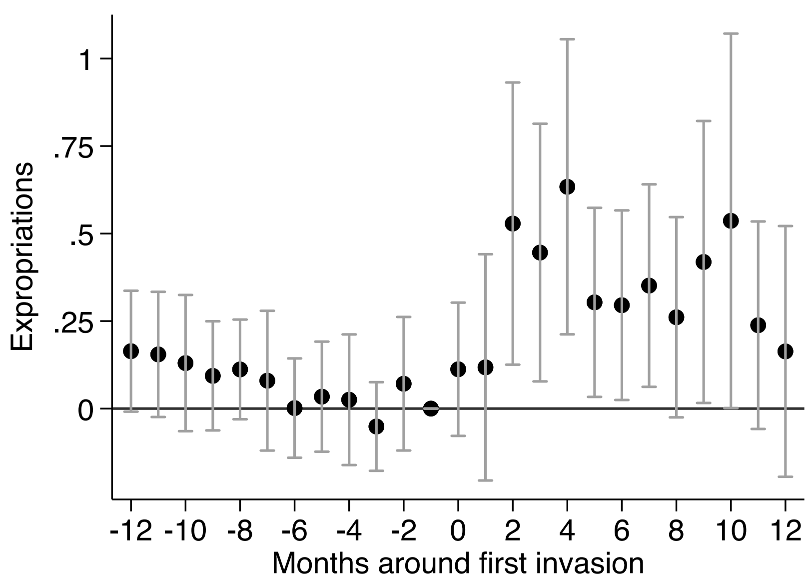
(b) Average size of expropriated plots

Notes: These figures present estimates of equation (1.1) with their corresponding 95 percent confidence interval. Each panel uses a different dependent variable. Panel A uses the hyperbolic sine transformation proposed by Burbidge et al. (1988) as dependent variable, and Panel B uses the average size of expropriated plots.

Figure A.5: Robustness, controlling for availability of large plots



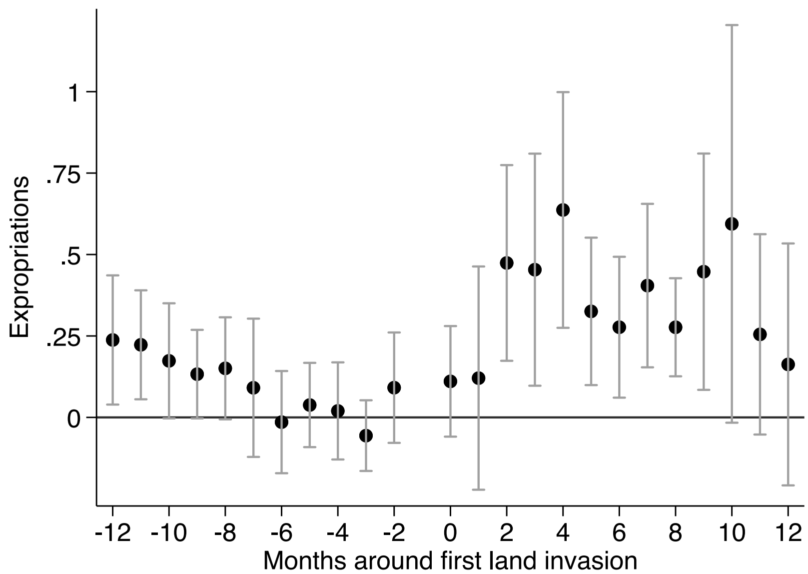
(a) Share of plots larger than 50 hectares



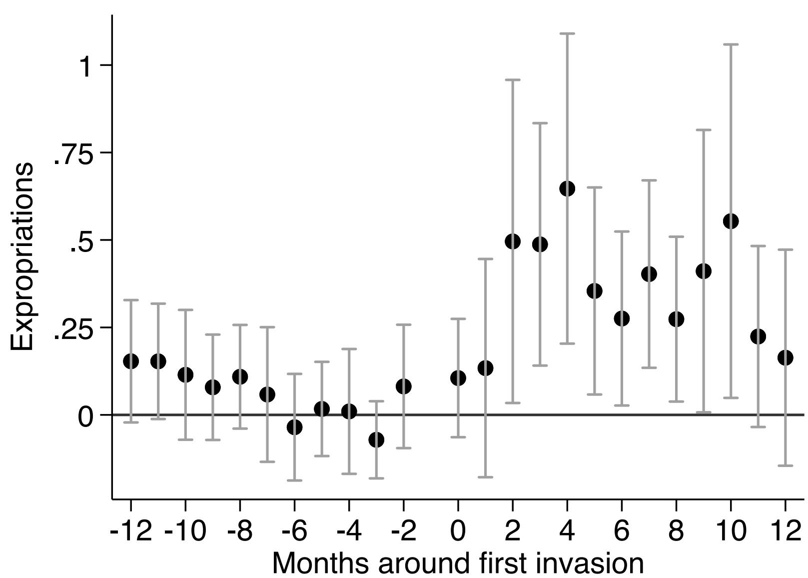
(b) Quintiles of average plot size

Notes: These figures present estimates of equation (1.1) with their corresponding 95 percent confidence interval. Panel (a) presents estimates of our main specification augmented with interaction terms between time fixed effects and the share of plots smaller than 50 hectares. Panel (b) presents estimates of our main specification augmented with interaction terms between time fixed effects and indicators for quintiles of the distribution of average plot size across counties.

