State Capacity, Informality, and Clientelism*

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Abstract

Understanding how to tackle clientelism is crucial for enhancing political accountability. Focusing on Brazil, this paper explores whether stricter enforcement of labor regulations can disrupt local clientelistic networks. To address endogeneity concerns, we use an instrumental variable approach, relying on within-municipality changes in proximity to labor offices and variations in the number of labor inspectors across states. We show that enhanced enforcement capacity significantly reduces the electoral success of clientelistic parties and curtails patronage and vote-buying. These effects are largely due to a decline in the proportion of informal workers, who exhibit a higher propensity to vote for clientelistic parties.

Keywords: Enforcement capacity, state capacity, informality, clientelism, vote-buying, patronage

JEL codes: D72, H11, H70, O12, O17

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1 Introduction

Weak state capacity and clientelism are intertwined in a self-reinforcing cycle: ineffective state institutions allow clientelism to flourish, and in turn, clientelism provides incentives for politicians to sustain and even further weaken these institutions (Fergusson et al. 2022a). This dynamic erodes the quality of governance, leading to pervasive corruption, constrained political competition, diminished trust in institutions, and an overall inadequate provision of public goods.[1] While previous research has explored how clientelism discourages efforts to improve local state institutions (Fergusson et al. 2022b; De La O 2024), the role of state capacity investments in curbing clientelism remains underexplored, largely due to the methodological challenges posed by reverse causality and the self-selection issue inherent in such investments.

This paper examines whether and how the expansion of state capacity by central authorities can disrupt entrenched clientelistic practices at the local level. Unlike initiatives led by local politicians, which often reinforce existing patron-client networks, central authorities are better positioned to establish administrative structures that are less susceptible to capture and, therefore, more effective at reshaping local political dynamics. Specifically, we investigate whether a capable state apparatus, through effective law enforcement, can alleviate some of the economic vulnerabilities that compel citizens to rely on clientelistic relations as an informal insurance mechanism (see, e.g., Anderson et al. 2015; Bobonis et al. 2022), and thereby weaken the influence of clientelistic parties across municipalities.

To examine the effect of state capacity expansion on local clientelism, we examine temporal and spatial variations in the intensity of labor law enforcement across Brazil. The Ministry of Labor plays a pivotal role in this context, as it uniquely oversees the implementation of labor regulations, including the establishment of labor offices and the geographic deployment of inspectors, which directly influence the intensity of enforcement across the territory. Our primary independent variable, enforcement intensity, is measured as the number of inspections per hundred formal firms at the municipal level. To evaluate clientelism and its related outcomes, such as patronage and vote-buying, we draw on multiple data sources, including three expert surveys at the party level (Kitschelt et al. 2013; Norris 2020; Lindberg et al. 2022), microdata on the entire universe of Brazilian bureaucrats, and individual-level data from five waves of the Latinobarómetro survey.

[1]For comprehensive studies on clientelist politics and its implications, see Bates (1981); Kitschelt and Wilkinson (2007); Baland and Robinson (2008); Piattoni (2001); Bardhan and Mookherjee (2012); Robinson and Verdier (2013); Stokes et al. (2013); Anderson et al. (2015). On the effects of state capacity, see e.g., Acemoglu (2005); Besley and Persson (2009, 2010); Aneja and Xu (forthcoming).

To address potential endogeneity concerns, we adopt an instrumental variable approach. Specifically, we use two instruments to capture exogenous variation in local enforcement capacity: the distance to the nearest labor office and the number of labor inspectors assigned to each state. Greater distances and fewer inspectors are systematically associated with lower inspection rates, making these instruments well-suited for isolating the causal effect of enforcement intensity. Our approach builds on the methodology of Almeida and Carneiro (2012) and Ponczek and Ulyssea (2022), who use similar variables to study the impact of labor inspections on labor market outcomes. However, our analysis differs in two important ways. First, we shift the focus to a distinct dependent variable: the dynamics of local clientelism. Second, while prior studies rely solely on cross-sectional variation in the location of labor offices, we enhance this framework by incorporating temporal data, including the establishment dates of labor offices and changes in the number of inspectors across states over time. This approach enables us to assess how changes in enforcement capacity over time impact municipalities, while effectively controlling for observed and unobserved time-invariant characteristics specific to each locality.

The main threat to identification stems from the potential endogeneity of labor office locations and inspector resource allocation, which could correlate with municipal clientelism. We address this and other threats to identification in different ways. First, we include municipality fixed effects in all regressions to account for time-invariant local characteristics. Second, we consider the specific criteria set by the Ministry of Labor for allocating a labor office, which are based on factors such as population size and labor market conditions. Importantly, these criteria apply exclusively to host municipalities and not to neighboring areas. To avoid any biases stemming from the selection of host municipalities, we exclude them from our analysis, and focus solely on assessing how changes in enforcement capacity impact clientelism in the remaining localities. Additionally, our results remain robust when we include other potentially relevant time-varying controls. Finally, we also employ an event-study approach leveraging the timing of office openings. This method shows no pre-treatment differences in clientelism or labor formality levels across municipalities prior to the establishment of a nearby labor office, confirming that the opening of new offices is not preceded by changes in our key variables of interest.

Our findings show that increased enforcement capacity has a significant impact on reducing clientelistic practices. Specifically, it lowers the vote share of clientelist parties, decreases their likelihood of winning local elections, and reduces the prevalence of local reports of vote-buying. Both demand-side and supply-side factors play a critical role in driving these results. On the supply side, stronger enforcement reduces the overall number of mayoral candidates, a decrease driven entirely by a decline in candidates from clientelist parties. On

the demand side, we observe an increase in voter turnout and a decrease in support for clientelistic candidates, even after accounting for the share of candidates from clientelist parties running for office. To further validate these findings, we examine whether enhanced enforcement, by altering the profile of elected candidates, affects patronage appointments—a core element of clientelism. Our analysis reveals that improved enforcement capacity reduces the hiring of bureaucrats on temporary contracts, which are typically associated with political appointees (Kemahlioğlu 2013; Brollo et al. 2017; Colonnelli et al. 2020; Toral 2023). Finally, we observe broader governance improvements, including significant enhancements in municipal-level outcomes in education and health.

We argue that stricter enforcement of labor regulations weakens the success of clientelistic parties by reshaping the local labor market. Specifically, rigorous enforcement reduces informality and increases formal employment, thereby decreasing citizens' reliance on clientelistic relationships to manage risks. To investigate this mechanism, we use detailed data on labor market outcomes. Microdata from the Demographic Census and administrative records of the *Relação Anual de Informações Sociais* (RAIS), which covers the entire population of formal workers in the country, show that municipalities with higher enforcement capacity witness significant increases in the share of formal workers and decreases in informality. Furthermore, individual-level data from the 2014 Brazilian Electoral Panel Study indicate that informal workers are more likely to vote for candidates from clientelistic parties, even after accounting for income levels. This finding aligns with research highlighting the role of clientelistic networks in providing informal insurance to vulnerable populations (Rains and Wibbels 2023; Bobonis et al. 2022), and reinforces the association between informality and weaker programmatic demands (Gottlieb 2024).

We conduct a series of robustness checks to validate our findings. First, we explore alternative measures of clientelism, extend the analysis to city council elections, and account for clientelism at the coalition level. We also test alternative definitions of programmatic parties, demonstrating that increased enforcement particularly benefits parties that prioritize economic issues and those with centralized structures, as these parties are better equipped to enforce discipline through their control over candidate nominations and promotions (Morgenstern 2003). Importantly, we address several potential confounders and rule out alternative interpretations of our findings, including personalism, populism, and specific ideologies. Additionally, we confirm that our results are not driven by changes in state or national leadership by demonstrating robustness even when excluding co-partisans of the governor and president. Lastly, we test various first-stage specifications, explore alternative measures of distance to the nearest labor office, and assess different approaches to handling missing clientelism scores in expert surveys. These checks collectively strengthen the credibility of

our findings.

This paper primarily contributes to the literature studying the relationship between state capacity and clientelism. While previous studies have established a strong association between these variables, suggesting that causality can operate in both directions (Fergusson et al. 2022a), much of the existing research has focused on the impact of clientelism on state capacity. This includes its influence on the size of the public sector, the politicization of the bureaucracy (Brierley 2021; Calvo and Murillo 2004; Cruz and Keefer 2015; Geddes 1991; Gimpelson and Treisman 2002; Golden 2003; Grzymala-Busse 2008; Hicken 2011; Oliveros 2021), and its role in perpetuating detrimental cycles (Bustikova and Corduneanu-Huci 2017; Fergusson et al. 2022b; De La O 2024). In contrast, this paper investigates whether centrally-driven investments in administrative capacity can reduce local clientelism. Our findings demonstrate that such investments not only weaken the electoral success of clientelistic parties but also result in significant changes in local bureaucratic hiring practices and lead to improvements in the quality of municipal administration.

Our study also adds to existing research on the consequences of enforcement for labor market structures by offering new evidence using a novel empirical strategy and expanded data from the Brazilian context. Previous research has demonstrated the significant impact of enforcement on informality.[2] For example, de Andrade et al. (2016) show through a field experiment in Belo Horizonte that receiving a visit from an inspector increases formalization rates among firms by 21 to 27 percentage points. Similarly, Brotherhood et al. (2023) find that when inspectors catch firms employing informal workers, these firms significantly increase their number of formal employees. Almeida and Carneiro (2012), using cross-municipal variation in enforcement levels in 2000, report that increased labor inspections lead to higher formal employment, lower self-employment, and a rise in non-employment. Building on this body of work, we revisit the relationship between enforcement and labor market outcomes by adopting a more comprehensive approach. Specifically, we leverage the timing of labor office openings to exploit temporal variation in enforcement, incorporate an additional wave of census data, and utilize detailed annual records of formal employees. To address potential endogeneity concerns, we include municipality fixed effects to control for unobserved, time-invariant local characteristics, employ an instrumental variable strategy, and implement an event study approach. Our findings expand the literature by demonstrating that strengthened enforcement reduces informality, self-employment, and non-employment across municipalities. These results align with recent studies showing that stricter labor regulation enforcement does not necessarily lead to higher unemployment rates (Meghir et al. 2015; Ulyssea 2020; Haanwinckel and Soares 2021; Dix-Carneiro et al. 2021).

[2]See Ulyssea (2020) for a review of this literature.

Finally, our study contributes to the literature on the individual-level determinants of demand for clientelism. While extensive research has shown that poverty increases susceptibility to clientelistic offers (e.g., Calvo and Murillo 2004; Kitschelt and Wilkinson 2007; Robinson and Verdier 2013), and that poverty-reduction programs can diminish the influence of clientelist patrons (Frey 2019), the role of interventions targeting labor market vulnerability remains less explored. By examining this question, we add to the growing body of work connecting vulnerability to clientelism (Nichter 2018; Murillo et al. 2021; Bobonis et al. 2022; Rains and Wibbels 2023; Gottlieb 2024). Our findings demonstrate that formalizing labor markets can reduce citizens' reliance on clientelistic relationships, offering a novel perspective on the association between labor law enforcement and political behavior.

The remainder of article is organized as follows. Section 2 reviews the relevant literature and outlines our argument regarding the relationship between labor regulation enforcement and clientelism. Section 3 derives the testable empirical hypotheses. Section 4 provides background information on Brazil. Section 5 details the data and empirical strategy. Section 6 presents the main findings and explores the underlying mechanisms. Section 8 addresses potential threats to identification and presents robustness tests. Section 9 concludes.

2 State Capacity and Clientelism

The concept of state capacity has been widely examined and debated in the social sciences.[3] Although definitions and measurement approaches vary, most studies converge on the idea of state capacity as the ability of the state to penetrate society and effectively implement political decisions across its territory—a concept Mann (1984) referred to as "infrastructural power."

State capacity has often been categorized into three core dimensions: extractive, coercive, and administrative (Soifer 2015; Berwick and Christia 2018; Hanson and Sigman 2021). Extractive capacity involves the state's ability to generate revenue, which is essential for funding public services and governance. Coercive capacity relates to the state's capability to maintain order and manage both internal and external threats through the deployment of force. Administrative capacity, in turn, underpins the effective implementation of policies, enforcement of laws, management of public resources, and execution of government functions at all levels. This capacity is typically associated with the presence of strong, professional, and well-equipped bureaucracies distributed across various jurisdictions. It also involves the

[3]For insightful overviews, see, e.g., Soifer (2008); Hendrix (2010); Centeno et al. (2017); Johnson and Koyama (2017); Berwick and Christia (2018); Hanson and Sigman (2021); Dincecco and Wang (2022); Suryanarayan (2024).

development of standardized operational procedures and the presence of essential infrastructure, such as buildings, equipment, technology, and data-gathering capabilities (Centeno et al. 2017; Besley and Persson 2009; Scott 1998). In the absence of a well-trained bureaucracy to monitor compliance, penalize violations, and ensure effective governance across the state's territory, enforcement is weak, laws may remain merely on paper, and policies fail to translate into tangible outcomes.

Historically, areas with limited state capacity have been particularly vulnerable to clientelism. In the "brown areas" described by O'Donnell (1983), where the state is territorially or functionally absent, programmatic promises lack credibility and are often unavailable (Keefer and Vlaicu 2008; Hicken and Nathan 2020). In such contexts, politicians frequently resort to clientelism, offering cash, consumption goods, public sector jobs, or selective access to publicly provided benefits in exchange for electoral support.[4] This contingent and targeted distribution of resources transforms "rights-claiming citizens into supplicants for special favors" (Bardhan 2022), creating a cycle of dependency that reinforces the incentives of clientelistic parties to preserve the status quo. In line with this, research shows that these parties actively resist investments in state capacity to protect their comparative electoral advantage, as stronger institutions shift voter preferences toward programmatic policies and public goods provision rather than particularistic transfers (Fergusson et al. 2022b). This resistance perpetuates the underprovision of public goods, reducing citizens' willingness to pay taxes and further weakening state capacity (Fergusson et al. 2022a). Additionally, clientelistic parties often politicize the hiring of public employees (Brollo et al. 2017; Colonnelli et al. 2020) and deliberately sustain low-skilled bureaucracies (De La O 2024). These practices undermine administrative capacity, obstruct broader development efforts, and entrench the very socioeconomic conditions that enable clientelism to thrive, reinforcing patronage as an appealing and enduring political strategy.

Although several studies have examined how clientelism influences state capacity, the causal effects of state capacity on clientelism remain less explored. Investigating this relationship empirically presents several challenges. One major issue is the self-selection of municipalities into capacity-building programs. This makes it difficult to discern whether observed effects are due to the programs themselves or to pre-existing conditions that both predispose certain municipalities to pursue such enhancements and independently influence levels of clientelism. Reverse causality further complicates this analysis, as clientelism affects the likelihood and effectiveness of state capacity investments (Fergusson et al. 2022a,b;

[4]See, for instance, Calvo and Murillo (2013); Mainwaring (1999); Robinson and Verdier (2013); Diaz-Cayeros et al. (2016); Hicken (2011); Khemani (2015); Kitschelt and Wilkinson (2007); Stokes et al. (2013).

De La O 2024). This dynamic is well illustrated by Gottlieb and Hollenbach (2021), who shows that Brazilian politicians attempting to improve fiscal capacity in municipalities with initially weak tax institutions face greater electoral penalties than those in better-equipped localities. Consequently, the heightened risk of political backlash in low-capacity municipalities may deter politicians from undertaking efforts to enhance local fiscal capabilities. Moreover, even when investments are undertaken, they are often less effective and less sustainable in low-capacity areas, limiting their potential to significantly reduce clientelism. These patterns not only highlight the endogeneity of such investments but also underscore the significant challenges involved in breaking the cycle of weak state institutions and reliance on particularistic politics.

This paper addresses these challenges by exploring how state capacity investments initiated by higher levels of government influence the electoral success of clientelistic parties in local elections. Focusing on externally driven interventions offers two key advantages. First, it addresses the issue of self-selection bias, whereby only municipalities that are already institutionally capable or less entrenched in clientelism choose to invest in building local state capacity. Second, it mitigates the problem of attrition bias, which arises when capacity-building efforts are sustained only in municipalities where clientelist networks fail to undermine, reverse, or co-opt them. Because these interventions are initiated and managed by higher levels of government, they are less susceptible to local political interference. This ensures that the observed outcomes capture a broader range of contexts, rather than reflecting only the experiences of the most favorable environments.

A number of national or supra-local programs have been shown to effectively weaken the grip of local clientelistic politics. For instance, Dower and Pfutze (2015) found that a land certification program in Mexico, which established secure property rights, significantly curtailed the ability of the dominant party to control voter behavior. In a similar vein, Frey's (2019) study in Brazil revealed that national redistributive policies, such as the *Bolsa Família* cash transfer program, have an indirect income effect on political behavior, reducing voter dependency on clientelism by permanently lifting people out of poverty. Notably, Frey (2019) also highlighted that these programs are particularly effective in weakening the influence of dominant political elites when they are designed to prevent the capture of resources. Together, these findings suggest that supra-local policies can erode the support base of local patrons by enhancing voters' economic well-being. Building on this insight, this paper argues that direct investments in state capacity can achieve similar outcomes by reducing voters' vulnerability to risk.

Informality as a Mechanism. Economic vulnerability is not solely a function of income *level* but also of income *variability* (Ligon and Schechter 2003). Scholars have con-

sistently demonstrated that economically disadvantaged individuals are particularly susceptible to clientelism (Calvo and Murillo 2004; Acemoglu 2005; Kitschelt and Wilkinson 2007; Stokes 2005; Robinson and Verdier 2013; Bobonis et al. 2022), as their precarious circumstances make the short-term benefits provided by patrons especially appealing. Conversely, as voters' economic situation improves, they may abandon clientelistic relationships, opting instead to make what Magaloni (2006) describes as "ideological investments" in other parties. However, poverty alone does not fully explain vulnerability to clientelism, nor does it perfectly correlate with individuals' exposure to risk and uncertainty.

Even among individuals with similar income levels, those exposed to greater risks—such as income variability, irregular work periods, and unpredictable pay—are more likely to rely on patron-client relationships. Informal workers, in particular, face higher levels of risk than their formal-sector counterparts due to their lack of access to social security mechanisms that could buffer them against life events such as illness, disability, unemployment, or old age. As Gottlieb (2024) highlights, informal work is characterized by its "uncertain, undocumented, and irregular relationship to the state," which not only lowers workers' expectations of the state's capacity to deliver public goods but also weakens their ability to mobilize around programmatic demands. Moreover, universal programmatic policies often fail to resonate with informal workers, who typically interact with the state through intermediaries rather than directly (Auerbach et al. 2018), and frequently view such policies as either inadequate or unlikely to reach them effectively (Rains and Wibbels 2023). As a result, informal workers tend to favor particularistic exchanges that deliver immediate, tangible benefits over impersonal, standardized policies (Altamirano 2015; Rains and Wibbels 2023; Gottlieb 2024).

This preference for particularistic exchanges aligns with evidence that clientelistic networks often serve as informal insurance mechanisms (Schmidt et al. 1977; Scott 1972), providing access to essential resources like financial aid, healthcare, and employment, which makes them particularly attractive to vulnerable populations (Murillo et al. 2021). However, reliance on these networks tends to decrease when sources of insecurity are effectively mitigated. For instance, Bobonis et al. (2022) found that targeted interventions, such as installing residential water cisterns in drought-prone areas of Brazil, significantly reduced residents' dependence on private benefits from local politicians. This discussion informs a broader theoretical expectation: strengthening the capacity of the state to effectively fulfill some of its core functions—such as providing social safety nets or delivering reliable public goods—can shield individuals from economic shocks, reduce their vulnerability, and ultimately decrease their reliance on clientelistic exchanges.

3 Conceptual Framework

Building on these insights, we argue that expanding the state's bureaucratic capacity and enhancing its ability to enforce labor laws effectively across its territory can reduce labor informality. By doing so, it diminishes citizens' reliance on personalistic exchanges and fosters a preference for more policy-oriented candidates. In response, clientelistic politicians are likely to adjust their strategies to adapt to the evolving electoral environment. These dynamics—driven by shifts in voter behavior on the demand side and strategic adaptations by politicians on the supply side—are expected to jointly contribute to a decline in clientelism.

As workers transition into the formal sector and gain access to mechanisms that mitigate economic risks—such as social security, health insurance, or stable employment—their reliance on informal patron-client networks as a form of insurance diminishes. This shift is particularly significant, as scholars have shown that clientelism is not solely a top-down strategy driven by politicians; voters play an active role in sustaining these exchanges. Acting as key instigators, they frequently approach brokers and politicians to demand assistance and negotiate agreements (Auyero 2000; Lindberg 2010; Nichter and Peress 2017; Nichter 2018; Hicken et al. 2019; Nathan 2019). Consequently, as labor regulation enforcement improves and informality decreases, voter-initiated demands for particularistic exchanges are expected to decline.

Without proactive voter engagement, clientelist politicians face three critical challenges. First, transaction costs increase as politicians must devote greater effort in locating and negotiating with potential targets in an environment where voter preferences and intentions are less transparent. Second, as voters become less vulnerable, politicians must offer larger material incentives to win their support, further increasing the overall cost of clientelistic strategies. Third, the decline in voter-initiated demands also undermines the effectiveness of clientelistic exchanges. In the absence of clear signals from voters, politicians are more likely to misallocate resources by directing benefits toward individuals who are less inclined to reciprocate with electoral support. These inefficiencies undermine the returns on clientelistic investments, reducing their overall appeal and viability as a political strategy in contexts where voters are less vulnerable and less inclined to initiate exchanges.

In response to these changing conditions, office-seeking candidates aiming to maximize their electoral prospects may adopt one of three approaches: shifting strategies, persisting with clientelism, or withdrawing from the race altogether. The first approach involves transitioning from clientelist strategies, such as offering targeted transfers, to programmatic promises centered on public goods provision. This transition not only diminishes the overall reliance on clientelistic tactics—including vote buying, turnout buying, abstention buying,

and double persuasion (Gans-Morse et al. 2014)—but also reduces the electoral support for parties that rely on these strategies. This decline occurs because candidates from clientelistic parties typically lack credibility in delivering public goods and fulfilling broader policy promises. As a result, even with a strategic pivot to programmatic appeals, these candidates are likely to attract fewer votes. The second possible response is for candidates to continue relying on clientelistic strategies, despite their declining effectiveness. As voter vulnerability decreases and the cost of securing individual votes rises, candidates with fixed budget constraints are able to purchase only a fraction of the votes they could previously afford. This scenario leads to both a decline in the frequency of vote-buying and further erosion of electoral support for clientelistic parties. Based on this, we propose the following testable hypothesis:

H1. *As enforcement increases, the prevalence of particularistic exchanges declines.*

The third and most drastic response involves candidates from clientelistic parties withdrawing from the race entirely. Faced with lower credibility when making programmatic appeals and the higher costs associated with vote-buying, some candidates may conclude that they are unable to compete effectively in the transformed political landscape. This withdrawal not only reduces the incidence of vote-buying and the vote share for clientelistic parties, as the first two responses do, but also leads to a decline in the number of candidates from clientelistic parties contesting elections. Consequently, on the supply side, we hypothesize:

H2. *Increased enforcement leads to a reduction in the number of candidates from clientelistic parties running for office.*

On the demand side, if reduced vulnerability diminishes the appeal of clientelistic candidates to voters, we would expect a decline in the vote share of clientelistic parties, even when accounting for supply-side constraints. This expectation leads to the following testable hypothesis:

H3. *Enhanced enforcement lowers the vote share of clientelistic parties, even when accounting for the proportion of their candidates contesting the election.*

Collectively, these dynamics give rise to our final hypothesis:

H4. *Stronger enforcement decreases the probability of a clientelistic party candidate winning local elections.*

These hypotheses establish a framework for empirically examining the relationship between enhanced labor law enforcement, declining informality, and shifts in clientelist politics.

The combined supply- and demand-side dynamics are synthesized and illustrated in Figure 1. Section 5 details the empirical strategy and data employed to test these hypotheses, building on the contextual foundation established in Section 4.

4 Context

4.1 Labor Regulations and Enforcement Capacity

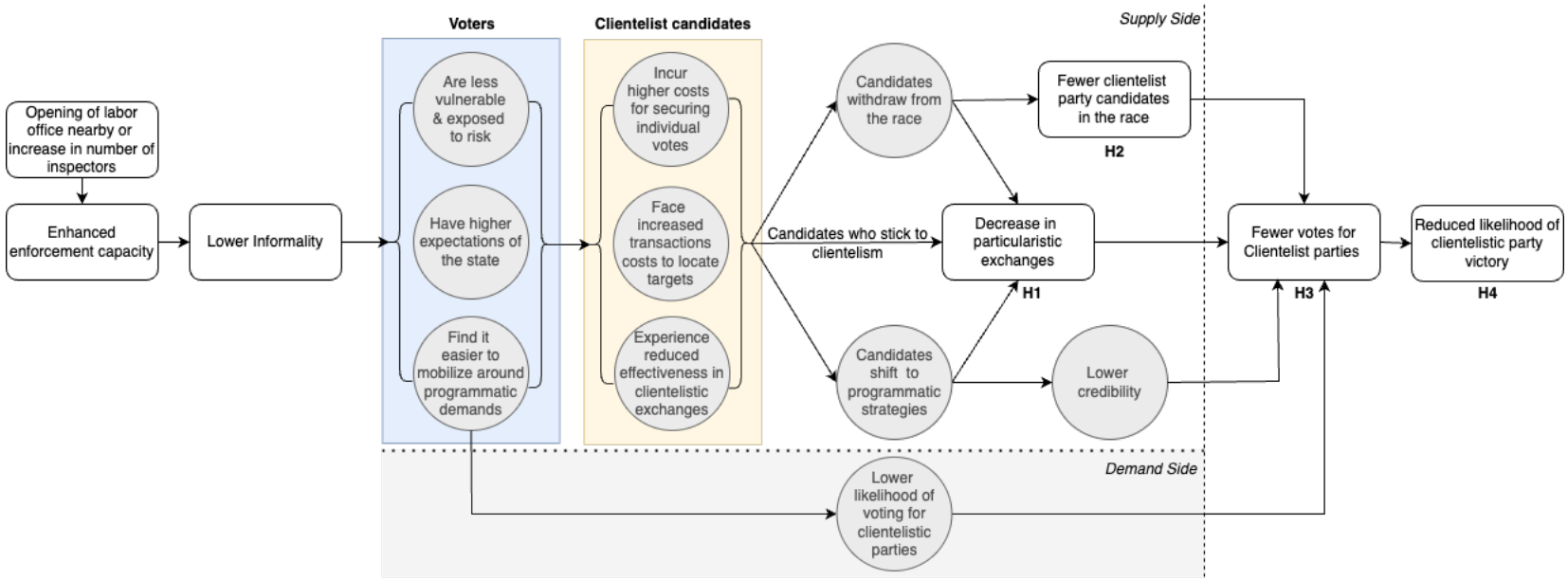
In Brazil, the labor code known as the *Consolidação das Leis Trabalhistas* (CLT), established in 1943 and based on the civil law system, governs the permissible types of labor contracts, including their conditions and terms of termination. The 1988 Federal Constitution significantly expanded labor regulations and worker benefits, increasing the costs associated with hiring and firing (de Barros and Corseuil 2004).

Labor taxes in the country are notably high, with major components including a 20% social security contribution, 9% direct payroll taxes, and an 8.5% contribution to a severance fund. Under existing regulations, formal workers are required to hold a *carteira de trabalho*—a labor card issued by the Ministry of Labor that records their entire formal employment history. Registration with this card entitles workers to key benefits, including unemployment insurance, severance pay, paid vacation, and overtime premiums. Additionally, workers who are unjustly dismissed are entitled to further compensation from their employers. However, these substantial benefits are accessible only to a portion of the workforce; as of 2010, nearly 32% of workers were either self-employed or engaged in informal jobs, leaving them excluded from these protections.

The high complexity and cost of these regulations, which often push firms and workers toward informal labor arrangements, highlights the critical importance of enforcement in promoting formalization. Enforcement is primarily the responsibility of the Ministry of Labor, which operates through a decentralized network. At the state level, labor offices known as *Superintendências Regionais do Trabalho e Emprego* (SRTE) are based in state capitals, with local offices, known as *Gerências Regionais do Trabalho e Emprego* (GRTE), are distributed across municipalities. The presence and capacity of these offices vary with the state's size and economic importance (Almeida and Carneiro 2012).

Inspectors, stationed at specific *gerências*, periodically rotate to help deter corruption (Cardoso and Lage 2007). They are tasked with evaluating firms on various aspects of labor law compliance, including the formal registration of workers, adherence to minimum wage regulations, and observance of working hours. Their compensation includes a performance-based component, which can constitute up to 45% of their salary (Almeida and Carneiro

Figure 1: Enforcement Capacity and Support for Clientelist Parties: Demand and Supply Side Channels



Notes: The diagram depicts the pathways through which enforcement can affect the local electoral performance of clientelistic parties. Gray circles represent the underlying mechanisms outlined in the conceptual framework, while white rectangles denote the variables employed to empirically test the proposed hypotheses.

2012). Firm inspections may be triggered by random audits or reports of noncompliance. These reports can originate from workers, unions, the public prosecutor's office, or the police. However, most inspections typically begin based on anonymous reports.

Local offices operate under general directives from the national headquarters of the agency, but ground-level inspectors retain discretion over which establishments to visit and whether to address violations informally or proceed with formal citations and adjudication. Clientelist incumbents could, thus, attempt to disrupt the enforcement of labor laws. Such interference might be expected to bias our estimates of inspection effects toward zero. We will examine this issue in greater detail in the sections 5.2 and 7.

Enforcement intensity exhibits substantial variation both across and within states. Previous research highlights that the density of local labor offices and the travel distances that inspectors must cover to reach firms are crucial factors impacting enforcement capacity across local labor markets (Almeida and Carneiro 2012; Ponczek and Ulyssea 2022). In addition to the geographic distribution of these offices, states also vary in the number of inspectors assigned to each location. Furthermore, variability in the timing of the openings of labor offices means that regions previously not monitored become more likely to undergo inspections over time. This spatial and temporal diversity in enforcement infrastructure critically influences the effectiveness of labor law compliance.

4.2 Brazilian Parties and Local Elections

Brazil's municipal governments operate with a high degree of autonomy, controlling significant resources and overseeing the provision of essential public services such as education, health, and infrastructure. Mayors are elected every four years through a majority rule system and are subject to a term limit of two consecutive terms.[5] They hold substantial authority over local budgets and resource allocation. A critical dimension of this autonomy is the patronage power wielded by mayors, particularly through the distribution of temporary contract jobs and commissioned positions. While Brazil's civil service is largely meritocratic, these discretionary appointments enable mayors to reward political supporters and consolidate clientelistic networks (Brollo et al. 2017; Colonnelli et al. 2020; Toral 2023).

The broader political environment also facilitates clientelism. Brazil's party system is both fragmented and characterized by weak ideological linkages between voters and parties. Although some scholars have suggested that voter-party ties are stronger than commonly portrayed (Braga and Pimentel 2011), the prevailing view is that these connections remain

[5]Municipalities with over 200,000 registered voters hold a second round of elections if no candidate secures more than 50% of the votes in the first round.

tenuous (Limongi and Cortez 2010; Ferreira et al. 2008). Indeed, Samuels and Zucco (2014) show that party identification among Brazilian voters has historically remained low, never surpassing 50%. This weak ideological alignment fosters an environment in which most parties rely heavily on clientelistic rather than programmatic electoral strategies, making them largely interchangeable (Desposato 2006).

An emblematic example is the Liberal Front Party (PFL), a machine party that emerged during Brazil's transition to democracy. The PFL split from the pro-military regime party and built its influence through patronage and clientelistic practices rather than programmatic appeals (Power 2000). Similarly, the Brazilian Democratic Movement Party (PMDB) operates as a loose coalition of state and local elites. Despite its state-level strength and participation in most national governing coalitions, the PMDB's central leadership has limited authority over its lower-level politicians and state branches, further exemplifying the decentralized and clientelistic nature of Brazilian politics (Mainwaring 1999).

While non-programmatic linkages predominate, two parties stand out for their relative institutionalization and emphasis on policy-centered appeals: the center-left Workers' Party (PT) and the center-right Brazilian Social Democratic Party (PSDB). The PT, founded in 1980 by labor union activists, grassroots movements, and Catholic-based communities, established strong links with civil society and enforced high levels of internal discipline, enhancing its ability to pursue programmatic policy goals centered on social policies and public goods provision (Samuels 2004; Hunter 2010). The PSDB, while less institutionalized than the PT, also displayed a relatively coherent ideological profile and centralized structure. Their rivalry dominated Brazil's presidential races from 1994 to 2014, influencing subnational elections as well (Mainwaring et al. 2018). Collectively, these four parties won an average of 42.7% of mayoral elections across the seven electoral cycles held between 1996 and 2020. Beyond them, Brazil's electoral scene also features a large and diverse group of small and medium-sized parties that span the ideological spectrum (Zucco 2011).

5 Empirical Framework

5.1 Data

Our primary measure for assessing the effectiveness of labor regulation enforcement at the municipal level is the logarithm of inspections per 100 firms from 1995 to 2020. Data regarding the number of inspections and inspectors was sourced from the Brazilian Ministry

of Labor.[6] As indicated in Table 1, the average rate is three inspections per hundred firms, although there is considerable variation across municipalities. Additionally, we examine the spatial distribution of the Ministry of Labor's administrative capacity by analyzing the locations of regional and local labor offices and the distribution of inspectors across states. Table 1 shows that each office has an average of 24 inspectors, and the average distance between municipalities and their nearest office is 166 kilometers. However, the dispersion in these measurements is also substantial.

To capture temporal variation in enforcement intensity, we incorporate annual changes in the average number of inspectors per office at the state level, as well as the establishment dates of each labor office. Information on office establishment dates was primarily sourced from inspection data provided by the Brazilian Ministry of Labor, where each inspection record specifies the responsible office. We designate the first year an office appears in the dataset as its inauguration year, and similarly, if an office ceases to appear, we mark the final year as its closing year.[7] For offices established before 1995, we supplemented this data with information from Ponczek and Ulyssea (2022) and directly contacted offices not covered in their dataset. Among the 139 offices nationwide, ninety-one were established before 1990, thirty-two between 1991 and 2000, and the remainder after 2000. We calculate the distance from the centroid of each municipality to the nearest labor office for each year using car travel routes provided by the OpenStreetMap API.[8] While OpenStreetMap offers detailed, current road maps, several roads have been constructed since 1996 (the first election year in our dataset). To ensure robustness, we conduct additional checks using alternative distance measures, including Euclidean distances and distances calculated using historical road maps for the years 2000, 2005, 2010, and 2015, provided by de Castro Victoria et al. (2021).

To measure our primary dependent variable—support for clientelistic parties at the municipal-level—we rely on three expert surveys that gauge the extent of clientelism in party organizations: the Democratic Accountability and Linkages Project (DALP) (Kitschelt et al. 2013), V-Party (Lindberg et al. 2022), and the Global Party Survey (GPS) (Norris 2020). These surveys collect assessments from international experts on the political behavior of parties, including clientelistic practices. Appendix OA.1 provides details on the questions, scales, and political parties included in each survey, along with their respective clientelism scores. From these scores, we create binary classifications for each survey to serve as our baseline measures. Since the scores are assigned at the party level, we apply the correspond-

[6]This data was obtained through a request made under the *Lei de Acesso à Informação* (Access to Information Law).

[7]Only seven offices closed during the studied period.

[8]See https://www.openstreetmap.org.

Table 1: Descriptive Statistics

	Mean	Standard deviation	Median
Enforcement			
Inspections per 100 firms	2.95	5.06	1.52
Car distance to nearest LO (per 100 km)	1.66	5.96	0.99
Inspectors per office (region level)	23.57	11.71	20.51
Clientelism			
Probability of winning clientelistic party (DALP) (%)	59.09	49.17	100.00
Probability of winning clientelistic party (V-party) (%)	52.39	49.94	100.00
Probability of winning clientelistic party (GPS) (%)	42.21	49.39	0.00
Salience of clientelism (GPS) (%)	37.44	17.48	42.90
Share of clientelistic candidates running for office (DALP) (%)	54.41	33.71	50.00
Share of individuals reporting vote-buying (Latinomarometro) (%)	26.90	19.05	23.08
Share of individuals reporting vote-buying (LAPOP) (%)	23.40	19.43	21.43
Municipal bureaucracy			
Num. of bureaucrats	737	1038	413
Share of temporary bureaucrats (%)	15.71	27.41	1.72
Share of graduate bureaucrats (%)	24.32	15.46	23.94
Labor market			
Share of formal workers, Census (%)	20.11	12.40	17.47
Share of informal workers, Census (%)	17.38	7.05	16.72
Share of self-employed workers, Census (%)	0.18	0.09	15.83
Share of non-employment, Census (%)	39.34	10.54	38.95
Formal workers in log, RAIS	6.23	2.11	6.21
Num. of municipalities	5454		
Num. of observations	36458		

Yearly enforcement data, covering the period from 1995 to 2020, is obtained from the Brazilian Ministry of Labor. Car distances are calculated using OpenStreetMaps. Clientelism measures are derived from the Democratic Accountability and Linkages Project (DALP) (Kitschelt et al. 2013), V-Party (Lindberg et al. 2022), and the Global Party Survey (GPS) (Norris 2020). These data are applied to municipal elections held in 1996, 2000, 2004, 2008, 2012, 2016, and 2020, excluding extraordinary elections. Additional data from Latinobarómetro encompass 3,173 individuals surveyed across 63 municipalities in 2002, 2005, 2006, 2017, and 2018. Census data for the analysis are from 1991, 2000, and 2010. The shares of formal, informal, and non-employed individuals are calculated as proportions of the working-age population, defined as those aged 18 to 64 who are not enrolled in school. Detailed information on data sources and variable construction is provided in Appendix OA.1.

ing classification to all candidates affiliated with a given party.[9] Our preferred measure comes from DALP, which aggregates multiple clientelism-related questions into a comprehensive index, covers a wide range of parties, and has been previously applied in studies on Brazil (Frey 2019). According to this measure, 60% of mayors belong to parties classified as clientelistic, underscoring the widespread prevalence of such practices (Table 1). While our main analysis classifies clientelism based on the party affiliation of the elected mayor, Section 8 presents supplementary analyses using the average clientelism score of all parties within the mayor's coalition.