Figure A.6: Alternative clustering methods



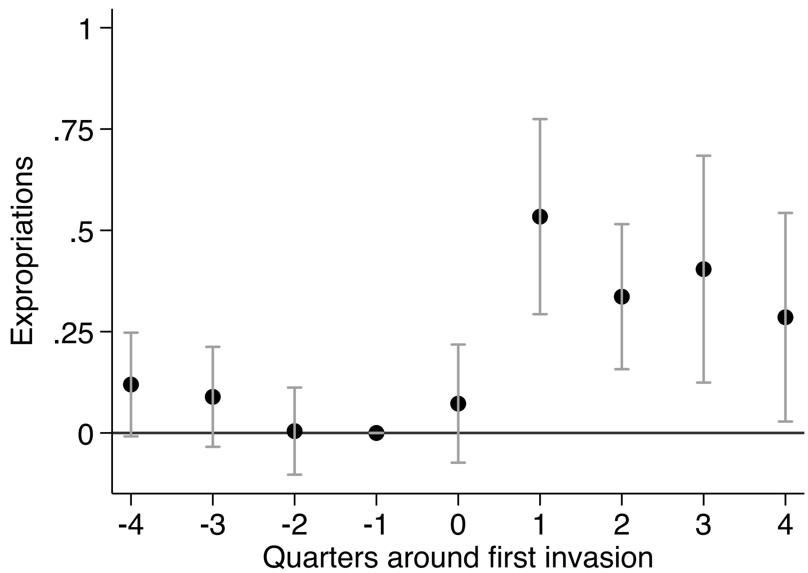
(a) Two-way clustering



(b) Spatial correlation

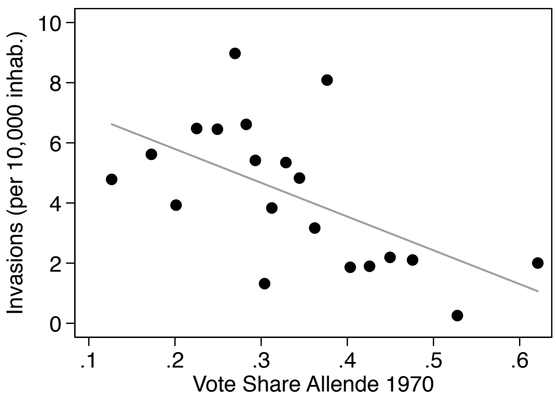
Notes: These figures present estimates of equation (1.1) with their corresponding 95 percent confidence interval using alternative clustering methods for standard errors. Panel A follows Brown and Warner (1985) and uses two-way clustering to allow correlation of outcomes within event dates. Panel B follows Conley (1999) and allows for spatial correlation of outcomes across counties during each time period. The latter uses a heteroskedasticty and autocorrelation consistent covariance estimation with distances from the centroids of counties and a Bartlett kernel which cut offs at 100kms using distances from centroid to centroid.

Figure A.7: Alternative periods of time

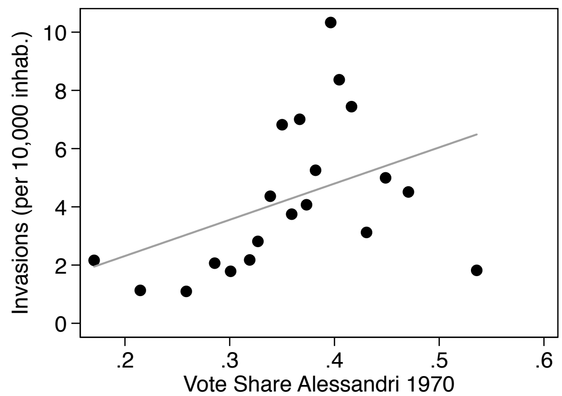


Notes: This figure presents estimates of equation (1.1) with their corresponding 95 percent confidence interval but using an alternative frequency of periods, namely quarters instead of months.

Figure A.8: Land invasions and votes in the 1970 election



(a) Invasions and vote share



(b) Invasions and vote share

Notes: Binscatter plots representing the cross-sectional relationship between the total number of plots invaded per 10,000 inhabitants (y-axis) and the vote shares for Salvador Allende (Panel A) and Jorge Alessandri (Panel B) in the 1970 presidential election.

Table A.1: Unions and land invasions

Dependent variable: log of total number of plots invaded

	Unit of observation:					
	Counties (1970-1973)			Provinces (1967-1970)		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of unions	0.36***	0.28***	0.21***	0.07***	0.04	0.17
	(0.05)	(0.05)	(0.05)	(0.02)	(0.04)	(0.10)
Observations	221	221	221	25	25	25
R-squared	0.17	0.34	0.56	0.25	0.55	0.91
Controls		X	X		X	X
Province fixed effects			X			
Region fixed effects						X

Notes: Cross-sectional estimates of the total number of plots invaded (in logarithm) on the total number of unions. Data on the number of unions by county comes from Gómez and Klein (1972). The set of "Controls" include: land inequality in 1965, agricultural surface (in hectares), agricultural production in 1965, the total number of agricultural workers, the 1970 population, the intensity of land reform until 1969. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Robustness of results to different functional forms

	Share of plots expropriated	Total number of hectares expropriated	Logarithm of hectares expropriated	Total number of hectares distributed
	(1)	(2)	(3)	(4)
Indicator for 12-month period after first invasion	0.02**	261	0.32**	30.2
	(0.01)	(271)	(0.13)	(100.6)
Counties	221	221	176	221
Observations	11,050	11,050	1,625	11,050
County fixed effects	X	X	X	X
Month fixed effects	X	X	X	X

Notes: Each coefficient comes from an estimation of equation (1.2) using a different dependent variable. Each observation corresponds to a county-month pair in the period between 01/1970 and 04/1972. Standard errors are clustered by county. Statistical significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A.3: Correlation between invasions and local support for the Allende coalition in 1971

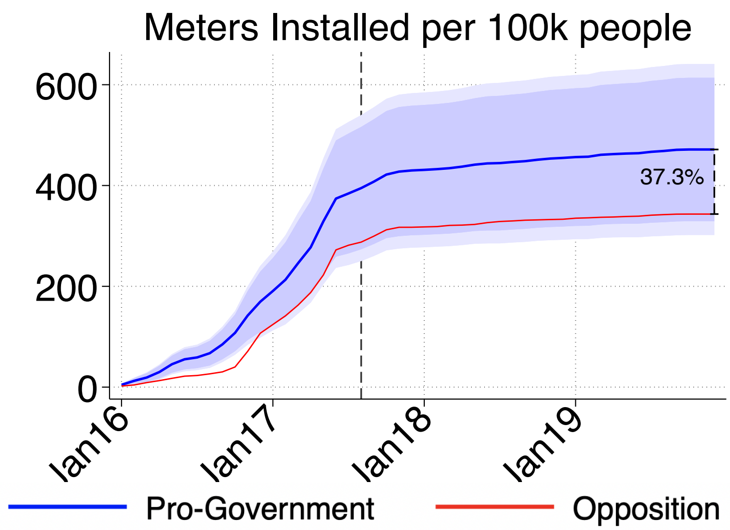
	Dep. variable: Vote share Popular Unity (UP) in the 1971 local elections		
	(1)	(2)	(3)
Land invasions before the 1971 local election	-0.005**	0.0003	0.0004
	(0.002)	(0.001)	(0.001)
Vote share Allende (UP) in 1970		0.86***	0.85***
		(0.13)	(0.13)
Vote share Tomic (PDC) in 1970		-0.01	-0.01
		(0.19)	(0.070)
Expropriations before the 1971 local election			-0.001
			(0.18)
Counties	219	213	213
R-squared	0.04	0.69	0.69