While the expert surveys provide a broad assessment of the extent to which parties engage in clientelistic practices—such as vote-buying, selective provision of public goods, allocation

[9]Following Frey (2019), parties with missing scores are coded as non-clientelistic, as these are typically small parties with minimal clientelistic practices. However, Section 8 demonstrates the robustness of our results when excluding these parties or treating them as clientelistic.

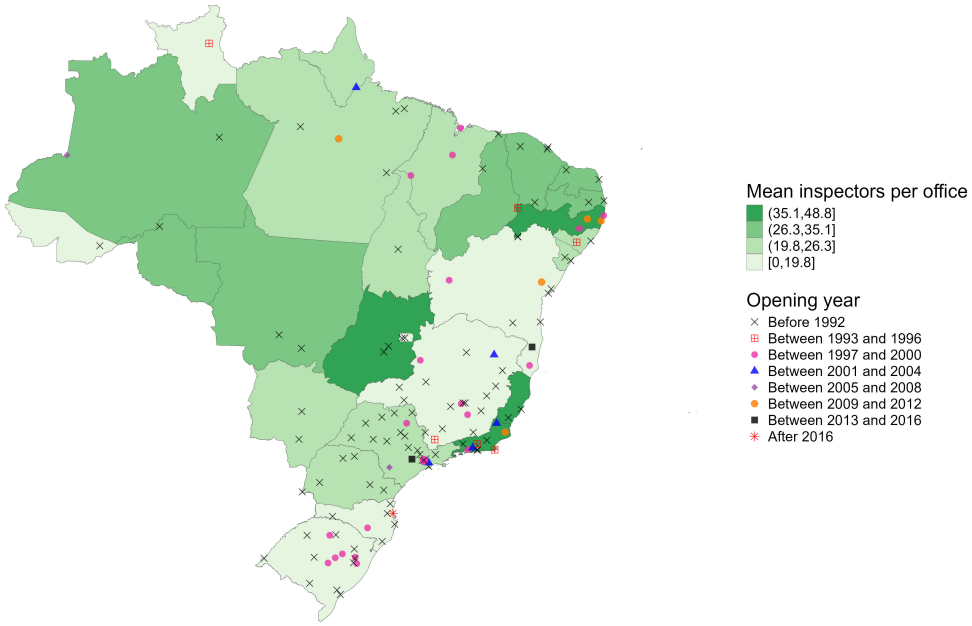
of public sector jobs, and distribution of public contracts—they are measured at the party level and may not fully capture regional variations in how the same party employs clientelism across different regions. To address this limitation and enhance the robustness of our findings, we incorporate additional measures from two complementary sources—administrative records and individual-level surveys—that capture two specific forms of particularistic exchanges: vote-buying and patronage. To assess vote-buying, we examine individual-level data from Latinobarómetro. Although this survey is not representative at the municipal level, they indicate that approximately 27% of respondents report experiencing vote-buying within their municipality. For patronage, we draw on extensive data on municipal-level bureaucratic contracts. In Brazil, bureaucrats generally hold one of two types of contracts: (1) permanent (*estatutário*) or (2) temporary (*temporário*). Permanent contracts require candidates to pass competitive examinations and are regulated by civil service statutes (*estatuto*) enacted by the municipal government. According to the Brazilian constitution, all long-term positions must be filled through these contracts, which grant lifetime tenure—following a probationary period—to the top-ranked candidates in competitive examinations. Temporary contracts, by contrast, are legally intended for political appointees or for filling short-term or urgent staffing needs. However, they are sometimes used inappropriately in contexts where permanent contracts would be more suitable. Unlike their permanent counterparts, temporary employees typically receive one-year contracts that generally expire in December and can be terminated relatively easily. Given this greater discretion in hiring and firing we use variation in the number of temporary contracts as an indicator of patronage at the local level, following Brollo et al. (2017), Colonnelli et al. (2020), Toral (2023), and Ferrali et al. (2024). As shown in Table 1, 16% of the bureaucrats in our sample are appointed through temporary contracts.

Figure 2 illustrates the key variations examined in our analysis. Panel A maps the spatial distribution of enforcement capacity, measured as inspectors per office, as well as the opening of local labor offices over time. This visualization reveals substantial fluctuations in enforcement capacity across both municipalities and time periods. Panel B, in turn, highlights the varying success of clientelistic parties across Brazilian municipalities. Together, these panels underscore the wide-ranging differences in enforcement intensity and clientelistic influence that form the core of our empirical analysis.

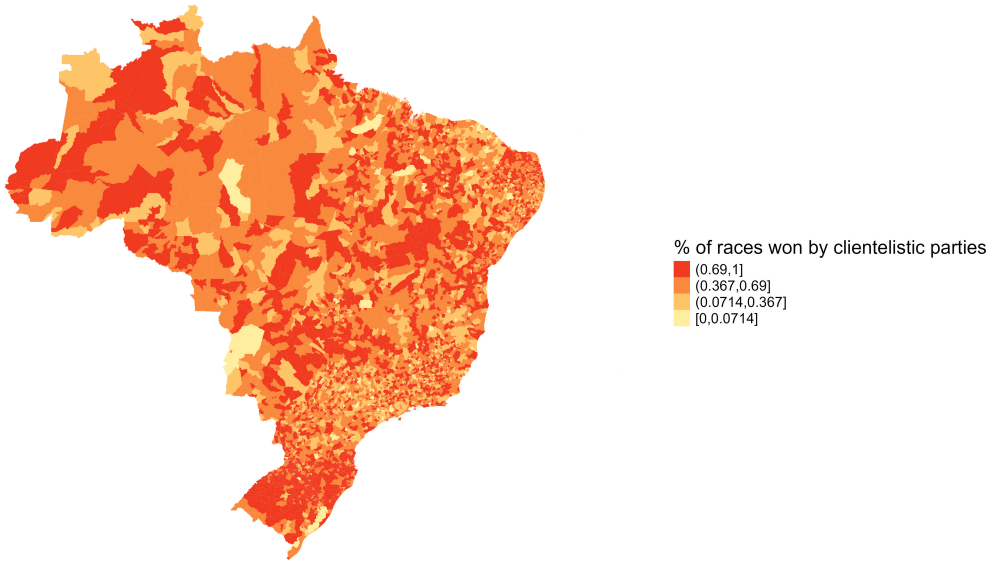
Lastly, to investigate the mechanism through which increased enforcement is hypothesized to influence clientelism, we draw on individual labor market data from the Decennial Population Census. These data include the shares of workers who are formally employed (with a signed labor card), informally employed (without a signed labor card), self-employed,

Figure 2: Clientelism and Enforcement of Labor Regulations

Panel A: Enforcement Capacity: Labor Offices and Inspectors

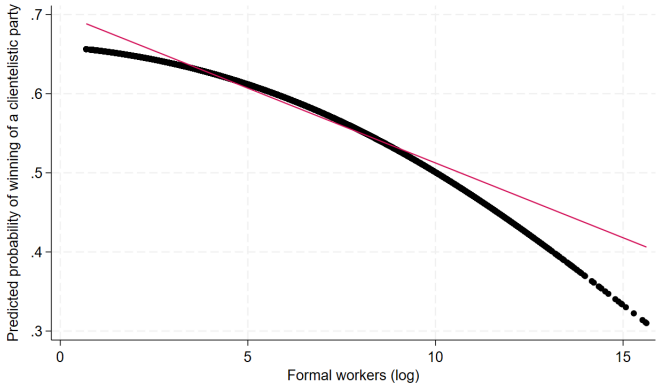


Panel B: Prevalence of Clientelism:
Percentage of Municipal Elections Won by Clientelistic Parties (1996–2020)



Notes: Panel A displays the distribution of the average number of inspectors per office from 1995 to 2020, and labor offices categorized by their opening year. Panel B illustrates the percentage of municipal races won by clientelistic parties across the seven elections held between 1996 and 2020. Parties are classified according to the DALP clientelistic scores (Kitschelt et al. 2013). For a detailed description of the variables, refer to Appendix OA.1.

Figure 3: Clientelism and Formality



The figure displays the relationship between the probability of a mayor from a clientelistic party winning and the size of the formal sector. The dots represent predictions derived from a probit model that includes a quadratic term for formal workers, while the red line shows predictions estimated using a linear OLS model. Clientelistic parties are identified based on the DALP survey (Kitschelt et al. 2013), and estimates of formal workers are derived from RAIS data. Additional details on the variables are provided in the Appendix OA.1.

and non-employed (either unemployed or outside the labor force).[10] To complement the Census data, we use the *Relação Anual de Informações Sociais* (RAIS), an administrative dataset from the Ministry of Labor that covers all formal firms and workers. As reported in Table 1, Brazilian municipalities exhibit high levels of informality; in the median municipality, informal workers make up 17% of the economically active population, a proportion comparable to that of formal workers. Moreover, as illustrated in Figure 3, there is a strong negative correlation between the size of the formal sector and the probability that a clientelistic party will win local elections.

5.2 Empirical Strategy

The aim of our empirical analysis is to determine whether municipal-level labor regulation enforcement influences the level of support for clientelistic parties. To capture enforcement, we rely on the logged number of inspections per 100 firms in each municipality. Our research design leverages a panel dataset covering seven Brazilian election years (1996, 2000, 2004, 2008, 2012, 2016, and 2020), allowing us to examine how changes in enforcement intensity over time affect electoral outcomes. We start by considering the following reduced form model:

$$Y_{m,t} = \beta \; Enforcement_{m,c(t)} + \mu_m + \lambda_t + \epsilon_{m,t} \tag{1}$$

[10]Census data does not distinguish between unemployment and being outside the labor force.

where m denotes the municipality and, when we use election-year data, t corresponds to the election year, $Y_{m,t}$ is the outcome of interest, $Enforcement_{m,c(t)}$ indicates the mean log number of inspections per 100 firms per year in municipality m during the entire electoral cycle preceding the election at year t, μ_m is the vector of municipality fixed effects, λ_t is the vector of election-year fixed effects, and $\epsilon_{m,t}$ denotes the idiosyncratic error.[11] For analyses conducted at the calendar-year level, we use $Enforcement_{m,t-1}$ as the main independent variable. This measure represents the logged number of inspections per 100 firms in municipality m during the previous calendar year $(t-1)$. Municipality fixed effects account for time-invariant confounders within each municipality. For instance, variables such as average poverty, level of development, climatic and geographic conditions, proximity to relevant markets, state-specific characteristics, and the general provision of local public goods and infrastructure are all accounted for by incorporating municipality fixed effects. Election-year or calendar-year fixed effects control for events specific to each year that affect all municipalities uniformly, such as the party of the president, economic crises, or the pandemic. β is the parameter of interest and measures the differential impact of enforcement on political outcomes across municipalities.

Our primary challenge for identification is the potential endogeneity of our enforcement measure (inspections per firm). For example, the government might adjust enforcement resources in response to changes in local labor market dynamics. Alternatively, non-enforcement may result from a political decision by bureaucrats or politicians (Holland 2017; Feierherd 2020; Tendler 2002; Amengual and Dargent 2020; Brollo et al. 2020). Moreover, this measure could be subject to measurement error. To address these issues, we adopt an instrumental variable approach. Following the logic of Almeida and Carneiro (2012) and Ponczek and Ulyssea (2022), our instrument exploits the interaction between the mean number of state-level inspectors per office ($Inspectors_{r,c(t)}$) and the travel time by car to the nearest labor office ($Distance_{m,c(t)}$). This approach leverages the core components of the enforcement capacity—inspector availability and the logistical hurdles of office access. The underlying intuition is that municipalities located farther from enforcement offices are less likely to receive inspections, especially where the number of inspectors is limited. In a fixed-effects panel model, the validity of the instrument also depends on incorporating temporal variation. To that end, we use data on the establishment years of new labor offices and on changes in the number of state inspectors over time. These elements provide dynamic

[11] In Brazil, elections generally take place in October, and the winning mayor assumes office the following January. As a result, we define an electoral cycle as beginning the year after an election and extending until the year of the next election. For example, for the 2000 election, the preceding electoral cycle covers the years 1997 through 2000.

fluctuations in enforcement infrastructure that are essential for our analysis. The first stage of our two-stage least squares (2SLS) instrumental variable (IV) estimation is specified as follows:

$$\begin{aligned} Enforcement_{m,c(t)} = \; & \alpha_1 \; Inspectors_{r,c(t)} + \alpha_2 \; Distance_{m,c(t)} + \alpha_3 \; Inspectors^2_{r,c(t)} + \\ & \alpha_4 \; Distance^2_{m,c(t)} + \alpha_5 \; Inspectors_{r,c(t)} \times Distance_{m,c(t)} + \\ & \mu_m + \lambda_t + \epsilon_{m,t} \end{aligned} \tag{2}$$

where, when using election-year data, $Distance_{m,c(t)}$ represents the average car travel distance (in units of 100 km) between municipality m and the nearest labor office over the preceding electoral cycle, and $Inspectors_{r,c(t)}$ is the average number of inspectors per office in the corresponding state during the same period. For year-level regressions, we use $Enforcement_{m,t-1}$, which is defined as the logged number of inspections per 100 firms in municipality m during the previous year $(t-1)$, $Distance_{m,t-1}$, which denotes the car travel distance between the municipality and the nearest labor office (per 100 kms) in year $t-1$, and $Inspectors_{r,t-1}$, the number of inspectors per office at the state level in year $t-1$.

In our primary first-stage specification, we allow for heterogeneous instrument effects, following Dieterle and Snell (2016). We also test the robustness of our results using a linear first-stage model, as reported in Section 8. In the second stage, we regress the likelihood of success of a clientelistic party—or our other dependent variables of interest—on the predicted enforcement intensity, denoted by $\widehat{Enforcement}_{m,c(t)}$:

$$Y_{m,t} = \beta \; \widehat{Enforcement}_{m,c(t)} + \mu_m + \lambda_t + \epsilon_{i,t} \tag{3}$$

The primary threat to identification arises from the non-random placement of labor offices. The Ministry of Labor has established specific criteria that a municipality must meet to host a labor office (see Appendix OA.2). In particular, the location of these offices is influenced by factors such as population and labor market conditions. However, it is important to emphasize that these criteria apply solely to the host municipality where the labor office is established, not to neighboring municipalities that inspectors travel to for firm inspections. As a result, we exclude host cities from our analyses and assume that the placement of labor offices is exogenous for the remaining municipalities.

Another identification assumption is that pre-existing trends in outcomes are unrelated to municipal distance from labor offices and the number of inspectors at the state level. In Section 8, we provide evidence supporting this assumption, demonstrating that distance to labor offices and inspector availability are uncorrelated with the mayor's level of clientelism. This aligns with the fact that decisions regarding labor office placement and inspector staffing

are made at higher administrative levels, independent of local political dynamics (see Section 4). Furthermore, results from an event-study approach, which exploits the staggered timing of labor office openings, reveal parallel trends in clientelism and the prevalence of labor formalization, strengthening confidence in the validity of our identification strategy.

6 Results

6.1 Enforcement and Clientelism

We begin our analysis with H4, the most comprehensive hypothesis, as it encapsulates the overall impact of enforcement on local clientelistic dynamics. Next, we examine H2, which reflects supply-side effects, and then turn to H3, which focuses more specifically on demand-side dynamics. These analyses leverage municipal-level data on electoral outcomes together with clientelism scores from expert surveys. Lastly, we address H1, using individual-level survey data on reported vote-buying.

To assess the impact of enforcement intensity on the likelihood of candidates from clientelistic parties winning elections, we rely on two key outcome measures. First, we analyze the probability that a candidate from a clientelistic party is elected mayor, based on classifications from expert survey scores. Second, we examine the likelihood that clientelism is identified as a "salient aspect" for the elected party. This is our most restrictive measure, reflecting the proportion of survey respondents who select clientelism as the most important strategy for a party to attract and retain voters, derived from a single-choice question that includes 17 possible options. These variables are derived from the three expert surveys discussed earlier: DALP, GPS, and V-Party (see Section 4 and Appendix OA.1). OLS estimates of Equation (1) are displayed in Table A.1. The analysis indicates that an increase in the number of inspections during the previous electoral term is associated with a reduced likelihood of a clientelistic party winning the election and a lower salience of clientelism for the elected party. However, the magnitude of this effect is relatively small, and for one of the four examined outcomes, the effect is not statistically significant.

As previously noted, the measure of inspections per firm is potentially subject to both measurement error and endogeneity. To address these issues and provide a more direct interpretation of enforcement effects, we estimate an instrumental variable model. In this model, car travel distance to the nearest labor office and the number of inspectors per office at the state level, along with their squared terms and interaction, are employed as instruments for enforcement intensity (see Equation 2). The first-stage results are presented in Table A.2. As expected, the number of inspections decreases as the distance to the

Table 2: Effects of Enforcement on Clientelism: Expert Surveys

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-Party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-Party)
$\text{Enforcement}_{m,c(t)}$	−0.383***	−0.239***	−0.145**	−0.205***
	(0.083)	(0.074)	(0.072)	(0.038)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36441	36441	36441	36441
Kleibergen-Paap rk LM statistic	92.68	92.68	92.68	92.68
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.17	16.17	16.17	16.17
Hansen J statistic	10.16	20.93	7.62	15.34
p-value	0.038	0.000	0.107	0.004

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before the election at time t. Parties are categorized as clientelistic based on classifications derived from the corresponding scores in each survey. For a detailed description of the variables, see Appendix OA.1.

nearest labor office increases, reflecting the logistical challenges inspectors face with longer travel times. Conversely, the number of inspections increases with a higher average number of inspectors per office in the state. Both relationships exhibit non-linear patterns. Moreover, the interaction term between distance and the number of inspectors has a negative coefficient, suggesting that the effectiveness of additional inspectors diminishes as travel distances grow.

Our main results, shown in Table 2, indicate that strengthening enforcement capabilities significantly decreases both the likelihood of electoral victories for clientelistic parties and the salience of clientelism for the elected party, providing support for H4. Specifically, when the number of inspections increases by 10%, there is a notable 3.8 percentage point decrease in the probability of a clientelistic party winning, as measured by the DALP score. This shift corresponds to a decrease in the proportion of municipalities governed by clientelistic parties from 59.1% to 55.27%. The instrumental variable (IV) estimates, which are both larger in magnitude and more statistically significant than the ordinary least squares (OLS) estimates, suggest that clientelism is more prevalent in municipalities with lower state capacity. Diagnostic statistics are reported at the bottom of the table, including the Kleibergen-Paap (2006) test for underidentification and weak instruments. The Kleibergen-Paap statistic strongly rejects the null hypothesis of underidentification across all models. Furthermore, the Hansen J statistic rejects the null hypothesis of overidentification, providing additional confidence in the validity of the instruments.

We next examine the overall electoral support for clientelist parties in mayoral elections.

Table 3: Effects of Enforcement on Clientelism: Supply-Side Dynamics

	(1) Number of candidates (log)	(2) Number of clientelistic candidates (log)	(3) Number of non-clientelistic candidates (log)
$\text{Enforcement}_{m,c(t)}$	−0.337***	−0.385***	−0.091
	(0.045)	(0.068)	(0.060)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep.var	1.232	0.765	0.714
Observations	36441	36441	36441
Kleibergen-Paap rk LM statistic	109.42	109.42	109.42
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	21.09	21.09	21.09
Hansen J statistic	22.54	10.04	34.90
p-value	0.000	0.040	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Clientelistic party is defined using the DALP database, as described in Appendix OA.1.

The results, shown in Table A.3, indicate that a 10% increase in labor inspections corresponds to a reduction in vote share for clientelist parties by 0.8 to 2.5 percentage points, depending on the measure used. This complements the findings on the likelihood of victory by revealing how enforcement undermines clientelist parties' ability to mobilize voters, even when they still win elections. A lower vote share weakens their mandate, reduces legitimacy, and limits opportunities to sustain or expand clientelistic networks, posing long-term challenges to clientelism as a viable strategy.

Building on the framework outlined in Section 2, we analyze the supply-side effects underlying our main findings, as reflected in H2. Table 3 demonstrates that increased enforcement leads to a reduction in the number of candidates participating in mayoral elections (Column 1). Notably, this decline is entirely attributable to a decrease in candidates from clientelistic parties (Column 2), with no impact on the number of candidates from non-clientelistic parties (Column 3).

To assess the demand-side dynamics, we re-estimate the analysis presented in Table A.3, adding a control for the proportion of candidates from clientelistic parties running for office. As shown in Table 4, the effect of enforcement on the vote share of clientelistic parties remains negative and statistically significant for two out of three measures, consistent with H3. Complementary results, using the probability of winning as the dependent variable, are presented in Table A.4, showing significant effects for all of our measures of clientelism. These results highlight both supply- and demand-side effects. The reduced magnitude sug-

Table 4: Effects of Enforcement on Clientelism: Demand-Side Dynamics

	(1) Vote share of clientelistic parties (DALP)	(2) Vote share of clientelistic parties (V-Party)	(3) Vote share of clientelistic parties (GPS)
$\text{Enforcement}_{m,c(t)}$	−0.148***	−0.064	−0.151***
	(0.042)	(0.040)	(0.039)
Num of clientelistic candidates $(\log)_{m,t}$	0.700***	0.659***	0.556***
	(0.005)	(0.005)	(0.005)
Num of candidates $(\log)_{m,t}$	−0.514***	−0.431***	−0.340***
	(0.010)	(0.009)	(0.009)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep.var	0.443	0.377	0.287
Observations	36414	36414	36413
Kleibergen-Paap rk LM statistic	86.21	85.99	86.46
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	15.20	15.15	15.23
Hansen J statistic	4.71	2.61	2.27
p-value	0.318	0.625	0.686

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. The dependent variables represent the vote share in the first round of elections for all candidates from parties identified as clientelistic, according to the various survey-based classifications. Similarly, the number of candidates from clientelistic parties is determined based on the classification provided by the corresponding scores from each survey. For a detailed description of the variables, see Appendix OA.1.

gests enforcement influences outcomes in part by decreasing the share of candidates from clientelistic parties contesting elections. However, the persistent negative effect of enforcement also points to a shift in voter behavior, weakening support for clientelistic parties and further eroding their electoral viability.

Next, we examine whether increased enforcement affects the prevalence of particularistic exchanges, as predicted by H1. As discussed in Section 2, we hypothesize that increased enforcement and reduced voter vulnerability prompt politicians to decrease the number of clientelistic offers they make. To evaluate this, we analyze individual-level data from five waves of the nationally representative Latinobarómetro survey. This survey geolocates respondents and collects information about their experiences with particularistic transfers during recent elections, offering valuable insights into the prevalence of such exchanges from the perspective of the electorate.[12] The results presented in Table 5 indicate that increased

[12]The wording of the questions varies slightly across different waves. For detailed descriptions of the relevant questions, see Appendix OA.1.

Table 5: Effects of Enforcement on Clientelism: Individual Reports of Vote-Buying During Elections

	(1) Reported vote-buying	(2) Reported vote-buying
$\text{Enforcement}_{m,t-1}$	−0.439**	−0.463**
	(0.203)	(0.210)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Individual controls	No	Yes
Mean dep.var	0.261	0.262
Observations	3103	2896
Kleibergen-Paap rk LM statistic	3.21	3.22
p-value	0.523	0.521
Kleibergen-Paap rk Wald F statistic	5.54	5.56

* p<0.10, ** p<0.05, *** p<0.010. IV estimates. Standard errors are clustered at the municipal level. Column (1) provides results without additional covariates, while Column (2) incorporates the following individual-level controls: age, education level, gender, and income. The variable $\text{Enforcement}_{m,t-1}$ represents the logarithm of the average number of inspections per 100 firms in the year preceding the survey. For a detailed description of the variables, see Appendix OA.1.

enforcement significantly reduces the likelihood of respondents reporting instances of particularistic exchanges during elections in their municipalities, consistent with H1.

In summary, we observe a significant decline in support for clientelist parties in response to increased enforcement, accompanied by a substantial reduction in the likelihood of a clientelist party winning local elections (H4). These outcomes appear to result from a combination of demand- and supply-side effects: fewer clientelist challengers contest elections (H2), and clientelist parties secure a lower share of votes, even after accounting for the proportion of clientelist candidates in the race (H3). Furthermore, individuals in municipalities with higher enforcement are less likely to report instances of particularistic exchanges (H1), underscoring the broader role of enforcement in reducing the prevalence of clientelistic practices.

6.2 Enforcement and Post-Election Outcomes

A key implication of this analysis is that a genuine shift in the type of elected candidate—beyond a mere adjustment in electoral strategy—should result in meaningful changes in the behavior of incumbents and the policies they implement.

However, this outcome is not guaranteed for two main reasons. First, entrenched clientelistic interests and networks can limit the ability of programmatic mayors to implement substantial reforms, even after being elected. For instance, evidence from Mexico demonstrates that low-skilled bureaucracies often resist change, even under programmatic parties,

making the transition to a professionalized administrative model particularly challenging (De La O 2024). Moreover, candidates from ostensibly programmatic parties may have incentives to engage in non-programmatic actions once in office, undermining their party's intended agenda (Johannessen 2020). Second, reducing patronage typically requires significant bureaucratic turnover, which can temporarily disrupt the provision of public goods (Akhtari et al. 2022; Toral 2023). This disruption can hinder the immediate effectiveness of reforms aimed at curbing clientelism, thereby delaying the anticipated positive outcomes.

In this section, we evaluate whether reductions in elected officials' clientelism are associated with improvements in other key local outcomes. Specifically, we examine two critical variables: the composition of the bureaucracy and the quality of public goods provision. If our findings reveal meaningful improvements in both bureaucratic composition and public goods provision despite these potential obstacles, it would provide strong evidence of a substantive and practical distinction between clientelistic and programmatic parties. Moreover, such results would demonstrate that incumbents from programmatic parties, once elected, possess both the capacity and the willingness to reshape local bureaucracies and enhance public goods delivery, thereby contributing to broader advancements in local governance.

Bureaucratic Composition. Clientelistic governments are known to cultivate low-skilled bureaucracies that lack the administrative capacity necessary for sustained economic and human development (De La O 2024). By obstructing the development of professionalized bureaucracies with clear rules, career paths, and functional autonomy, clientelist parties often foster politicized cadres of officials (Brollo et al. 2017; Brierley 2021; Peterlevitz 2022; Bardhan 2022). Patronage—a hallmark of clientelism—enables these governments to reward political loyalty through discretionary appointments, creating a system of ex-post clientelism that is credible and incentive-compatible (Robinson and Verdier 2013).

Given these dynamics, stricter enforcement of labor regulations, which increases the likelihood of programmatic candidates winning office, should lead to a reduction in patronage practices. Specifically, we would expect to observe a shift toward more educated bureaucracies and a decline in the use of temporary contracts. As described in Section 4, 85% of bureaucrats in Brazil are hired through merit-based public examinations, while the remaining 15% are appointed under temporary, non-civil servant contracts, which are often associated with patronage practices (Brollo et al. 2017; Colonnelli et al. 2020; Toral 2023; Ferrali et al. 2024). Moreover, political discretion in hiring has been shown to reduce the average competence of bureaucrats, typically assessed by their level of educational attainment (Colonnelli et al. 2020).

Our analysis, presented in Table 6, supports these expectations. While the overall proportion of new bureaucratic hires remains unchanged, stricter enforcement significantly alters

Table 6: Effects of Enforcement on the Composition of the Local Bureaucracy

	(1) New hires (% local bureaucracy)	(2) Temporary contracts (% of total hires)	(3) Graduate bureaucrats (% of total hires)
$\text{Enforcement}_{m,c(t)}$	−0.368	−0.166***	0.041
	(1.499)	(0.062)	(0.028)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep.var	0.392	0.225	0.276
Observations	30472	30256	30256
Kleibergen-Paap rk LM statistic	68.35	67.17	67.17
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	13.04	13.01	13.01
Hansen J statistic	4.79	72.33	7.04
p-value	0.309	0.000	0.134

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Bureaucrats' data is sourced from RAIS. We define temporary bureaucrats as individuals who do not hold a permanent contract, requiring candidates to pass competitive examinations for entry into the public sector. For more detail, see Section 5.1 and Appendix OA.1.

their composition..[13] Specifically, a 10% increase in enforcement reduces the share of temporary bureaucrat hires by 1.67 percentage points. Additionally, we observe a positive effect on the share of graduate bureaucrat hires, increasing by 0.4 percentage points, though this coefficient is less precisely estimated (p-value = 0.141). Despite these modest changes, the results are significant and suggest that elected candidates from programmatic parties prioritize merit-based hiring practices, fostering a more skilled and professionalized bureaucracy.

Public Goods Provision. Clientelist incumbents are also known to distort public goods provision, prioritizing private or "club" benefits over broad-access goods such as education and health (Diaz-Cayeros et al. 2016; Hicken 2011; Khemani 2015; Kitschelt and Wilkinson 2007; Stokes et al. 2013). This bias stems from the tendency of clientelistic parties to prioritize excludable goods and benefits that can be selectively allocated to reward loyal supporters (Bardhan and Mookherjee 2020). Additionally, the inability of clientelistic politicians to make credible pre-electoral commitments often leads to inefficient redistributive practices, such as offering bureaucratic jobs or discretionary rents (Keefer and Vlaicu 2008; Lindberg 2010; Robinson and Verdier 2013), further undermining the universal provision of public goods and services.

While expenditure data is often used to assess public goods provision, it can be misleading

[13]We calculate this variable by dividing the total number of new hires made in the first year of an electoral cycle by the total number of bureaucrats employed in the municipality during the previous year.

in certain contexts. Fergusson et al. (2023) show that in clientelistic settings, spending on education often fails to reliably reflect the actual quality or quantity of services provided, due to corrupt practices such as resource diversion. Furthermore, even at a given level of spending, programmatic mayors are more likely to closely monitor and efficiently manage municipal resources, ensuring that services are delivered as effectively as possible.[14] Given these considerations, rather than relying on spending data, we focus on educational and health indicators related to the infrastructure of schools and healthcare facilities.

Our educational input indicators include measures of municipal schools' access to physical facilities and equipment, such as internet connectivity, libraries, and science laboratories.[15] These indicators are constructed using data from the School Census, spanning the period from 1997 to 2020. Similarly, our healthcare input indicators measure the average number of hospital beds and medical equipment per health facility in each municipality. As with schools, we focus exclusively on health establishments managed by municipal governments. These data are sourced from the *Cadastro Nacional de Estabelecimentos de Saúde* (CNES), covering the period from 2005 to 2020. Our results, presented in Table 7, indicate that increased enforcement significantly enhances the quality of health and educational outcomes across municipalities. Specifically, it increases the average number of hospital beds and medical equipments per health establishment, as well as the share of schools equipped with key resources such as libraries, science labs, and internet access.

Together, the findings in this section indicate that, despite the challenges posed by entrenched clientelist networks and the short-term disruptions associated with the reduction of patronage, enhanced enforcement—by facilitating the election of more programmatic incumbents—leads to significant improvements in bureaucratic practices and public goods provision, thereby strengthening local governance.

6.3 Mechanisms

In this section, we explore the mechanisms through which enforcement capacity impacts clientelism. Specifically, we examine whether strengthening the bureaucratic resources of the Ministry of Labor reduces labor informality. Additionally, we assess the relationship between labor informality and the likelihood of voting for clientelistic parties—a link well-documented in the literature but yet to be examined within our context.

[14] Output measures, such as mean years of schooling, standardized test performance, infant mortality, and life expectancy, are less suitable because they reflect not only the mayor's efforts to improve services but also the baseline structural conditions of each municipality.

[15] We use the same set of educational outcomes employed by Brollo and Troiano (2016).

Table 7: Effects of Enforcement on Local Public Goods

	(1) Hospital beds per health establishment	(2) Medical equipments per health establishment	(3) % of schools with library	(4) % of schools with science lab	(5) % of schools with internet
$\text{Enforcement}_{m,c(t)}$	0.107***	0.047***	0.591***	0.020*	0.528***
	(0.042)	(0.014)	(0.080)	(0.011)	(0.082)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes	Yes
Mean dep.var	2.038	0.819	0.266	0.031	0.350
Observations	10100	21059	30901	30901	30900
Kleibergen-Paap rk LM statistic	146.63	186.93	75.54	75.54	75.54
p-value	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	37.50	41.85	13.88	13.88	13.89
Hansen J statistic	6.77	11.48	38.79	7.49	101.77

* p<0.10, ** p<0.05, *** p<0.010. IV estimates from Equation 3. Standard errors are clustered at the municipal level. All regressions include year and election fixed effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Health variables are sourced from the *Cadastro Nacional de Estabelecimentos de Saúde* (CNES) from the Health Ministry, and school outcomes from the school census data from *Instituto Nacional de Estudos e Pesquisas Educacionais* (Inep). Our analysis is limited to schools and health establishments that are managed at the municipal level.

Enforcement and Informality. The relationship between enforcement and informality is not straightforward. On one hand, stricter enforcement can incentivize firms to comply with labor regulations, thereby increasing formal employment. On the other hand, it can raise labor costs for firms, potentially pushing some workers into non-employment or self-employment.[16] To further investigate this relationship, we use two waves of Census data (2000 and 2010). To mitigate the limitations of these data—specifically their infrequency and reliance on self-reported information—we supplement our analysis with annual RAIS data, which offers comprehensive and detailed records covering the entire universe of formal workers in the country.[17]

Our results, presented in Table 8, demonstrate that stronger enforcement significantly increases the proportion of formal workers in the workforce while concurrently reducing the shares of informal workers and the self-employed. Additionally, we observe a decline in

[16] As in Almeida and Carneiro (2012), we focus on non-employment rather than unemployment due to the lack of survey questions that reliably distinguish between individuals who are not working and those actively seeking employment.

[17] A similar analysis was conducted in the seminal work of Almeida and Carneiro (2012), but our approach differs in several important ways. First, we incorporate additional data from the 2010 Demographic Census and micro-data from RAIS. Second, while the original study relied on a cross-sectional design with state fixed effects, we exploit temporal variation from the opening of new labor offices and include municipality fixed effects to better control for time-invariant local-level heterogeneity. Third, unlike the original study's reduced-form approach, we employ an instrumental variable (IV) strategy to address potential endogeneity concerns. For further details, see Appendix OA.3.

Table 8: Effects of Enforcement on Local Labor Outcomes

	(1) Share of formal workers (Census)	(2) Share of informal workers (Census)	(3) Share of self-employed workers (Census)	(4) Share of non-workers (Census)	(5) Formal workers (log) (RAIS)
$\text{Enforcement}_{m,t-1}$	0.069***	−0.045**	−0.092***	−0.117**	0.637***
	(0.025)	(0.020)	(0.030)	(0.048)	(0.085)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Mean dep.var	0.201	0.174	0.180	0.393	6.130
Observations	10672	10672	10672	10672	137706
Kleibergen-Paap rk LM statistic	19.41	19.41	19.41	19.41	245.53
p-value	0.002	0.002	0.002	0.002	0.000
Kleibergen-Paap rk Wald F statistic	4.37	4.37	4.37	4.37	50.19
Hansen J statistic	45.10	36.03	1.50	18.56	45.40
p-value	0.000	0.000	0.827	0.001	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,t-1}$ refers to the log of the mean number of inspections per 100 firms in the previous year. In Columns (1) through (4), the variables are expressed as shares of the total economically active population. The specification in Column (5) relies on yearly data, and its dependent variable—derived from RAIS—is log-transformed. For a detailed description of all variables, see the Appendix OA.1.

the share of non-employed individuals.[18] The specification in Column (5), based on yearly micro-data, further supports these findings by demonstrating that stronger enforcement promotes formalization—in line with prior studies in the literature (de Andrade et al. 2016; Brotherhood et al. 2023).[19] Taken together, these findings provide compelling evidence that enhanced enforcement reduces informality while fostering growth in formal employment.

Informality and Clientelism. A key question that remains unaddressed in our analysis is whether transitioning from informal to formal employment directly influences voter behavior. While the absence of individual-level panel data prevents us from tracking such changes specifically in response to variations in enforcement, we can still provide suggestive evidence that informal workers are, in general, disproportionately more likely to support clientelistic parties compared to their formally employed counterparts.

Addressing this question is inherently challenging due to the limited availability of surveys that simultaneously capture labor informality and voting decisions at the municipal level. Although existing research has documented a strong correlation between labor status and

[18]This aligns with existing research showing that stricter labor regulation enforcement does not necessarily increase unemployment rates (Meghir et al. 2015; Ulyssea 2020; Haanwinckel and Soares 2021; Dix-Carneiro et al. 2021).

[19]Unlike the models using Census data, the dependent variable in Column (5) is not expressed as a share due to the absence of annual data on the economically active population. However, as demonstrated in Table A.5, the results for remain robust when local population size is included as a control variable or when we use the share of formal workers relative to the total population as the dependent variable.

Table 9: Labor Informality and the Likelihood of Voting for Clientelistic Candidate in Local Elections

	(1) Clientelism Score of voted party (DALP)	(2) Clientelism Score of voted party (V-Party)	(3) Clientelism Score of voted party (GPS)	(4) Salience of clientelism of voted party (V-Party)
Informal Worker	0.319***	0.277***	0.168*	0.199**
	(0.107)	(0.098)	(0.094)	(0.094)
Observations	829	829	829	829
Individual-level controls	Yes	Yes	Yes	Yes
R-squared	0.041	0.033	0.029	0.022

* p<0.10, ** p<0.05, *** p<0.010. Standard errors are clustered at the municipal level. All regressions include age, education level, gender, income, and occupation type as controls. The dependent variable is the continuous clientelism score of the party for which the individual reported voting in the local election. For a detailed description of the variables, see Appendix OA.1.

ideological preferences in Latin America (for a meta-analysis, see Baker and Dorr 2022), this relationship tells us little about informality's connection to clientelistic support. This is because ideological orientation (e.g., left or right) does not map cleanly onto the degree of party clientelism, as demonstrated in Table OA.1 and further discussed in Section 8.