Notes: Cross sectional regressions at the county level where the dependent variable is the vote share obtained by the Popular Unity in the 1971 local government elections. Each column includes a different set of independent variables. Robust standard errors in parentheses. Statistical significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Appendix B

Appendix: Decomposing Political Favoritism in Kenyan Mass Electrification

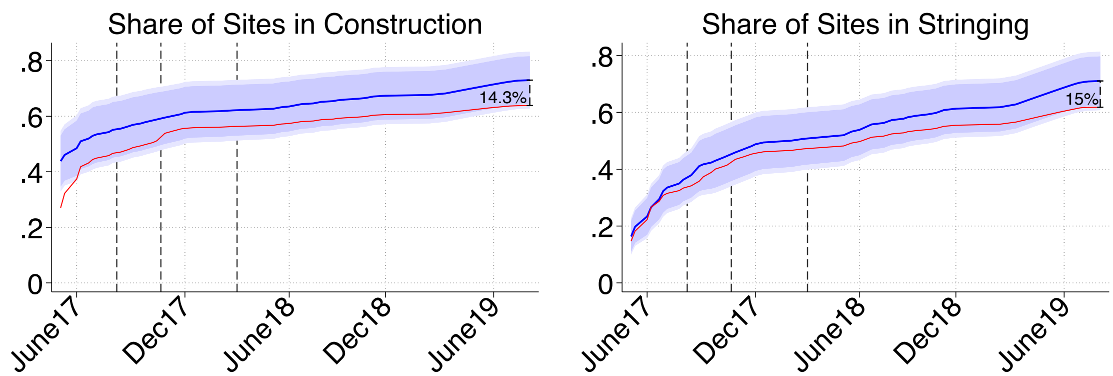
Figure B.1: Number of customers connected per 100,000 residents (Adjacent Wards)



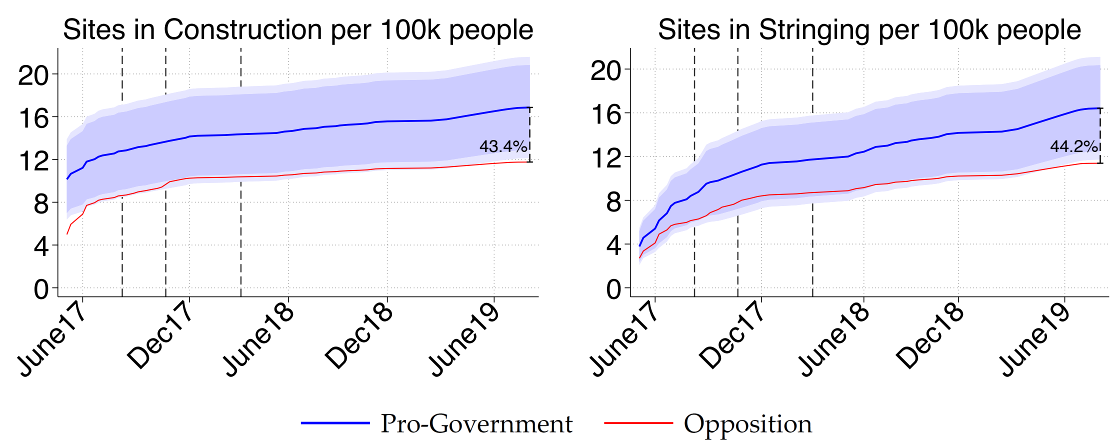
Note: This figure plots coefficients from equation 2.5 for the adjacent wards sample. The red line plots the γ_k's (meters per 100,000 people in opposition wards). The blue line plots $\gamma_k + \beta_k$ (meters per 100,000 people in pro-government wards). The blue shaded area is the confidence interval of the β_k's, the difference between pro-government and opposition wards. The dashed vertical line represents the August 2017 Presidential election. The adjacent wards sample has 150 pro-government wards (with 833 transformers) and 134 opposition wards (with 647 transformers).

Figure B.2: Construction Progress in Pro-Government and Opposition Areas (Adjacent Wards)