To examine the empirical relationship between informal labor status and support for clientelistic parties, we use the Brazilian Electoral Panel Study (BEPS) survey, which offers two key advantages. First, it employs the "benefits" definition of informality, classifying workers who contribute to social security via payroll taxes as formal, while all others are considered informal (Gasparini and Tornarolli 2009).[20] Second, unlike most surveys that focus primarily on presidential elections, the BEPS includes questions about respondents' choices in the most recent local elections. This feature allows us to analyze party preferences in greater detail and link them to clientelism scores derived from expert surveys. Our findings, presented in Table 9, confirm that informal workers are significantly more likely to support candidates from clientelistic parties, substantiating the proposed link between labor informality and voting behavior. These findings corroborate recent research indicating that vulnerability heightens individuals' reliance on clientelistic relationships as a form of insurance (Bobonis et al. 2022). Additionally, they support the notion that labor informality diminishes demands for programmatic governance and disrupts ideological ties between voters and political parties (Altamirano 2015; Rains and Wibbels 2023; Gottlieb 2024).

[20]The benefits definition is widely regarded as the most accurate. The productive definition, which treats self-employment as informality, is considered "theoretically weak"(Baker and Dorr 2022).

7 Threats to Identification

Although our enforcement capacity measure is predetermined and our analysis includes municipality and year fixed effects, the non-random placement of labor offices remains a potential concern. As noted, the Ministry of Labor allocates labor offices based on specific criteria, such as minimum population thresholds and local labor market size (see Appendix OA.2). To mitigate this issue, our previous analyses exclude municipalities that host labor offices, assuming that any change in proximity to these offices is exogenous for neighboring municipalities. While neighboring municipalities may resemble host localities in certain respects, it is reasonable to assume that no single neighboring municipality influences the timing of office openings. To further investigate potential biases arising from the non-random placement of labor offices, this section employs an event-study specification. We also show the robustness of our results to the inclusion of time-varying municipal controls, and state-by-year fixed effects, which help minimize the influence of potential unobserved confounders.

Event-study. The event-study design offers two key advantages. First, it allows us to examine the plausibility of the parallel trends assumption by confirming that treated and control municipalities followed similar trajectories before the intervention. Second, it enables us to investigate the dynamic effects of labor office openings, providing insights into whether these effects persist, increase, or diminish over time. Together, these advantages offer complementary evidence to the IV design regarding the timing and duration of the intervention's influence.

It is important to note, however, that this approach is not designed to estimate the causal effect of enforcement intensity, which is the focus of our main empirical strategy. Rather, the purpose of the event study is to verify that new labor offices open in ways unrelated to pre-treatment trends in the main outcome (e.g., clientelism) or in our proposed mechanism (e.g., labor formalization). If such alignment existed, it would raise legitimate concerns about selection bias. Therefore, unlike the IV design, the event study does not capture the full effect of enforcement intensity—which depends on both the distance to labor offices and the number of inspectors per office. Instead, this model controls for the number of inspectors in each state and for the number of pre-existing offices.

In this analysis, we define the treatment as the establishment of a new labor office within a specified distance from each municipality, considering four radius thresholds: 50 km, 60 km, 70 km, and 80 km (the median distance for municipalities with at least one inspection). To ensure a more accurate comparison, we restrict the sample to municipalities that eventually experience the opening of a labor office within the chosen distance. Consequently, the control group comprises only those municipalities where an office is slated to open at a later point

in time, within the same radius. This design allows us to more precisely isolate the causal impact of labor office openings. Specifically, we estimate the following Equation (4):

$$y_{m,t} = \sum_{k \in \{-x,\ldots,z\} \setminus \{-1\}} \beta_k t^k_{m,t} + \alpha_{m,t} \mathbf{X_{m,t}} + \mu_m + \lambda_t + \epsilon_{jt} \tag{4}$$

with $t^k_{m,t}$ a binary variable that equals 1 if period t corresponds to k years after the opening of an office within the specified radius in municipality m and 0 otherwise. As such, parameter β_0 captures the instantaneous effect of an office opening, while β_1 measures the impact one year post-inauguration, and so on. We also include periods before the opening (i.e., $k < 0$) to check for potential violations of the parallel trends assumption. The vector $\mathbf{X_{m,t}}$ includes the number of inspectors and the number of offices within each state. We estimate equation (4) using De Chaisemartin and d'Haultfoeuille's (2024) approach, which accommodates the staggered nature of the treatment and allows for the inclusion of time-varying controls.

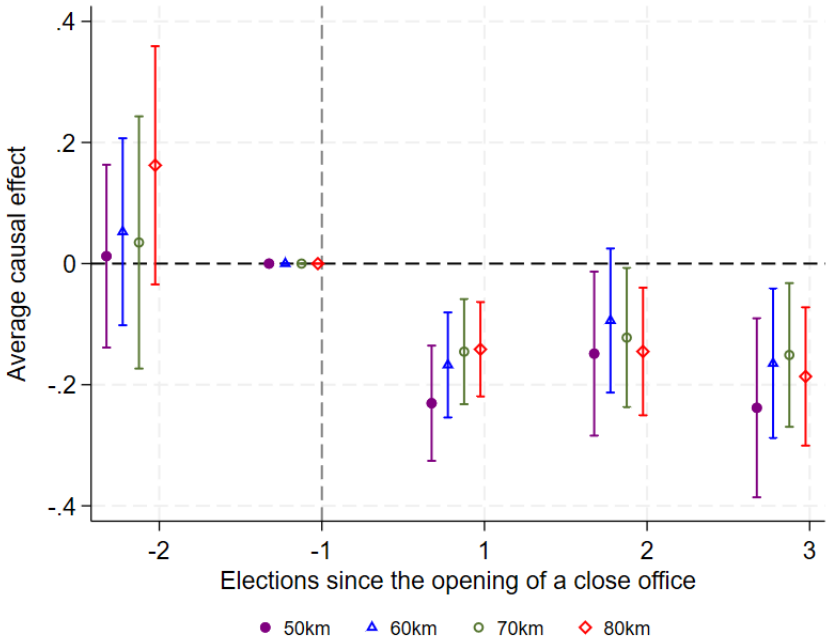
The event study results offer further evidence that the opening of labor offices directly contributes to at least part of the observed differences in post-treatment outcomes. Panel A of Figure 4 shows that establishing a new labor office leads to a marked decrease in the winning party's clientelism score—an effect that persists for at least three election cycles, indicating both a substantial and long-lasting reduction in clientelism. Moreover, Figure A1 demonstrates a similar pattern in the probability that a clientelistic party wins. Panel B extends the analysis to the formal labor force, indicating an increase in formal employment for at least two election cycles following the establishment of a labor office. Crucially, we find no evidence of divergent trends between treated and control non-host municipalities prior to the opening of a new labor office, indicating that the parallel trends assumption holds within this group. This result suggests that new offices are not systematically introduced in areas already experiencing changes in labor formalization or clientelism, thereby mitigating concerns about selection bias.

Results with time-varying controls. We also present two specifications incorporating controls for municipal demographic and economic characteristics that may influence labor market outcomes. Panel A of Table A.6 includes lagged values of local GDP, population, and state-by-year fixed effects, capturing differential state-level trends over time. Panel B, on the other hand, controls for baseline 1991 levels of urbanization, high-school-educated workers, and the female population, each interacted with time trends, following the approach of Ponczek and Ulyssea (2022). Although data limitations lead to a reduction in the number of observations, the results remain robust and, if anything, exhibit even stronger magnitudes.

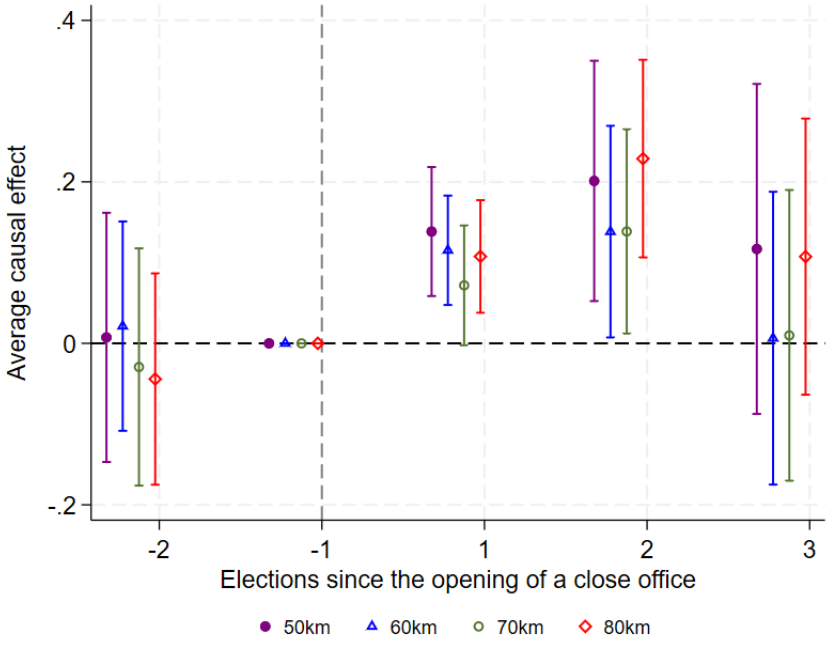
Local Political Manipulation. Another concern is the potential influence of mayors

Figure 4: Event study

Panel A: Winning Party's Clientelism Score



Panel B: Number of Formal Workers



Estimates of Equation (4) controlling for the number of inspectors and the number of pre-exisiting offices within each state. Confidence intervals are at the 90% level. The sample is restricted to municipalities that experienced the opening of an office within a 50, 60, 70, or 80 km radius. Panel A presents the results for the winning party's clientelism score, as defined by the DALP database, while Panel B focuses on the average (logged) number of formal workers per electoral cycle. This variable is sourced from RAIS. For a detailed description of the variables, refer to Appendix OA.1.

affiliated with clientelistic parties on the establishment of new labor offices within or near their municipalities. With host municipalities excluded from the analysis, the remaining concern is whether individual mayors might significantly affect the placement of offices in neighboring areas. Although this scenario is unlikely, we investigate whether changes in distances to labor offices or the allocation of inspectors at the state level between elections $t-1$ and t are systematically linked to the level of clientelism exhibited by the incumbent mayor during that term. The results in Table A.7 show no significant relationship between changes in the distance to the nearest labor office and the clientelistic orientation of the incumbent mayor's party. Similarly, as shown in Table A.8, there is no evidence of a correlation between the number of inspectors per office and the proportion of clientelistic incumbents within each state.

Taken together, these findings reinforce the notion that centrally driven investments in administrative capacity, such as the establishment of labor offices and allocation of inspectors, are largely insulated from manipulation by local political actors.

8 Robustness Checks and Alternative Interpretations

8.1 Validating the Measure of Party Clientelism

In this section, we address potential limitations of our measure of party clientelism, including concerns related to missing data and the influence of parties' organizational structures. Additionally, we conduct further tests to validate the robustness of our findings across various related outcomes. These efforts reinforce the evidence that our results capture a genuine reduction in clientelism and a broader shift toward programmatic governance.

Alternative Dependent Variables. In our baseline analysis, we rely on binary clientelism scores derived from expert surveys. To verify the robustness of our findings, we also use continuous clientelism scores in alternative specifications. Next, we examine whether increased enforcement influences the probability that the incumbent party both participates in the election and secures re-election. We then extend the analysis to include city council election outcomes and incorporate the clientelism scores of candidate coalitions, rather than focusing solely on individual parties.

First, in Table A.9, we replace the binary classification with continuous clientelism scores from DALP, V-Party, and GPS. The results remain consistent, confirming the robustness of our findings.[21] Second, Table A.10 examines the impact of enforcement on the likelihood

[21] Definitions of these variables are provided in Appendix OA.1. Similar to the binary measures, missing values are imputed using the minimum score of the variable.

Table 10: Effects of Enforcement on Coalition-Level Clientelism

	(1) Average clientelism score of winning coalition (DALP)	(2) Average clientelism score of winning coalition (V-Party)	(3) Average clientelism score of winning coalition (GPS)	(4) Salience of clientelism of winning coalition (V-Party)
$\text{Enforcement}_{m,c(t)}$	−0.391***	−0.356***	−0.300***	−0.223***
	(0.075)	(0.069)	(0.070)	(0.036)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.617	0.379	0.523	0.271
Observations	36441	36441	36441	36441
Kleibergen-Paap rk LM statistic	92.68	92.68	92.68	92.68
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.17	16.17	16.17	16.17
Hansen J statistic	19.43	21.38	10.16	24.07
p-value	0.001	0.000	0.038	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. The dependent variables are the share of clientelistic parties of the candidate's coalition. For a detailed description of the variables, see Appendix OA.1.

of the incumbent party contesting again and securing re-election. Column (1) shows that enforcement does not affect the likelihood of the incumbent party contesting the election; however, Column (2) reveals that enforcement significantly reduces their chances of winning. Notably, this reduction in winning probability is significant only for clientelistic parties, as demonstrated by the combined coefficients ($\beta_1 + \beta_2$) reported in Column (4).

Prior research indicates that council candidates often act as brokers for their mayoral counterparts (Novaes 2018). Consequently, if enforcement weakens the effectiveness of clientelism, we would anticipate a similar decline in electoral support for council candidates from clientelist parties. The results presented in Table A.11 align with this expectation: increased enforcement significantly reduces the vote share of clientelist parties in council elections. This finding underscores the broader impact of enforcement on dismantling clientelistic networks, extending beyond mayoral contests to affect local governance more broadly.

Next, we consider the role of coalitions. Although candidates running for public office are required to affiliate with a single political party, coalitions are permitted and commonly utilized in mayoral elections. Approximately 75% of mayoral candidates are part of a political coalition, which is typically formed at the municipal level shortly before each election. To reflect these alliances, we construct an alternative dependent variable by averaging the binary clientelism scores of all parties in each candidate's coalition, rather than focusing solely on the candidate's own party. For instance, if two out of five coalition parties are classified as clientelist, the coalition's clientelism score is 0.4. As shown in Table 10, our results remain robust under this specification.

Finally, we assess whether our main findings could be attributed to a decrease in voter

Table 11: Effects of Enforcement on Turnout

	(1) Turnout	(2) Probability of a clientelistic party winning (DALP)	(3) Probability of a clientelistic party winning (V-Party)	(4) Probability of a clientelistic party winning (GPS)	(5) Salience of clientelism (V-Party)
Enforcement$_{m,c(t)}$	5.922***	−0.413***	−0.256***	−0.153**	−0.220***
	(0.895)	(0.091)	(0.080)	(0.078)	(0.041)
Turnout		0.005***	0.003**	0.001	0.003***
		(0.001)	(0.001)	(0.001)	(0.001)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes	Yes
Mean dep.var	75.360	0.591	0.524	0.422	0.316
Observations	36413	36413	36413	36413	36413
Kleibergen-Paap rk LM statistic	107.70	97.48	97.48	97.48	97.48
p-value	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	20.86	18.84	18.84	18.84	18.84
Hansen J statistic	9.45	13.06	25.49	11.59	19.10
p-value	0.051	0.011	0.000	0.021	0.001

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. Enforcement$_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before the election at time t. For a detailed description of the variables, see Appendix OA.1.

turnout rather than a genuine shift in voter preferences (Table 11). One possibility is that increased enforcement discourages supporters of clientelist parties from participating in elections, rather than inducing them to switch their votes. If this were true, we would expect a negative relationship between enforcement and voter turnout. However, our results show the opposite: turnout increases, consistent with a decline in "abstention-buying"—a practice whereby indifferent or opposing voters are compensated for not casting a ballot (e.g., Gans-Morse et al. 2014). To rule out the possibility that shifts in turnout drive our findings, we re-estimate our baseline models including turnout as a control (Columns 2–5 of Table 11). The negative effect of enforcement on clientelism remains both substantial and statistically significant.

Addressing Missing Clientelism Scores. As detailed in Appendix OA.1, the expert surveys used to classify parties by their level of clientelism cover approximately 92% of mayoral candidates.[22] In our baseline analysis, we follow Frey (2019) by coding parties with missing scores as non-clientelist. To verify that our results are robust to alternative classifications of these smaller parties, we perform additional tests. As shown in Table A.12, the negative effect of labor law enforcement on the probability that clientelist parties win remains significant when we exclude these parties entirely (Panel A) or recode them as clientelist (Panel B). These findings confirm that our conclusions do not hinge on how

[22]Refer to Table OA.1 for a complete list of parties and their respective clientelism scores, including those with missing values.

missing clientelism scores are handled.

Alternative Definitions of Programmatic Party. Clientelist parties rely on transactional relationships with voters, circumventing ideological appeals and partisan loyalty. This reliance weakens ideological linkages (Ruth 2016; Kitschelt et al. 2010), reducing the salience of policy as a key driver of voter behavior. In contrast, programmatic parties develop coherent platforms, align member interests around policy goals, and provide public goods non-selectively. If our hypothesis is correct, stronger enforcement should curb clientelistic practices and, in turn, improve electoral outcomes for programmatic parties. To further validate this proposition, we explore several alternative definitions of programmatic parties. First, we investigate whether increased enforcement shifts electoral support toward parties that emphasize policy-driven platforms, particularly those focused on economic issues.[23] As shown in Column (1) of Table 12, while the result is not statistically significant at conventional levels, increased inspections are associated with greater support for this subset of programmatic parties.[24]

Another potential complication is that even within programmatic parties, individual candidates may differ in their reliance on clientelistic practices. Recent research emphasizes that understanding party strategies requires looking beyond voter-party linkages to also consider party organization and resource structures (Aspinall et al. 2022; Berenschot and Aspinall 2022; Hicken and Nathan 2020). While our primary measures capture a party's overall reliance on clientelism, they do not directly account for differences in three critical dimensions: (1) the degree of party centralization, (2) organizational capacity, and (3) sources of funding.

The degree of party centralization is a key factor influencing candidates' discretion in employing clientelistic tactics (Sells 2020). Decentralized parties, often composed of loose networks of local elites, grant subnational actors significant autonomy to adopt clientelistic strategies (Mainwaring 1999). In contrast, centralized parties with strong national leadership are better equipped to enforce programmatic discipline (Morgenstern 2003). To measure centralization, we follow Sells (2020) and examine the distribution of bargaining power across different levels of governance in two key areas: (i) candidate selection and (ii) electoral strategy decisions. In more centralized parties, national leaders dominate these processes, limiting the influence of local and regional actors.