A. Share of LMCP Sites



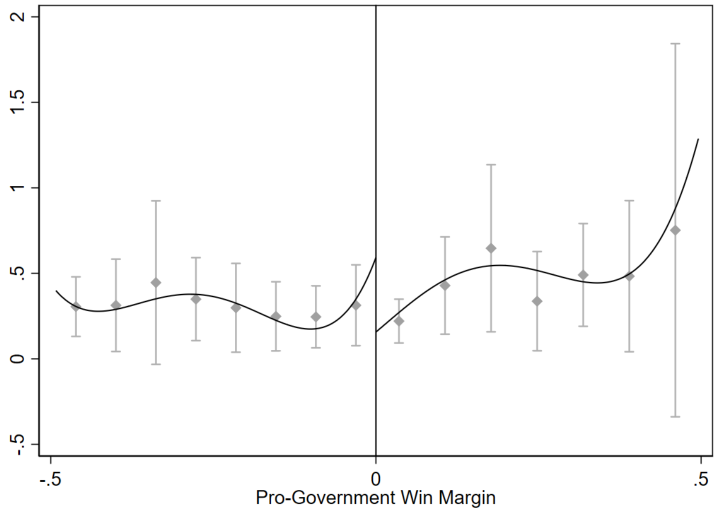
B. Number of Sites Per Capita



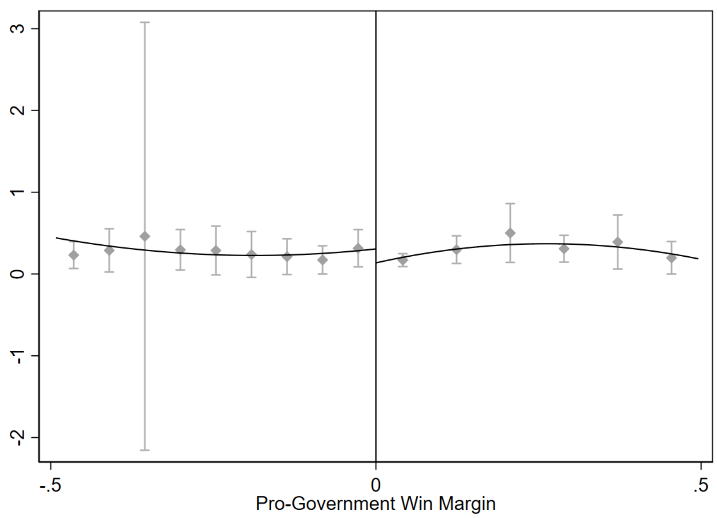
Note: This figure plots coefficients from equation 2.5 for the adjacent ward sample, using as an outcome variable the share of sites (top row) and the number of sites per capita (bottom row) in each construction stage. The red line plots the γ_k's, which are the share of sites (top row) or sites per 100,000 people (bottom row) that are at least in construction or in stringing each week in opposition wards. The blue line plots the γ_k's + β_k's, which are the share of sites (top row) or sites per 100,000 people (bottom row) that are at least in construction or in stringing each week in pro-government wards. The blue shaded area is the confidence interval of the β_k's, the difference between pro-government and opposition wards. The dashed vertical lines represent (from left to right) the August 2017 Presidential election, the October 2017 repeat election, and the March 2018 "Handshake". The adjacent wards sample has 150 pro-government wards (with 833 transformers) and 134 opposition wards (with 647 transformers).

Figure B.3: LMCP sites with construction progress

Panel a: Share of LMCP Transformers in a Constituency With Construction Started

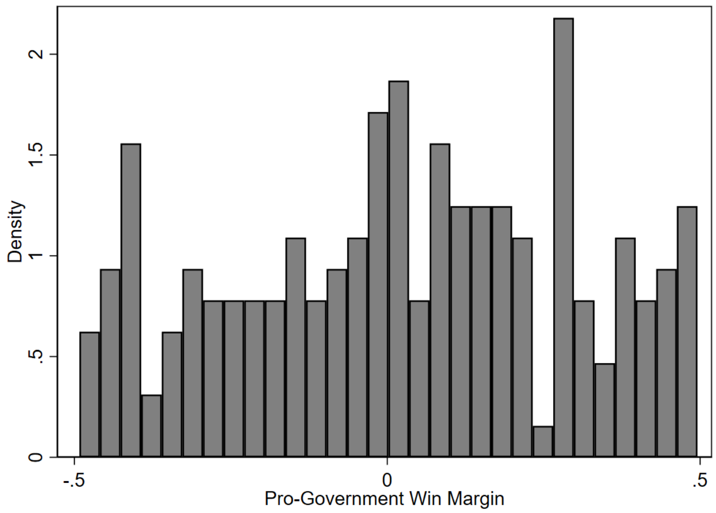


Panel b: Share of LMCP Transformers in a Constituency With Stringing



Note: The running variable—pro-government win margin—represents the difference between the vote share of the best performing candidate in a race for Member of Parliament who was in the Jubilee coalition in the 2013 general elections and the winner (if that candidate lost) or the best-performing candidate not in the Jubilee coalition (if that candidate won). Each observation is a constituency. We control for a quadratic trend. Lines represent 95 confidence intervals. We consider construction progress by March 2019.

Figure B.4: 2013 Members of Parliament Win Margins



Note: The running variable—pro-government win margin—represents the difference between the vote share of the best performing candidate in a race for Member of Parliament who was in the Jubilee coalition in the 2013 general elections and the winner (if that candidate lost) or the best-performing candidate not in the Jubilee coalition (if that candidate won). Each observation is a constituency.

Table B.1: Balance Table between Adjacent Wards

	(1) Opposition		(2) Pro-government		T-test Difference
Variable	N	Mean/SE	N	Mean/SE	(1)-(2)
Electricity	198	18.715 (1.928)	208	21.967 (2.062)	-3.252
Primary Education	198	49.903 (0.874)	208	51.941 (0.885)	-2.039
Secondary Education	198	22.675 (1.034)	208	23.609 (1.072)	-0.934
Population	198	24637.904 (718.896)	208	23862.005 (619.867)	775.899
Area	198	253.563 (19.160)	208	206.135 (16.045)	47.428*
Household size	198	4.145 (0.048)	208	4.027 (0.052)	0.119*
Gradient	198	3.470 (0.129)	208	3.906 (0.143)	-0.436**
Work for Pay	198	22.688 (1.007)	208	23.267 (0.973)	-0.579
Dependency Ratio	198	0.924 (0.017)	208	0.878 (0.017)	0.046*
Iron roof	198	63.989 (1.765)	208	67.950 (1.669)	-3.960
Urban transformers	198	21.530 (5.094)	208	12.966 (2.181)	8.564
Urban ward	198	0.202 (0.029)	208	0.168 (0.026)	0.034
Granular pop count	198	1.13e+05 (23815.926)	208	73253.462 (10837.160)	39859.403
Granular pop density	198	1939.151 (241.757)	208	1759.511 (215.466)	179.640
F-test of joint significance (F-stat)					4.195***
F-test, number of observations					406

Wards are selected based on the 2013 election according to the adjacent wards procedure described in Section 2.3 and shown in Figure 2.4. This table shows the mean of each variable for each group of wards, and the difference of the means in column 3. Variables at the ward level include share of adults with primary education, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, and being an urban ward. A ward is urban if one of the 42 main towns of Kenya is located in it, or if the ward is in Nairobi or Mombasa County. Variables originally at the transformer level which where averaged at the ward include gradient and granular population density. Variables originally at the transformer level which were summed over at the ward level include granular population count, and the number of urban transformers. Urban transformers are those within 5kms of a town center and those in Nairobi or Mombasa County. Missing values are imputed with the group mean. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.2: Number of sites in progress per 100,000 people in each ward (balanced panel), by 2013 Ward election result

	Construction Started			Stringing in Progress		
	(1)	(2)	(3)	(4)	(5)	(6)
Ward voted pro-govt in 2013	5.51***	5.51***	4.59***	4.73***	4.73***	2.60***
	(1.27)	(1.27)	(1.16)	(1.10)	(1.10)	(0.80)
Sample week [0-1]		5.39***	4.53***		8.40***	6.41***
		(0.27)	(0.29)		(0.43)	(0.35)
Interaction (pro-govt X week)			1.83***			4.26***
			(0.56)			(0.89)
Observations	114708	114708	114708	114708	114708	114708
Control Mean	10.48	10.48	10.48	8.95	8.95	8.95
Week Control	FE	Cont.	Cont.	FE	Cont.	Cont.