A party's organizational capacity also plays a critical role. Strong local party organizations reduce individual candidates' autonomy and facilitate the enforcement of centralized discipline. Conversely, parties with weaker infrastructures rely more heavily on personal, ad hoc networks developed by candidates, allowing greater independence in local campaign

[23]See Appendix OA.1 for the precise definition of this variable.

[24]The p-value for the estimated coefficient is 0.105.

strategies. To assess organizational strength, we examine whether parties (i) maintain permanent municipal-level offices and (ii) deploy staff who are continuously active in local communities.

Funding is another crucial dimension. Parties lacking a strong brand may depend heavily on individual candidates to finance their own campaigns (Yıldırım and Kitschelt 2022; Aspinall et al. 2022). This dependence grants candidates more freedom to raise funds and deploy resources with minimal oversight from party leaders. We capture this aspect using a variable that indicates whether candidates themselves provide the primary source of campaign funding. When candidates control financial resources, they can independently mobilize and monitor voters or reward supporters, effectively replicating many functions traditionally performed by parties (Hale 2005). These "party substitutes" allow politicians to pursue candidate-centered strategies, circumventing party leadership constraints and potentially diverging from the party's programmatic goals.

To explore how these dimensions might affect our results, we incorporate two additional items from the DALP database (Kitschelt et al. 2013) and three items from the V-Party dataset (Lindberg et al. 2022).[25] Specifically, we refine our classification of "programmatic" parties by introducing three alternative definitions. Parties are classified as programmatic if they meet one or more of the following criteria: (1) they are non-clientelistic and centralized, (2) non-clientelistic and organizationally strong, or (3) non-clientelistic and party-centered. To enhance comparability, we normalize the five variables discussed earlier to a range of 0 to 1, where 1 represents the highest degree of centralization, organizational strength, or reliance on party funding. Each normalized score is then weighted by a binary variable indicating whether the party is programmatic.[26] Consequently, parties that are clientelistic or lack other relevant traits—such as centralization—receive a score of zero. This approach enables us to capture nuanced variations among programmatic parties while systematically excluding those that do not meet the required criteria.

The results are presented in Columns (2) through (6) of Table 12, each corresponding to a different definition of programmatic parties. In Column (2), programmatic parties are defined as non-clientelistic parties where the candidate selection process is primarily controlled by national party leaders rather than local actors. Column (3) focuses on programmatic parties that formulate their electoral strategies with minimal input from local-level actors. Column (4) defines programmatic parties as those that are non-clientelistic and exhibit high levels of permanent local party activism. In Column (5), programmatic parties are identi-

[25]For detailed descriptions of the corresponding survey questions, see Appendix OA.1.

[26]Programmatic parties are defined as non-clientelistic, based on our binary measure derived from the DALP dataset.

Table 12: Effects of Enforcement on Programmatic Party Support: Alternative Measures

	(1) Salience of economic issues	(2) Programmatic and centralized candidates' selection	(3) Programmatic and centralized electoral strategy	(4) Programmatic and permanent local activists & personnel	(5) Programmatic and high presence of local offices	(6) Programmatic and low reliance on candidates' funds
$\text{Enforcement}_{m,c(t)}$	0.022	0.141**	0.111**	0.125***	0.035	0.065**
	(0.013)	(0.066)	(0.050)	(0.047)	(0.045)	(0.033)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var	2.052	0.274	0.105	0.212	0.223	0.157
Observations	24370	31604	31604	30748	30748	30748
Kleibergen-Paap rk LM statistic	59.25	84.98	84.98	90.64	90.64	90.64
p-value	0.000	0.000	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	10.25	14.81	14.81	17.97	17.97	17.97
Hansen J statistic	5.06	24.99	26.49	14.82	9.22	1.31
p-value	0.282	0.000	0.000	0.005	0.056	0.859

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Dependent variables refer to the winning party in municipal elections. For a detailed description of the variables, see Appendix OA.1.

fied as non-clientelistic parties that maintain a relatively high number of municipal offices. Finally, in Column (6), programmatic parties are characterized as non-clientelistic and relying minimally on candidates' self-financing. Across all these definitions, increased labor law enforcement meaningfully increases the likelihood of electing candidates from programmatic parties, with the exception of Column (5), where the results do not reach statistical significance at conventional levels.

Implications of Party-Switching. One might worry that frequent party switching undermines our results. Specifically, it could be argued that increased enforcement boosts the electoral success of programmatic parties simply because clientelistic politicians defect to them, thereby inflating their support. However, there are several reasons why this concern does not seriously challenge our findings. First, politicians who do switch parties generally move between similarly clientelistic organizations. Because clientelistic parties lack clear ideological positions, they are largely interchangeable, allowing local politicians to switch among them at minimal cost. In contrast, programmatic or ideological parties, by their very nature, are more distinct from rivals, and the exit costs for politicians affiliated with them are correspondingly higher (Sells 2020). These parties do more than simply offer campaign resources—they cultivate cohesive policy platforms, foster grassroots organization, and maintain a valuable brand that signals policy preferences to voters (Grynaviski 2010; Snyder Jr and Ting 2002). Consequently, the likelihood of clientelistic politicians suddenly migrating to programmatic parties, solely in response to heightened enforcement, remains low. In Brazil, some parties—most notably the Workers' Party (PT)—have successfully cultivated strong partisan followings by employing these strategies (Ames 2005; Mainwaring 1999; Samuels 2006). Politicians in such parties are less likely to defect, as their voter base tends to remain loyal to the party itself rather than to individual candidates (Desposato

2006).

Second, even if clientelist politicians switch to programmatic parties, such a transition itself demonstrates shifting electoral incentives—namely, the growing appeal of programmatic labels and the diminishing effectiveness of clientelistic strategies under heightened enforcement. A potential concern, however, is that these politicians might continue to employ clientelist tactics under their new party banner, thus undermining our measure. Our analysis directly addresses this issue. As discussed in the previous subsection, we obtain consistent results when employing a stricter definition of programmatic parties—one that includes only those featuring centralized organizations and limited autonomy for local candidates (Table 12). In other words, even if politicians with clientelist backgrounds join these parties, the parties' structural constraints and ideological discipline make it difficult for them to persist with particularistic strategies. Moreover, our findings show notable reductions in vote-buying and patronage, alongside increases in public goods provision. Taken together, these results imply that when heightened enforcement drives politicians from clientelist parties to adopt programmatic labels, they either fail to secure office under the new party's banner or adapt their strategies significantly to align with the party's programmatic norms.

8.2 Alternative Interpretations

In this section, we address several potential confounders that could influence the interpretation of our findings. These include state-specific dynamics, the effects of presidential and gubernatorial coattails, the distinction between clientelism and personalism, and the potential overlap of clientelism with ideological shifts or populism.

State-Level Dynamics. A potential concern is that clientelistic practices vary significantly across states, influenced more by state-specific dynamics than by party labels. For instance, some parties, such as the PTB and PT, may employ consistent linkage mechanisms nationwide, while others might allow for more local autonomy, perhaps even mimicking the linkage mechanisms of other parties in each state, instead of adhering to a uniform national strategy (see, e.g., Epstein 2009). To account for this, we incorporated state-by-year fixed effects into our baseline regressions. As shown in Table A.6, our results remain robust, indicating that the observed patterns are not driven by state-level dynamics but reflect broader distinctions between programmatic and clientelist strategies.

Presidential and Gubernatorial Coattails. Another potential concern is that changes in state or national leadership might mechanically influence local electoral outcomes. For example, the election of a programmatic governor could result in the establishment of more labor offices within a state, which might coincide with an increased likelihood of local

candidates from the governor's party being elected. In such cases, the observed reduction in support for clientelist parties could be attributed to coattail effects rather than the direct impact of enforcement.

To test whether our results are influenced by these coattail effects, we conduct additional analyses excluding candidates from the same party as the governor and co-partisans of the president from our sample. As shown in Tables A.13 and A.14, the results remain virtually unchanged, providing robust evidence that this alternative explanation does not account for our findings.

The Role of the PT. Another potential concern involves the influence of strong political parties, particularly the *Partido dos Trabalhadores* (PT). Prior research by Feierherd (2020) shows that PT-affiliated mayors are more likely to delay labor law enforcement and improve informal working conditions. Although our empirical strategy mitigates endogeneity concerns by leveraging exogenous variation in enforcement intensity—thereby limiting the potential for mayors to manipulate inspection frequencies—and by excluding municipalities hosting labor offices, it remains crucial to confirm that our findings are not influenced by PT-specific factors. To address this concern, we first conduct a robustness test that removes from our sample any municipality governed by a PT-affiliated mayor during the period when inspections took place. As shown in Table A.15, Panel A, our main results remain largely unchanged, suggesting that PT-related dynamics or mayoral manipulation of enforcement in the electoral cycle preceding the election of interest do not drive our findings.

Another potential concern is that labor offices are strategically located and inspectors assigned to municipalities where the PT aims to expand its influence. To address this, in Table A.15, Panel B, we exclude municipalities where the PT won elections after inspections took place. As the results show, the negative effect of enforcement on clientelism remains, suggesting that strategic expansion efforts by the PT do not drive our findings.

The Role of Ideology. Another possibility is that increased enforcement might shift voter preferences along the left-right ideological spectrum rather than reducing clientelism. However, our analysis using ideological scores from the DALP and V-Party datasets (Table 13, Columns 1-2) shows no significant effect of enforcement on the ideological leaning of winning parties. This result is consistent with the observation that in Brazil, parties with similar positions on the left-right spectrum often display markedly different levels of clientelism. As shown in Table OA.1, the PDT and PSB, both left-wing parties with comparable ideological scores (3.5 and 3.1, respectively), are classified differently in terms of clientelism: PDT is identified as clientelistic, while PSB is not. Similarly, among right-wing parties, the PTB and PSDB share similar ideological scores (6 and 6.1, respectively) but differ in clientelism classifications, with PTB classified as clientelistic and PSDB as non-clientelistic.

Table 13: Effects of Enforcement on Alternative Interpretations of Clientelism

	(1) Left-right scale (DALP)	(2) Left-right scale (V-party)	(3) Level of personalism	(4) Populism
$\text{Enforcement}_{m,c(t)}$	0.045	0.265	−0.011	−0.023
	(0.302)	(0.171)	(0.021)	(0.025)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	6.017	3.748	1.090	0.263
Observations	31856	30748	31856	30748
Kleibergen-Paap rk LM statistic	84.43	90.64	84.43	90.64
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	14.63	17.97	14.63	17.97
Hansen J statistic	30.41	18.33	6.83	6.03
p-value	0.000	0.001	0.145	0.197

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Dependent variables refer to the winning party in municipal elections. For a detailed description of the variables, see Appendix OA.1.

These findings suggest that clientelism operates independently of ideological alignment.

Differentiating Clientelism and Personalism. A related concern is whether our measure of clientelism inadvertently conflates it with personalism—a strategy in which candidates rely on charisma or personal social networks to win elections. Although clientelism and personalism often coexist, they are fundamentally distinct. Clientelist linkages involve conditional exchanges of selective benefits in return for electoral support, establishing reciprocal relationships between parties and voters. In contrast, personalist linkages are based on individual attributes, such as a candidate's charisma or popularity, which drive voter support independently of material incentives (Stokes 2005; Kitschelt 2000). To disentangle the effects of labor law enforcement on these two phenomena, we examined the electoral success of personalist candidates as a separate outcome. Using a measure of personalism derived from DALP (see Appendix OA.1), we found no significant relationship between enforcement and support for candidates from personalist parties (Table 13, Column 3). This finding is consistent with the literature, which emphasizes the conceptual separation between clientelism and personalism as distinct voter-politician linkage strategies (e.g., Sells 2020; Lyne 2005; Kitschelt 2000). Moreover, it aligns with the idea that reduced voter vulnerability, which may weaken dependence on clientelist networks as a form of economic insurance, does not necessarily affect support for candidates who rely on charisma or personal appeal.

Differentiating Clientelism and Populism. Finally, we analyze whether our measure inadvertently captures a decline in populist appeals rather than clientelism. Using a

populism score derived from the V-party database (see Appendix OA.1), we found no consistent relationship between enforcement and the level of populism in elected parties (Table 13, Column 4). This supports the view that clientelism and populism are also distinct phenomena.

8.3 First-Stage Robustness

Dieterle and Snell (2016) emphasize the importance of testing the robustness of first stages when employing an instrumental variable approach. In line with their recommendation, our baseline analysis incorporate the squared terms of the instrument. Table A.16 demonstrates the robustness of our main results when using a simpler linear specification in the first stage. Our results are stronger in magnitude, as distance and inspectors have non-linear effects as previously discussed.

In our baseline analysis of electoral data, we calculate the average number of inspections (and the corresponding instruments) over the entire previous electoral cycle. In contrast, for other dependent variables, such as labor market outcomes and individual survey reports of vote-buying, we measure the independent variable and instruments in the year immediately preceding the year in which the outcome variable is observed. When this alternative measurement approach—using enforcement and its instruments from the year before the election—is applied to electoral outcomes, the strength of our findings increases, as shown in Table A.17.

8.4 Alternative Measures of Distance

In this section, we assess the robustness of our findings by exploring various methods for measuring distances. Our initial results rely on the estimated car travel distance between each municipality and the nearest labor office in 2024. To account for potential changes in road conditions from 1996 to the present, we use road map data from the years 2000, 2005, 2010, and 2017 (obtained from de Castro Victoria et al. 2021), and calculate the distances between municipal centroids and labor offices using the most accurate road map available for each period. Our results remain consistent: alternative distance measurements continue to support our initial findings, as shown in Table 14. The robustness of our findings is further confirmed by the use of Euclidean distances, as shown in Table A.18.

In an alternative specification, we refine our analysis by excluding states where maritime transportation is the primary mode of travel for goods and passengers, rendering car travel distances largely irrelevant. Specifically, we exclude the states of Amazonas and Pará, following Almeida and Carneiro (2012). Our results remain robust, as demonstrated in Table

Table 14: Effects of Enforcement on Clientelism Using Historical Maps

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-dem)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (GPS)
$\text{Enforcement}_{m,c(t)}$	−0.399***	−0.284***	−0.232***	−0.208***
	(0.074)	(0.070)	(0.067)	(0.035)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36496	36496	36496	36496
Kleibergen-Paap rk LM statistic	100.67	100.67	100.67	100.67
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	33.12	33.12	33.12	33.12
Anderson-Rubin ξ^2 test (robust inference)	53.91	33.90	26.25	64.36
p-value	0.000	0.000	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. Estimates of equation (3). In the first stage, the distance to the nearest labor office is calculated using the most recent road map available among the years 2000, 2005, 2010, and 2017 (obtained from de Castro Victoria et al. 2021). $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

A.19.

9 Final remarks

Many developing countries are mired in a weak state trap, where low state capacity and entrenched clientelistic practices reinforce each other, leading to poor accountability, reduced political competition, and a focus on delivering particularistic benefits at the expense of public goods (Fergusson et al. 2022a). Previous research has elucidated the mechanisms through which clientelism hampers the expansion of state capacity. For instance, De La O (2024) demonstrates that local clientelistic incumbents, even when endowed with increased public funds, allocate resources in ways that primarily benefit their own parties. Additionally, when faced with political competition, these incumbents actively obstruct investments in state capacity to maintain their electoral advantage (Fergusson et al. 2022b). However, the inverse relationship—the effect of investments in state capacity on clientelism—remains less explored.

This paper addresses this question by focusing on centrally-driven enhancements of the state's administrative apparatus, a type of state capacity investment that is less susceptible to local capture. Leveraging geographic and temporal variation in the enforcement of labor regulations across Brazil, and employing an instrumental variable approach, we provide strong evidence that improved state capabilities significantly diminish the success of clientelistic parties. Additionally, our findings indicate significant reductions in vote-buying, a decrease in the hiring of politically appointed bureaucrats, and an improvement in local public goods provision.

Our analysis proposes that changes in labor market structure are a key mechanism by which state interventions can influence local politics. We demonstrate that municipalities with enhanced enforcement capacity experience notable increases in formal employment and significant reductions in informality. Moreover, individual-level data from the Brazilian Electoral Panel Study show that informal workers are more likely to support clientelistic candidates, in line with research underscoring how economic vulnerability critically shapes political behavior (Bobonis et al. 2022; Rains and Wibbels 2023; Gottlieb 2024). These findings illustrate the effectiveness of centrally-driven state interventions in fostering more programmatic party systems, cultivating more responsive political environments, and enhancing governance quality.

The long-term effects of increased enforcement, however, remain uncertain. Our findings indicate that a stronger enforcement infrastructure generally diminishes local informality and bolsters support for more programmatic parties in the short term. Over time, these effects could potentially become more pronounced as trust in state institutions gradually builds, steering voters further away from clientelistic preferences (Bustikova and Corduneanu-Huci

2017). However, it remains uncertain whether a single, centrally-driven intervention can initiate a sustained, positive, self-reinforcing cycle that continuously enhances state capacity and supports programmatic politics, or if it will only temporarily disrupt the influence of clientelistic parties. A key factor in determining the persistence of these effects is whether the shift from informality to formality genuinely reduces voters' susceptibility to particularistic offers, or simply changes the nature of the goods and services they expect in exchange for their votes. If the latter is true, it suggests that clientelistic parties may simply need to adjust their strategies to the new economic realities to regain influence. Thus, while the initial effects of enforcement are encouraging, the potential for clientelistic parties to learn and adapt poses a potential challenge to achieving long-lasting change.

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A Supplementary Tables

Table A.1: Effects of Enforcement on Clientelism: OLS Estimates

	(1) Probability of a clientelistic party winning (DALP)	(2) Probability of a clientelistic party winning (V-party)	(3) Probability of a clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.007	−0.008*	−0.010**	−0.004**
	(0.005)	(0.005)	(0.005)	(0.002)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.590	0.524	0.422	0.316
Observations	36672	36672	36672	36672
R-squared	0.290	0.270	0.287	0.270

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates of Equation (1). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.2: First Stage

	(1) $\text{Enforcement}_{m,c(t)}$
Distance to nearest $\text{LO}_{m,c(t)}$	−0.030***
	(0.011)
Distance to nearest $\text{LO}^2_{m,c(t)}$	0.0002***
	(0.000)
$\text{Inspectors per office}_{r,c(t)}$	0.011***
	(0.002)
$\text{Inspectors per office}^2_{r,c(t)}$	−0.0001***
	(0.000)
Distance to nearest $\text{LO}_{m,c(t)} \times \text{Inspectors per office}_{r,c(t)}$	−0.0001***
	(0.000)
Municipality FE	Yes
Election-year FE	Yes
Observations	36441
R-squared	0.592
F	21.092

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates of Equation (2). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Distance to the nearest labor office is calculated using the OpenStreetMap API and is expressed in units of 100 km. $\text{Inspectors}_{r,t-1}$ represents the average number of inspectors per office in region r in the previous electoral cycle.