Outcome variable: number of sites progressing, per 100,000 people. SE in parentheses. Standard errors clustered at the Ward level. Observations are weighted by ward population. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.3: Number of sites in progress in each ward (balanced panel), by 2013 Ward election result

	Construction Started			Stringing in Progress		
	(1)	(2)	(3)	(4)	(5)	(6)
Ward voted pro-govt in 2013	1.12***	1.12***	0.94***	0.96***	0.96***	0.53***
	(0.32)	(0.32)	(0.29)	(0.27)	(0.27)	(0.20)
Sample week [0-1]		1.31***	1.15***		2.03***	1.63***
		(0.07)	(0.08)		(0.10)	(0.10)
Interaction (pro-govt X week)			0.35**			0.85***
			(0.13)			(0.21)
Observations	114708	114708	114708	114708	114708	114708
Control Mean	2.44	2.44	2.44	2.09	2.09	2.09
Week Control	FE	Cont.	Cont.	FE	Cont.	Cont.

Outcome variable: number of sites progressing, per 100,000 people. SE in parentheses. Standard errors clustered at the Ward level. Observations are weighted by ward population. Statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.4: Individual transformer progress, by 2013 Ward election result

	Construction Started			Stringing in Progress			Complete		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ward voted pro-govt in 2013	0.06***	0.06***	0.06***	0.04***	0.04***	0.00	-0.08***	-0.08***	-0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Sample week [0-1]		0.24***	0.23***		0.38***	0.34***		0.27***	0.26***
		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)
Interaction (pro-govt X week)			0.00			0.08***			0.02
			(0.01)			(0.01)			(0.01)
Observations	314916	314916	314916	314916	314916	314916	314916	314916	314916
Control Mean	0.55	0.55	0.55	0.47	0.47	0.47	0.36	0.36	0.36
Week Control	FE	Cont.	Cont.	FE	Cont.	Cont.	FE	Cont.	Cont.

Outcome variable: whether a transformer reached a stage. SE in parentheses. Standard errors clustered at the Ward level. Observations are weighted by ward population. Statistical significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table B.5: Meters Installed before LMCP per LMCP transformer (placebo), by 2013 Ward election result

	(1)	(2)	(3)	(4)	(5)	(6)
Ward voted pro-govt in 2013	0.68*	0.68*	0.04	0.62	0.62	-0.09
	(0.37)	(0.37)	(0.07)	(0.42)	(0.42)	(0.29)
Sample month [0-1]		5.29***	4.57***		5.39***	4.58***
		(0.41)	(0.36)		(0.43)	(0.37)
Interaction (pro-govt X month)			1.27*			1.43*
			(0.76)			(0.79)
Observations	20052	20052	20052	18417	18417	18417
Control Mean	2.27	2.27	2.27	2.27	2.27	2.27
Month Control	FE	Cont.	Cont.	FE	Cont.	Cont.
Controls	No	No	No	Yes	Yes	Yes

Outcome variable: accumulated meters installed per LMCP transformer, starting in January 2014 until June 2015, including only those labeled other than LMCP/GPOBA. Standard errors are clustered by ward and are shown in parentheses. Observations are weighted by the number of transformers. Month Control FE includes month fixed effects. 'Sample month'=0 in the first month of the sample and 1 in the last month of the sample, increasing in linear increments over the interval. Number of transformers is the number of transformers corresponding to the meters in that ward. Controls include share of adults with primary education, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, being an urban ward, and ward population. Statistical significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table B.6: Percent of a Ward's LMCP transformers in progress, by 2013 MP election result

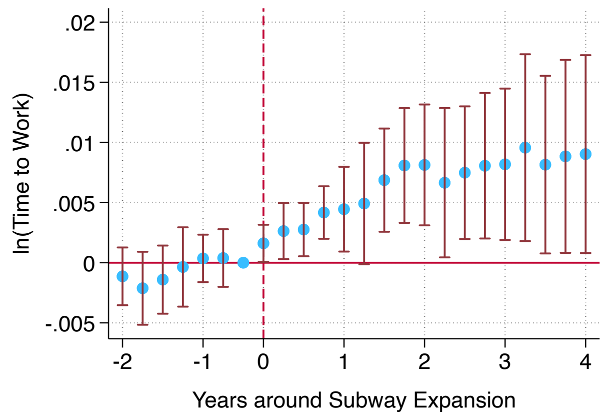
	Construction Started			Stringing in Progress		
	(1)	(2)	(3)	(4)	(5)	(6)
Aligned with MP in '13	1.14	1.31	1.28	3.91	3.76	3.74
	(3.06)	(3.33)	(3.34)	(2.40)	(2.57)	(2.57)
Sample week [0-1]		36.64***	36.09***		59.64***	59.12***
		(1.81)	(3.76)		(1.96)	(3.88)
Interaction (pro-govt X week)			0.94			0.90
			(5.03)			(5.47)
Observations	5610	5610	5610	5610	5610	5610
Control Mean	53.21	53.21	53.21	41.72	41.72	41.72
Week Control	FE	Cont.	Cont.	FE	Cont.	Cont.
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The outcome variable is the percentage of LMCP sites in each ward that have progressed to at least the indicated stage of construction (either construction started or stringing in progress). Standard errors are clustered by ward and are shown in parentheses. Observations are weighted by ward population. 'Aligned with MP in '13'=1 if the Ward voted for the winning MP in 2013. When Week Control="FE" the specification includes week fixed effects. The variable 'Sample week' equals 0 in the first week of the sample and 1 in the last week of the sample, increasing in linear increments over the interval. Controls include share of adults with primary education, share of adults with secondary education, share of households with electricity, share of adults who work for pay, total dependency ratio, share of households with a corrugated iron roof, ward area, average household size, and being an urban ward. Statistical significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

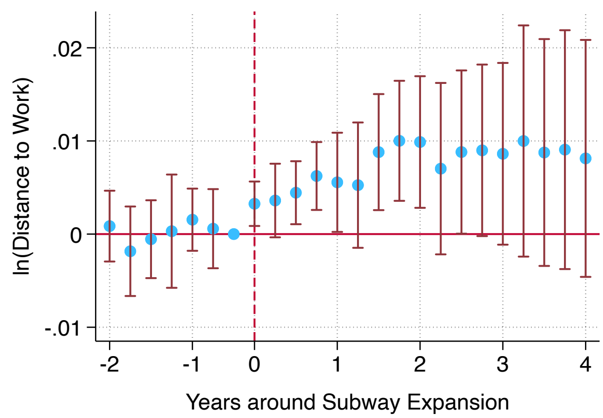
Appendix C

Appendix: Urban Transit Infrastructure: Spatial Mismatch and Labor Market Power

Figure C.1: Trimesters: Where to work



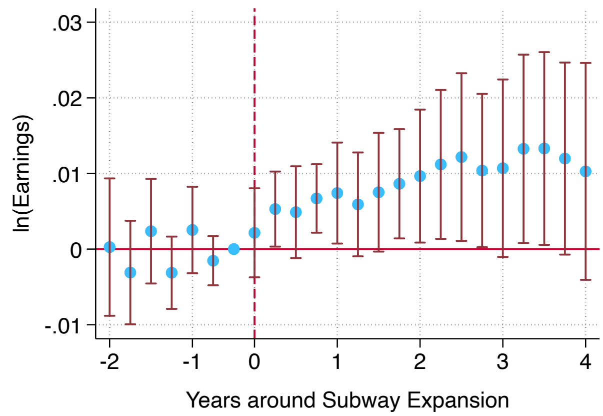
(a) Time to Work



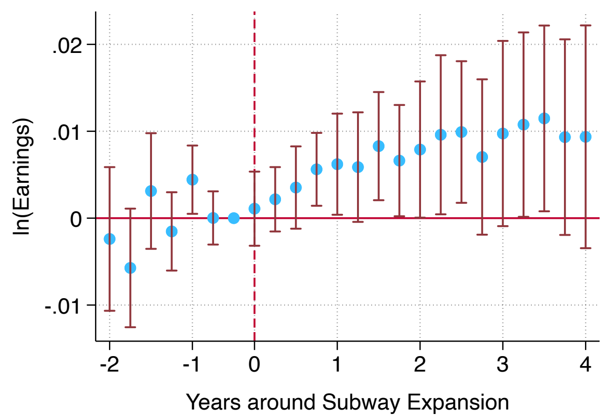
(b) Distance to Work

Notes: Event Study results on distance and time to work. Time to work is estimated before any subway expansion, and therefore is just another measure of distance, does not necessarily imply longer commutes. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.2: Trimesters: Earnings



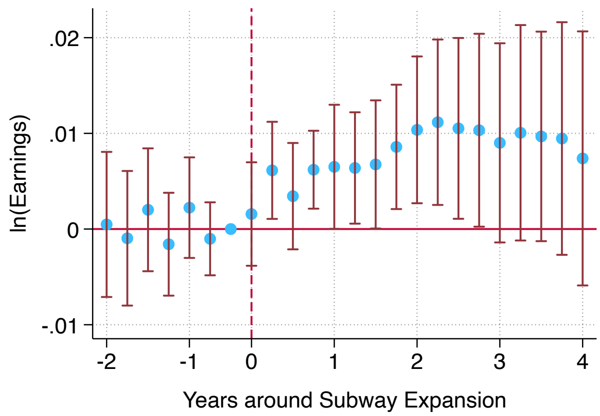
(a) Earnings



(b) Earnings - Stayers

Notes: Event Study results on earnings. Panel A using worker fixed effects, Panel B using worker-firm fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

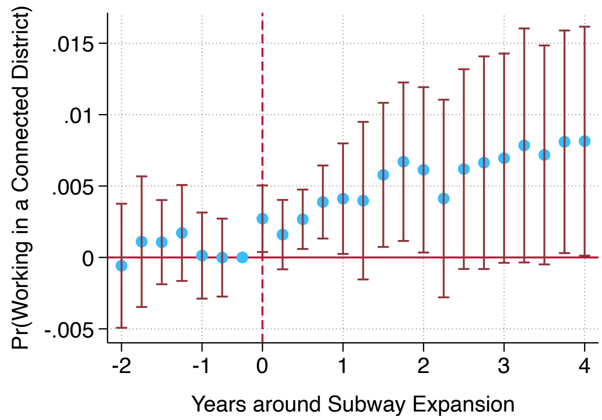
Figure C.3: Trimesters: Ruling out Labor Supply



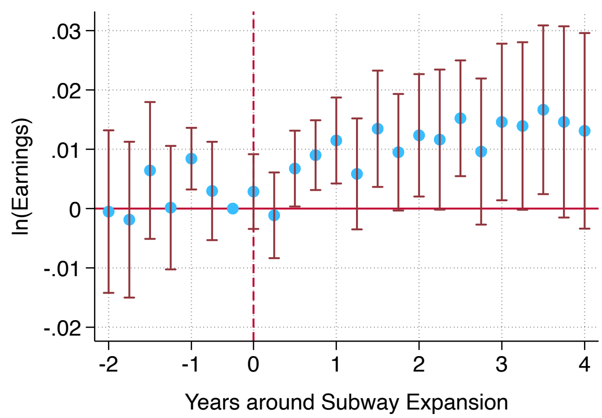
(a) Earnings - Stayers - District of Firm x Sector Fixed Effects

Notes: This event study includes Worker x Firm fixed effects, and Month X District of Firm x Sector fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.4: Trimesters: Worker Flows and Earnings of Unconnected