Table A.3: Effects of Enforcement on Vote Shares of Clientelistic Parties in Mayoral Elections

	(1) Vote share of clientelistic parties (DALP)	(2) Vote share of clientelistic parties (V-party)	(3) Vote share of clientelistic parties (GPS)
$\text{Enforcement}_{m,c(t)}$	−0.250***	−0.106**	−0.082**
	(0.060)	(0.052)	(0.041)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep.var	0.443	0.377	0.237
Observations	36414	36414	36412
Kleibergen-Paap rk LM statistic	91.18	91.18	90.86
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.08	16.08	16.02
Hansen J statistic	8.91	16.95	11.94
p-value	0.063	0.002	0.018

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before the election in year t. The dependent variables represent the vote share in the first round of elections for all candidates from parties identified as clientelistic, according to the various survey-based classifications. For a detailed description of the variables, see Appendix OA.1.

Table A.4: Effects of Enforcement on the Probability of a Clientelistic Party Winning: Demand-Side Dynamics

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-dem)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.250***	−0.166***	−0.168***	−0.183***
	(0.061)	(0.060)	(0.057)	(0.035)
Num of clientelistic candidates $(\log)_{m,t}$	0.775***	0.775***	0.731***	0.286***
	(0.006)	(0.005)	(0.006)	(0.003)
Num of candidates $(\log)_{m,t}$	-0.489***	-0.414***	-0.345***	-0.169***
	(0.014)	(0.013)	(0.012)	(0.007)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36441	36441	36441	36441
Kleibergen-Paap rk LM statistic	87.77	87.58	88.11	87.58
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	15.29	15.24	15.34	15.24
Hansen J statistic	3.82	13.67	7.00	8.78
p-value	0.430	0.008	0.136	0.067

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Candidates are categorized as clientelistic based on classifications derived from the corresponding scores in each survey. For a detailed description of the variables, see Appendix OA.1.

Table A.5: Effects of Enforcement on Formal Employment: Alternative measures

	(1) Formal workers (% Population)	(2) Formal workers (log)
Enforcement$_{m,t-1}$	0.038***	0.718***
	(0.012)	(0.089)
Population (log)		0.470***
		(0.048)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Mean dep.var	0.103	6.130
Observations	137704	137704
Kleibergen-Paap rk LM statistic	245.50	224.81
p-value	0.000	0.000
Kleibergen-Paap rk Wald F statistic	50.18	46.77
Hansen J statistic	69.68	41.94
p-value	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. Enforcement$_{m,t-1}$ refers to the log of the mean number of inspections per 100 firms from the year prior to the current census year. Column (1) is the share of formal worker on the total population. Column (2) accounts for population. Formal workers is sourced from RAIS. For a detailed description of the variables, see Appendix OA.1.

Table A.6: Effects of Enforcement on Clientelism: Including Controls

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
Panel A: Controlling for GDP, population, and state × year fixed-effects				
$\text{Enforcement}_{m,c(t)}$	−0.485**	−0.890**	−1.137**	−0.312***
	(0.232)	(0.330)	(0.444)	(0.104)
Municipality FE	Yes	Yes	Yes	Yes
State × Election-year FE	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Mean dep.var	0.570	0.511	0.404	0.311
Observations	30484	30484	30484	30484
Kleibergen-Paap rk LM statistic	4.35	4.35	4.35	4.35
p-value	0.500	0.500	0.500	0.500
Kleibergen-Paap rk Wald F statistic	335.52	335.52	335.52	335.52
Panel B: Controlling for share of urban, female, and high-skill population in 1991 interacted with time-trends				
$\text{Enforcement}_{m,c(t)}$	−0.710***	−0.463***	−0.337**	−0.344***
	(0.179)	(0.143)	(0.134)	(0.082)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Mean dep.var	0.584	0.518	0.417	0.313
Observations	29708	29708	29708	29708
Kleibergen-Paap rk LM statistic	27.44	27.44	27.44	27.44
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	5.74	5.74	5.74	5.74
Hansen J statistic	5.33	11.10	4.15	7.10
p-value	0.256	0.026	0.386	0.131

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). In Panel A standard errors are clustered at the municipal and at the state level and in Panel B at the municipal level. All regressions include municipality and year fixed-effects. The time-varying controls consist of the log of population, along with the shares of urbanization, high-school-educated workers, and the female population, each measured as of 1991 and interacted with time trends. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.7: Incumbent Party Clientelism and Distance to Nearest Labor Office

	Distance to Nearest Labor Office			
	(1)	(2)	(3)	(4)
Clientelistic party (DALP)	0.016			
	(0.018)			
Clientelistic party (V-party)		0.047		
		(0.048)		
Clientelistic party (GPS)			0.033	
			(0.035)	
Salience of clientelism				0.090
				(0.090)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
State × Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	1.601	1.601	1.530	1.530
Observations	31875	31875	21912	27130
R-squared	0.858	0.858	0.889	0.869

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates. Dependent variable is the mean distance to the nearest labor office in the electoral cycle before election year t. Standard errors are clustered at the municipality and at the state level. All regressions include municipality and year fixed-effects.

Table A.8: Incumbent Party Clientelism and the Number of Inspectors

	Number of Inspectors per Office			
	(1)	(2)	(3)	(4)
Clientelistic party$_{r,c(t)}$ (DALP)	−8.463			
	(7.296)			
Share of clientelistic parties$_{r,c(t)}$ (V-party)		−7.545		
		(7.092)		
Share of clientelistic parties$_{r,c(t)}$ (GPS)			−2.455	
			(6.349)	
Mean of salience of clientelism$_{r,c(t)}$				3.601
				(19.575)
State FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	24.568	24.568	24.568	24.568
Observations	153	153	153	153
R-squared	0.712	0.710	0.707	0.706

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates. Data are aggregated at the state-election level. Dependent variable is the mean number of inspectors at the state level in the electoral cycle before election year t. Standard errors are clustered at the region level. All regressions include region and year fixed-effects.

Table A.9: Enforcement and Clientelism: Continuous Measures

	(1) Clientelism Score of winning party (DALP)	(2) Clientelism Score of winning party (V-party)	(3) Clientelism Score of winning party (GPS)
$\text{Enforcement}_{m,c(t)}$	−0.386***	−0.932***	−0.669*
	(0.086)	(0.210)	(0.358)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep.var	2.957	0.851	4.770
Observations	36441	36441	36441
Kleibergen-Paap rk LM statistic	92.68	92.68	92.68
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.17	16.17	16.17
Hansen J statistic	8.07	21.53	7.65
p-value	0.089	0.000	0.105

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.10: Enforcement and Likelihood of Re-Election

	(1) Incumbent party re-contest	(2) Incumbent party re-elected	(3) Incumbent party re-contest	(4) Incumbent party re-elected
$\text{Enforcement}_{m,c(t)}$ (β_1)	−0.060	−0.120**	−0.016	−0.064
	(0.051)	(0.059)	(0.055)	(0.059)
$\text{Enforcement}_{m,c(t)}$ ×			−0.055	−0.041
$\text{Clientelistic party}_{m,c(t)}$ (β_2)			(0.048)	(0.033)
$\text{Clientelistic party}_{m,c(t)}$			0.064	0.031
			(0.052)	(0.035)
$\beta_1 + \beta_2$			−0.070	−0.104*
			(0.055)	(0.059)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.764	0.316	0.764	0.316
Observations	23941	30973	23941	30973
Kleibergen-Paap rk LM statistic	137.23	124.47	141.90	134.01
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	37.14	24.09	19.16	13.13
Hansen J statistic	3.54	5.66	9.42	19.79
p-value	0.471	0.226	0.308	0.011

* p<0.10, ** p<0.05, *** p<0.010. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. Estimates of equation (3). $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. Clientelistic party is defined using the DALP database, as detailed in Appendix OA.1.

Table A.11: Effects of Enforcement on Support for Clientelistic Parties in City Council Elections

	(1) % of votes for clientelistic councilor candidates (DALP)	(2) % of votes for clientelistic councilor candidates (V-party)	(3) % of votes for clientelistic councilor candidates (GPS)
$\text{Enforcement}_{m,c(t)}$	-0.057^{**}	-0.044^{**}	-0.114^{***}
	(0.024)	(0.021)	(0.021)
Number of candidates (log)	-0.086^{***}	-0.096^{***}	-0.071^{***}
	(0.005)	(0.005)	(0.004)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep. var.	0.283	0.215	0.147
Observations	31707	31707	31707
Kleibergen-Paap rk LM statistic	105.56	105.56	105.56
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	19.85	19.85	19.85
Hansen J statistic	30.29	29.61	16.94
p-value	0.000	0.000	0.002
	(4) % of votes for clientelistic elected councilors (DALP)	(5) % of votes for clientelistic elected councilors (V-party)	(6) % of votes for clientelistic elected councilors (GPS)
$\text{Enforcement}_{m,c(t)}$	-0.047^{*}	-0.036	-0.107^{***}
	(0.028)	(0.025)	(0.024)
Number of candidates (log)	-0.074^{***}	-0.084^{***}	-0.066^{***}
	(0.006)	(0.005)	(0.005)
Municipality FE	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes
Mean dep. var.	0.311	0.241	0.166
Observations	31692	31692	31692
Kleibergen-Paap rk LM statistic	104.83	104.83	104.83
p-value	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	19.71	19.71	19.71
Hansen J statistic	37.18	31.18	20.52
p-value	0.000	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.12: Effects of Enforcement on Clientelism: Dealing With Missing Scores

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
Panel A: Excluding Parties with Missing Scores				
$\text{Enforcement}_{m,c(t)}$	−0.133*	−0.283***	−0.095	−0.005
	(0.073)	(0.076)	(0.081)	(0.024)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.676	0.571	0.627	0.374
Observations	31856	33412	24370	30748
Kleibergen-Paap rk LM statistic	84.43	91.39	59.25	90.64
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	14.63	16.94	10.25	17.97
Hansen J statistic	25.43	16.80	16.16	10.33
p-value	0.000	0.002	0.003	0.035
Panel B: Treating Parties with Missing Scores as Clientelistic				
$\text{Enforcement}_{m,c(t)}$	−0.124*	−0.330***	−0.164**	0.019
	(0.066)	(0.078)	(0.065)	(0.023)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.717	0.607	0.749	0.379
Observations	36441	36441	36441	36441
Kleibergen-Paap rk LM statistic	92.68	92.68	92.68	92.68
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.17	16.17	16.17	16.17
Hansen J statistic	26.68	11.84	16.18	20.36
p-value	0.000	0.019	0.003	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. The sample of Panel A excludes parties with missing values for the clientelistic scores. In Panel B, parties with missing values for the clientelistic scores are coded as clientelistic, and missing values for the salience of clientelism are imputed using the sample median. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. All regressions include municipality and year fixed-effects. For a detailed description of the variables, see Appendix OA.1.

Table A.13: Effects of Enforcement on Clientelism: Excluding Co-Partisans of the Governor

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.315***	−0.293***	−0.087	−0.191***
	(0.086)	(0.088)	(0.088)	(0.043)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.650	0.564	0.448	0.322
Observations	29901	29901	29901	29901
Kleibergen-Paap rk LM statistic	73.45	73.45	73.45	73.45
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	12.28	12.28	12.28	12.28
Hansen J statistic	8.26	9.14	14.59	10.38
p-value	0.082	0.058	0.006	0.034

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. The sample excludes candidates from the same party as the governor of their region. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. All regressions include municipality and year fixed-effects. For a detailed description of the variables, see Appendix OA.1.

Table A.14: Effects of Enforcement on Clientelism: Excluding Co-Partisans of the President

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.319***	−0.166**	−0.119	−0.182***
	(0.078)	(0.073)	(0.073)	(0.037)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.644	0.571	0.457	0.336
Observations	33457	33457	33457	33457
Kleibergen-Paap rk LM statistic	92.72	92.72	92.72	92.72
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	16.67	16.67	16.67	16.67
Hansen J statistic	23.99	33.82	12.06	27.27
p-value	0.000	0.000	0.017	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. The sample excludes candidates from the same party as the president. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. All regressions include municipality and year fixed-effects. For a detailed description of the variables, see Appendix OA.1.

Table A.15: Effects of Enforcement on Clientelism Excluding PT Mayors

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
Panel A: Excluding municipalities governed by PT mayors				
$\text{Enforcement}_{m,c(t)}$	−0.416***	−0.271***	−0.165**	−0.223***
	(0.087)	(0.077)	(0.074)	(0.040)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.605	0.535	0.431	0.321
Observations	34508	34508	34508	34508
Kleibergen-Paap rk LM statistic	85.95	85.95	85.95	85.95
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	15.57	15.57	15.57	15.57
Hansen J statistic	7.95	17.76	7.58	12.32
p-value	0.094	0.001	0.108	0.015
Panel B: Excluding municipalities where PT mayors won				
$\text{Enforcement}_{m,c(t)}$	−0.371***	−0.221***	−0.135*	−0.204***
	(0.084)	(0.076)	(0.075)	(0.039)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.628	0.557	0.449	0.331
Observations	34292	34292	34292	34292
Kleibergen-Paap rk LM statistic	87.37	87.37	87.37	87.37
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	15.35	15.35	15.35	15.35
Hansen J statistic	9.61	20.44	6.34	14.30
p-value	0.048	0.000	0.175	0.006

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). The sample in Panel A excludes municipalities governed by PT (*Partido dos Trabalhadores*) mayors during the electoral cycle $c(t)$. Panel B, in turn, excludes municipalities where PT mayors were elected in the election year t. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.16: Effects of Enforcement on Clientelism: Linear First Stage

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.520***	−0.392***	−0.218**	−0.285***
	(0.113)	(0.096)	(0.085)	(0.054)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36441	36441	36441	36441
Kleibergen-Paap rk LM statistic	54.70	54.70	54.70	54.70
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	17.77	17.77	17.77	17.77
Hansen J statistic	3.41	7.51	4.09	4.04
p-value	0.182	0.023	0.129	0.132

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3) with a linear specification in the first stage. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Table A.17: Effects of Enforcement on Clientelism: 1-Year Lag Enforcement

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,t-1}$	−0.431***	−0.400***	−0.304***	−0.270***
	(0.086)	(0.083)	(0.081)	(0.043)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36379	36379	36379	36379
Kleibergen-Paap rk LM statistic	87.78	87.78	87.78	87.78
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	18.14	18.14	18.14	18.14
Hansen J statistic	5.39	9.11	9.21	7.90
p-value	0.249	0.058	0.056	0.095

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,t-1}$ refers to the mean number of inspections per 100 firms in the previous year. For a detailed description of the variables, see Appendix OA.1.

Table A.18: Effects of Enforcement on Clientelism using Euclidean Distance

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.399***	−0.284***	−0.232***	−0.208***
	(0.074)	(0.070)	(0.067)	(0.035)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.591	0.524	0.422	0.316
Observations	36496	36496	36496	36496
Kleibergen-Paap rk LM statistic	100.67	100.67	100.67	100.67
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	33.12	33.12	33.12	33.12
Anderson-Rubin ξ^2 test (robust inference)	53.91	33.90	26.25	64.36
p-value	0.000	0.000	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. In the first stage, distance to the nearest labor office is calculated using the Euclidean method. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

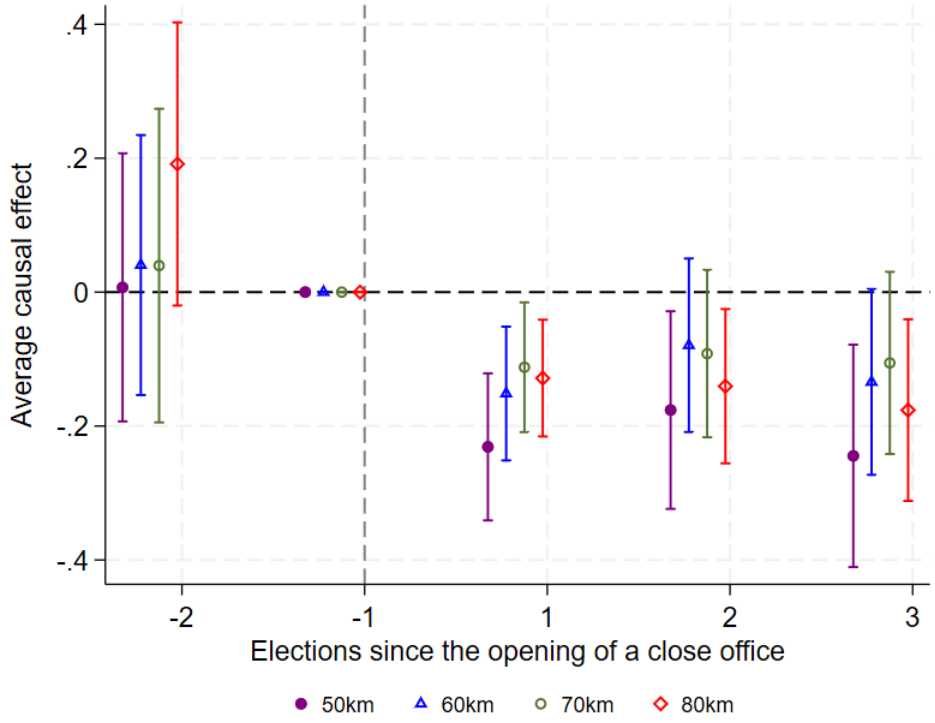
Table A.19: Effects of Enforcement on Clientelism Excluding the States of Amazonas and Pará

	(1) Probability of clientelistic party winning (DALP)	(2) Probability of clientelistic party winning (V-party)	(3) Probability of clientelistic party winning (GPS)	(4) Salience of clientelism (V-party)
$\text{Enforcement}_{m,c(t)}$	−0.390***	−0.265***	−0.183***	−0.192***
	(0.071)	(0.062)	(0.059)	(0.031)
Municipality FE	Yes	Yes	Yes	Yes
Election-year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.592	0.525	0.424	0.317
Observations	35144	35144	35144	35144
Kleibergen-Paap rk LM statistic	113.21	113.21	113.21	113.21
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	24.31	24.31	24.31	24.31
Hansen J statistic	13.74	27.42	16.75	20.08
p-value	0.008	0.000	0.002	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). The sample excludes the states of Amazonas and Pará. Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,c(t)}$ refers to the log of the mean number of inspections per 100 firms in the electoral cycle before election year t. For a detailed description of the variables, see Appendix OA.1.

Supplementary Figures

Figure A1: Event Study: Probability of Clientelistic Party Victory



Estimates of Equation (4), controlling for the number of inspectors and the number of pre-exisiting offices. Confidence intervals are at the 90% level. The sample is restricted to municipalities that experienced the opening of an office within a 50, 60, 70, or 80 km radius. The dependent variable is the probability that a clientelistic party, as classified by the DALP database, wins the mayoral election. For a detailed description of the variables, refer to Appendix OA.1.

Online Appendices

OA.1 Data Sources and Construction Details

Clientelism Measures

Our primary measure of clientelism is derived from the Democratic Accountability and Linkages Project (DALP) Kitschelt et al. (2013). This survey, conducted in 2008, features responses from political experts across several countries regarding the behavior of local political parties. In Brazil, 240 experts were interviewed.[1] To assess the level of clientelism across different parties, we use the approach outlined by Frey (2019), who employs scores from four specific questions related to the intensity with which parties engage in clientelistic exchanges to secure votes. These questions are as follows:

1. Consider whether candidates and parties give or promise to give citizens consumer goods (e.g., food or liquor, clothes, cookware, appliances, medicines, building materials etc.) as inducement to obtain their votes. How much effort do candidates and parties expend to attract voters by providing consumer goods?