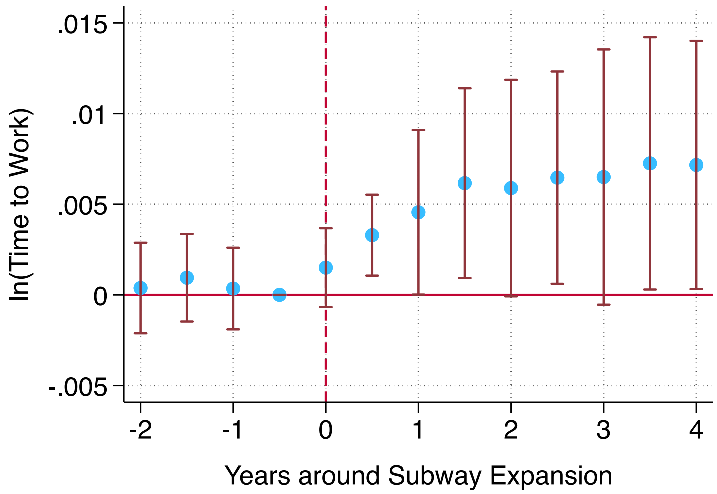
(a) Worker Flows



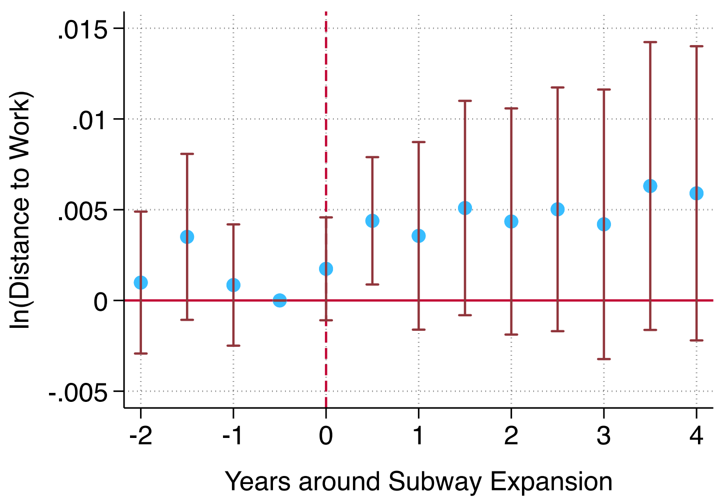
(b) Earnings - Stayers - Unconnected

Notes: For each treated district, we simulate commuting times before and after the treatment to all other districts. Then divide destination districts into above and below the median for each treated district. Above the median districts are referred to as connected districts, below the median as unconnected. Panel A shows that workers are more likely to work in connected districts, and Panel B shows that results on earnings using worker-firm fixed effects hold even for workers who are working in districts that did not get connected. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.5: Stacked Dif-in-Dif: Where to Work



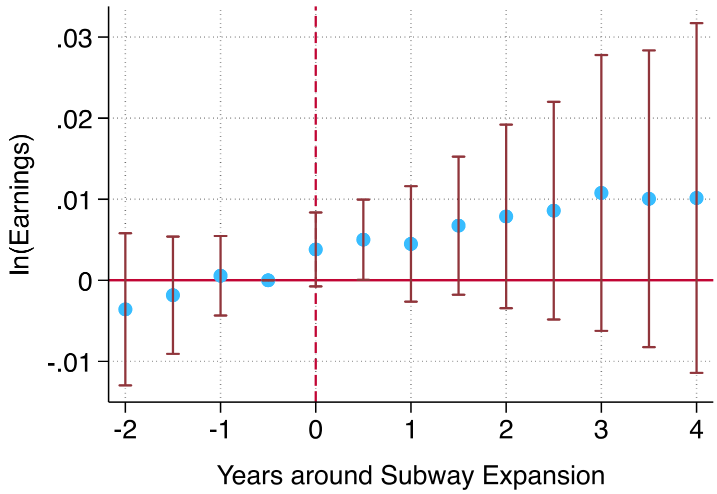
(a) Time to Work



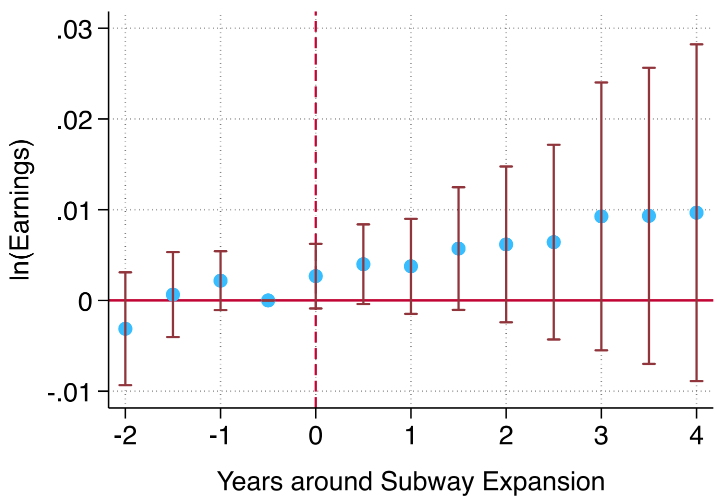
(b) Distance to Work

Notes: Stacked Dif-in-Dif using districts within wave with under 30% treatment intensity as controls for the treated districts in the wave. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.6: Stacked Dif-in-Dif: Earnings



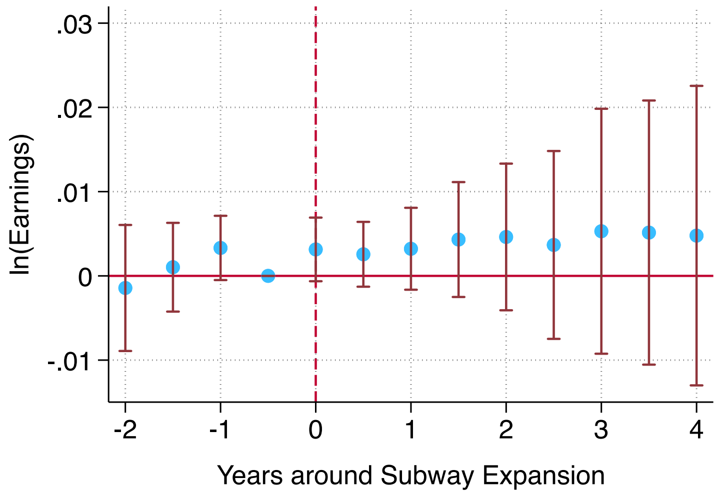
(a) Earnings



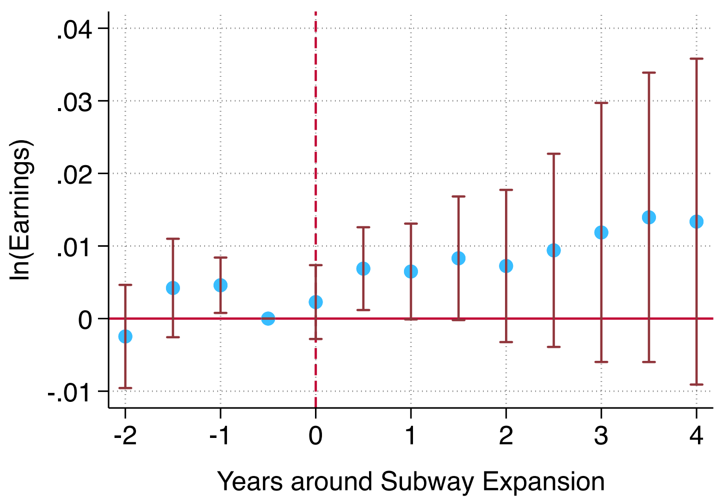
(b) Earnings - Stayers

Notes: Stacked Dif-in-Dif using districts within wave with under 30% treatment intensity as controls for the treated districts in the wave. Panel A uses worker fixed effects, while Panel B includes worker-firm fixed effects. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.7: Stacked Dif-in-Dif: Earnings (Robustness)



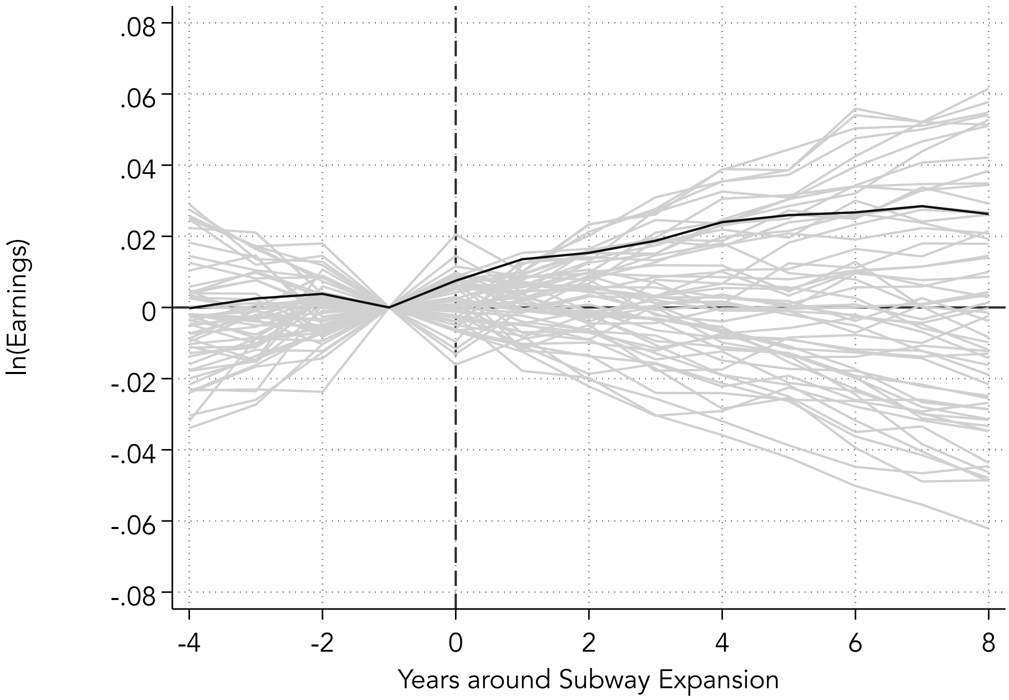
(a) Ruling out labor supply



(b) Ruling out hours/productivity

Notes: Stacked Dif-in-Dif using districts within wave with under 30% treatment intensity as controls for the treated districts in the wave. Both Panel include worker-firm fixed effects. Panel A also includes firm's district-month fixed effects instead of only month fixed effects. Panel B only includes workers who work in districts that were not connected by the new subway line. Coefficients are scaled by 0.42 to represent the effect on the average worker.

Figure C.8: The Effect of Subway Expansion on Earnings: Permutation Test



Notes: We take the 38 treatment timing-intensity pairs and randomize them across the 38 districts, and estimate the event study on earnings of workers. We repeat this 60 times, and plot the results. The break in the trend is in the top 5%.

Table C.1: Relationship between distance to subway and commute duration

	(1) ln(Trip Duration)	(2) ln(Trip Duration)	(3) ln(Trip Duration)	(4) ln(Trip Duration)
ln(Trip Distance)	0.20***	0.096**	0.19***	0.093*
	(0.024)	(0.048)	(0.023)	(0.048)
ln(Distance of O to Subway)	0.072***	0.077***		
	(0.0076)	(0.0083)		
ln(Dist of O to Subw + Dist of D to Subw)			0.12***	0.098***
			(0.011)	(0.010)
N	18417	14148	18417	14148
R2	0.55	0.66	0.55	0.66
OD District FE	Yes	No	Yes	No
OD Zone FE	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Std Errors	Cl at OD-District	Cl at OD-Zone	Cl at OD-District	Cl at OD-Zone

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: These regressions use data from the 2001 and 2012 Origin-Destination surveys. OD District FE are fixed effects for each pair of origin-destination districts. OD Zone FE divides Santiago into 400 rectangular zones, and is a fixed effect for each pair of origin-destination zones. Only work trips that use public transportation at some stage are included in this sample. Dist of O to Subway is the eucledian distance from the trip-origin to the subway, and Dist of D to Subway is the eucledian distance from the trip-destination to the subway.