2. Consider whether candidates and parties give or promise to give citizens preferential access to material advantages in public social policy schemes (e.g., preferential access to subsidized prescription drugs, public scholarships, public housing, better police protection etc.) as inducement to obtain their votes. How much effort do candidates and parties expend to attract voters by providing referential public benefits?

3. Consider whether candidates or parties give or promise to give citizens preferential access to employment in the public sector or in the publicly regulated private sector (e.g., post office, janitorial services, maintenance work, jobs at various skill levels in state owned enterprises or in large private enterprises with government contracts and subsidies, etc.) as inducement to obtain their vote. How much effort do candidates

[1] Kitschelt et al. (2013) define as experts "individuals who know about their country's parties, campaigns, and elections because they learned about these subject matters in their university training. Most of them are located in university political science departments, but they may also be affiliated with law faculties, sociology, history or public policy departments or work in independent research institutes or non-profit agencies and interest associations. Moreover, a small panel of 3-5 journalists who cover campaigns and elections will be included. They come from independent national newspapers or weekly journals with different political orientations."

or parties expend to attract voters by providing preferential access to employment opportunities?

4. Consider whether candidates or parties give or promise to give citizens and businesses preferential access to government contracts or procurement opportunities (e.g., public works/construction projects, military procurement projects without competitive bidding to companies whose employees support the awarding party) as inducement to gain their and their employees' votes. How much effort do candidates or parties expend to attract voters by offering them preferential access to government contracts or procurement opportunities?

Each question is rated on a scale from 1 to 4, where 1 signifies *A negligible effort or none at all*, 2 indicates *A minor effort*, 3 represents *A moderate effort*, and 4 denotes *A major effort*. There is also an option for *Don't know*. Following Frey (2019), we classify parties as clientelistic if they achieve an average score of 3 or higher on these questions. As robustness, we also employ a continuous measure of these scores. From 1996 to 2020, 92.76% of municipalities were governed by parties included in the DALP survey. Smaller parties not assessed by DALP are labeled as non-clientelistic. Excluding these parties from our analysis yields similar results.

The second expert survey we use is V-party (Lindberg et al. 2022). This database includes a specific question on clientelism, formulated as follows: *"To what extent do the party and its candidates provide targeted and excludable (clientelistic) goods and benefits—such as consumer goods, cash or preferential access to government services—in an effort to keep and gain votes?"*. Responses are scored on a 0–4 scale. For consistency with the DALP measure, we classify parties with an average score above 3 as clientelistic, and we also conduct robustness checks using the full ordinal scale. Unlike the DALP dataset, which offers only a single point in time, the V-Party database spans multiple years. It is administered during presidential election years, which fall two years before municipal elections. Consequently, for our analysis, we use each party's V-Party score from the presidential election that immediately precedes the municipal race. Overall, 92.3% of the municipalities in our sample are governed by parties included in the V-Party database. As before, any parties not covered by the database are coded as non-clientelistic.

Because the first V-Party item measures only whether a party participates in clientelist exchanges, we incorporate a second measure that assesses the salience of clientelist mobilization. This second measure captures how central clientelism is to a party's strategy for gaining and retaining voter support. Experts in the V-Party dataset are asked to identify, from a list of seventeen possible options, the one they believe is most relevant to a party's

voter mobilization efforts. Our measure for each party represents the proportion of experts who select: "Clientelism in order to keep or gain votes (the distribution of targeted and excludable benefits toward supporters)."

Our third measure of clientelism comes from the Global Party Survey (GPS) (Norris 2020). This expert survey includes a specific question on clientelism, which asks: *Next, on clientelism, where do parties currently stand on distributing public goods, like material benefits, subsidies, construction projects, and jobs. Do they favor giving universally to all citizens or else primarily to their own supporters?* The responses are scaled from one to ten. We classify parties as clientelistic if they have a mean score above 5. For robustness, we also analyze using the continuous scale of the scores.

The list of all of all political parties in our database with their clientelism and left-wing score is reported in Table OA.1. It is important to note that we account for party name changes over time, ensuring that their scores remain consistent throughout the analysis. As mentioned in Section 8, parties with similar score of left-right scale are coded differently in terms of clientelism.

Table OA.1: Clientelism and Ideology by Party

	DALP			V-party			Global party survey			
	Clientelism score (1-4)	Clientelism dummy	Left-right scale (1-10)	Clientelism score (0-4)	Clientelism dummy	Left-right scale (1-5)	Clientelism score (1-10)	Clientelism dummy	Salience of clientelism (0-1)	Left-right scale (1-10)
CIDADANIA	2.4	0.0	3.9	.	.	.	.	.	.	.
DC	.	.	.	.	.	.	.	.	.	.
NOVO	.	.	.	.	.	.	.	.	.	.
PAN	.	.	.	.	.	.	.	.	.	.
PATRIOTA	.	.	.	.	.	.	.	.	.	.
PC DO B	.	.	.	.	.	.	.	.	.	.
PDT	3.0	1.0	3.5	2.0	0.0	2.1	.	.	0.3	.
PFL/DEM	3.4	1.0	8.5	3.0	1.0	5.0	7.4	1.0	0.4	8.5
PHS	.	.	.	.	.	.	.	.	.	.
PMB	.	.	.	.	.	.	.	.	.	.
PMDB	3.5	1.0	5.8	3.8	1.0	3.5	7.3	1.0	0.6	6.2
PMN	.	.	.	.	.	.	.	.	.	.
PODE	.	.	.	.	.	.	.	.	.	.
PP	3.3	1.0	8.2	3.0	1.0	5.0	7.6	1.0	0.5	8.3
PPL	.	.	.	.	.	.	.	.	.	.
PPS	2.4	0.0	3.9	2.0	0.0	2.0	.	.	0.3	.
PR / PL	3.3	1.0	7.8	2.7	0.7	4.5	.	.	0.4	.
PRB	.	.	.	.	.	.	7.8	1.0	.	8.0
PRN	.	.	.	.	.	.	.	.	.	.
PRONA	.	.	.	.	.	.	.	.	.	.
PROS	.	.	.	.	.	.	.	.	.	.
PRP	.	.	.	.	.	.	.	.	.	.
PRTB	.	.	.	.	.	.	.	.	.	.
PSB	2.6	0.0	3.1	2.0	0.0	1.9	3.0	0.0	0.1	3.0
PSC	.	.	.	.	.	.	.	.	.	.
PSD	.	.	.	3.0	1.0	4.0	.	.	0.5	.
PSDB	2.5	0.0	6.0	1.8	0.0	3.8	4.8	0.0	0.2	5.8
PSL	.	.	.	2.0	0.0	5.0	8.5	1.0	0.0	8.9
PSN	.	.	.	.	.	.	.	.	.	.
PSOL	.	.	.	.	.	.	.	.	.	.
PT	2.7	0.0	3.0	1.6	0.0	1.8	2.1	0.0	0.1	2.4
PT DO B	.	.	.	.	.	.	.	.	.	.
PTB	3.5	1.0	6.1	3.0	1.0	4.0	.	.	0.5	.
PTC	.	.	.	.	.	.	.	.	.	.
PTN	.	.	.	.	.	.	.	.	.	.
PTR/PST	.	.	.	.	.	.	.	.	.	.
PV	.	.	.	.	.	.	.	.	.	.
REDE	.	.	.	.	.	.	.	.	.	.
REPUBLICANOS	.	.	.	.	.	.	.	.	.	.
SOLIDARIEDADE	.	.	.	.	.	.	.	.	.	.

The list of parties for all candidates in the sample and their classification in terms of clientelism was conducted using the DALP database (Kitschelt et al. 2013), the V-Dem party database (Lindberg et al. 2022), and the GPS database (Norris 2020). The V-Dem party database includes multiple waves; the table presents the mean values across all elections for V-Dem.

Electoral data

Electoral outcomes are sourced from *Base dos Dados*, which aggregates and harmonizes data from the *Tribunal Superior Eleitoral* (TSE) for all available election years. We include observations from elections in 2000, 2004, 2008, 2012, 2016, and 2020. Additionally, we use data from 1996 available for a limited set of municipalities.

RAIS

We leverage employer-employee data collected from the *Relação Anual de Informaçoes Sociais* (RAIS), an annual micro-level census of all formal sector employees, private or public, collected by the Ministry of Labor in Brazil. Annually, employers are required to submit detailed information for each of their employees. Any discrepancies or errors in reporting are subject to legal sanctions, including fines imposed on employers who are found to have misreported.

From this data we extract the total number of formal employees at the year-municipality level, and data on bureaucrats to assess the effects on patronage. The RAIS provides comprehensive data on every employee at the municipal level, detailing their age, wage, work experience, education level, and type of contract.

Latinobarómetro

To enrich our main analysis, we draw on an alternative measure of clientelism using individual-level data from the Latinobarómetro survey. Respondents across five waves—2002, 2005, 2006, 2017, and 2018—were asked whether they had witnessed or experienced vote-buying (specifically, being pressured to vote in a certain way in exchange for gifts or favors). Although the wording varies slightly across waves, each formulation captures the broader concept of receiving something in return for one's vote. To control for any influence these wording differences might have on responses, we include year fixed effects in all models.

The precise survey questions are as follows:

- 2002, 2005, and 2006: "Have you known of someone in the last elections who was pressured or received something to change his vote in a certain way?"
- 2017 and 2018: "During the last election campaign, did you see candidates or people from the parties distributing gifts or favors in your neighborhood?"

We combine the data from these five waves into a single dataset to analyze how local enforcement intensity relates to reports of vote-buying. Our dependent variable is a binary

indicator of "bribery," coded as 1 if respondents reported witnessing vote-buying and 0 otherwise. To further account for individual-level differences, we control for demographic factors—namely age group, income level, education level, gender, and racial identification.

BEPS

To investigate the relationship between informality and clientelism at the individual level, we analyze data from the 2014 Brazilian Electoral Panel Study. Formal wage earners are defined as those possessing a *carteira assinada*—a signed worker's card that verifies a private sector contract and enrollment in the state-run social security system. The question about mayoral vote choice is limited in geographic scope, being administered only to respondents within the state of São Paulo. This survey offers a distinct advantage by providing highly precise data on voter preferences. It presents respondents with a list of actual candidates relevant to each municipality, offering a more accurate measure than relying on voters' recall from memory.

Census data

We use data from the 1991, 2000, and 2010 census to estimate the share of formal, informal, self-employed, and non-employed population in each municipality (among the active population). We gather this information from questions regarding employment status and possession of a labor card, known as *carteira de trabalho*. We define the economically active population as individuals between 18 and 64 years old who are not enrolled in school or university.

The relevant questions have slightly different formulation and possible answers across census waves. In 1991, there is a specific question about whether the individual has a work permit. They can awnser: Yes, No, I don't know, or I am not an employee. In the question regarding type of occupations, the potential answers were:

1. Agricultural Worker
2. Partner or Sharecropper – Employee
3. Partner or Sharecropper – Self-employed or Independent
4. Domestic Worker – Employee
5. Domestic Worker – Self-employed or Independent
6. Private Sector Employee
7. Public Sector Employee – Civil Servant
8. Public Sector Employee – State-owned Enterprise
9. Self-employed or Independent

10. Employer
11. Unpaid Worker

We classify workers as formal (informal) based on whether they report holding a work permit (or not) and falling into categories 1, 4, 5, 6, 7, or 8.[2] Additionally, we define individuals as self-employed if they identify as "Domestic Worker – Self-employed or Independent" or simply as "Self-employed or Independent".

In 2000, the occupational options are:

1. Domestic worker with a signed work permit
2. Domestic worker without a signed work permit
3. Employee with a signed work permit
4. Employee without a signed work permit
5. Employer
6. Self-employed
7. Unpaid apprentice or intern
8. Unpaid household member assistant
9. Worker producing for own consumption

In addition to these options, there is a question asking whether the individual works in the public sector. For consistency with the 1991 census, we classify individuals as formal employees if they select option 1, 3, or declare that they work in the public sector. We categorize those who choose option 2 or 4 as informal workers, and individuals selecting option 6 are coded as self-employed.

Finally, in 2010 the options are:

1. Employee with a signed work permit
2. Military personnel (Army, Navy, Air Force, Military Police, or Fire Department)
3. Employee under the Legal Regime of Public Servants
4. Employee without a signed work permit
5. Self-employed
6. Employer
7. Unpaid worker

For this wave, we categorize individuals in categories 1, 2, and 3 as formal employees, those selecting option 4 as informal employees, and individuals choosing option 5 as self-employed.

[2]We exclude the Partner or Sharecropper categories to maintain consistency with future census waves.

Additional variables

Local outcomes: Educational outcomes are derived from the school census. We analyze the share of primary and secondary schools equipped with internet access, laboratory science facilities, and libraries between 1997 and 2020. Importantly, we restrict to schools that are managed by the municipality. Data on internet access is available starting from 1999.

For health outcomes, we use micro-data on health establishments sourced from the *Cadastro Nacional de Estabelecimentos de Saúde* (CNES), covering the period from 2005 to 2020. We also restrict the analysis to establishments managed at the municipal level. We calculate the average number of beds and available equipment in all such establishments within each municipality.

Importance of economic issues: We consider the measure of economic issue salience from GPS (Norris 2020), based on the question: "How important are economic issues for each of the following parties?"

Centralization: In Table 12, we examine five measures of party centralization: two obtained from the DALP database (Kitschelt et al. 2013) and three from the V-Party dataset (Lindberg et al. 2022). To enhance the comparability of these variables, we normalized each to a scale ranging from 0 to 1, with 1 representing the highest level of centralization.

For the first two we follow Sells (2020) and we consider the following questions from the DALP database (Kitschelt et al. 2013) as a measure of centralization:

Programmatic and Centralized Candidates' Selection: This variable is derived from the question: "More generally, the power to select candidates in national legislative elections is always divided between local/municipal party actors, regional/state-level party organizations, and national party leaders. Often one particular level of party organization dominates the selection process, while in other places candidate selection is the outcome of bargaining between the different levels of party organization. Which of the following four options best describes the following parties' balance of power in selecting candidates for national legislative elections?"

Possible answers are: 1: National party leaders control the process of candidate selections. 2: Regional/state-level party organizations control the process of candidate selections. 3: Selection is the outcome of bargaining between different levels 4: Local/municipal actors control the process of candidate selections

Before normalization, we multiplied this variable by -1 to ensure that higher values correspond to greater centralization.

Programmatic and Centralized Electoral Strategy: The second question from DALP is: "Similarly, which of the following options best characterizes the process by which the following parties decide on electoral strategy, for example campaign platforms and slogans, coalition strategies, and campaign resource allocations?" Possible answers are: 1: Electoral Strategy is chosen by national party leaders with little participation from local or state level organizations. 2: Electoral strategy is chosen by regional or state-level organizations. 3: The choice of electoral strategy is the outcome of bargaining between the different levels of party organization. 4: Electoral strategy is chosen by local or municipal level actors.

This measure is also multiplied by -1, to ensure that higher values indicate greater centralization.

We complement these variables with three measures of centralization derived from the V-party database.

Funds Raised by Candidates: This variable measures the proportion of respondents in the V-Party database who selected "Funds of candidates" in response to the question: "What were the major sources of party funds for this election campaign?" Respondents could choose up to three answers from a list of seven options. As before, this variable is multiplied by -1.

Local Party Offices: This variable reflects the response to the question: "Does this party maintain permanent offices that operate outside of election campaigns at the local or municipal level? Clarification: By 'local or municipal' we mean low level administrative divisions that are ranked below regions, provinces, or states. We refer to offices that ensure professional personal and continued interaction of the party with citizens. Permanent offices operate outside of election campaigns."

The scale of responses goes from 0 to 4 where: 0: The party does not have permanent local offices. 1: The party has permanent local offices in few municipalities. 2: The party has permanent local offices in some municipalities. 3: The party has permanent local offices in most municipalities. 4: The party has permanent local offices in all or almost all municipalities.

Local Party Activists/Personnel: This variable is derived from responses to the question: "To what degree are party activists and personnel permanently active in local communities? Clarification: Please consider the degree to which party activists and personnel are active both during election and non-election periods. Party personnel refers to paid staff."

The scale of responses goes from 0 to 4 where: 0: There is negligible permanent presence of party activists and personnel in local communities. 1: There is minor permanent presence of party activists and personnel in local communities. 2: There is noticeable permanent

presence of party activists and personnel in local communities. 3: There is significant permanent presence of party activists and personnel in local communities. 4: There is widespread permanent presence of party activists and personnel in local communities.

Left-right scale: We use the left-right scale from the DALP and the V-Party dataset (Lindberg et al. 2022), where experts are asked to rank parties on a scale from furthest left to furthest right. In DALP the scales goes from 1 to 10, while in V-party from 1 to 6.

Level of personalism: We consider the following question from the DALP database (Kitschelt et al. 2013) : "To what extent do parties seek to mobilize electoral support by featuring a party leader's charismatic personality?" The scale goes from (1) Very little / not at all to(4) Very strongly.

Level of populism: We consider the following question from the V-party database (Lindberg et al. 2022): To what extent do representatives of the party use populist rhetoric (narrowly defined)?

OA.2 Criteria for the Establishment of Labor Offices

The Ministry of Labor defines the following requirements for the allocation of labor offices across municipalities:

- the availability of financial resources, personnel, and physical structure;
- an estimated population of at least one hundred thousand inhabitants;
- at least twenty thousand active employment contracts;
- at least one thousand five hundred commercial establishments.

Nonetheless, there can be some exceptions if a municipality:

- has received or is about to receive investments that could significantly expand the local labor market;
- is a regional hub municipality, according to the classification of the Brazilian Institute of Geography and Statistics (IBGE);
- is located in a border region;
- presents social conditions that require the presence of a labor office, subject to a technical opinion from the Labor Secretariat.

OA.3 Differences from Almeida and Carneiro (2012)

When analyzing the effects of enforcement of labor regulations on state capacity, we differ from Almeida and Carneiro (2012) by finding: (i) a negative effect on informality instead of a null result, (ii) a decrease in non-employment instead of an increase. The aim of this section is to identify whether such inconsistencies come from data differences or from the empirical strategy.

Our dataset differs from that used by Almeida and Carneiro (2012) in several key aspects: the time period, the method of measuring distance, and the definition of formal and informal workers. Firstly, while their labor market outcomes are derived from cross-sectional data from the 2000 census, we utilize additional census data from 2010, providing a broader temporal scope. Secondly, we calculate distances using Open Street Maps, whereas they rely on data from a Brazilian auto insurance company (BB). Thirdly, our definition of employees includes domestic workers, public employees, and military personnel, which are excluded in their study. Lastly, they measure the number of inspectors per 100,000 firms based on data from 2001—an year after their census data—while we use a measure of lagged inspectors per office to assess enforcement intensity.

Regarding differences in our empirical strategy compared to Almeida and Carneiro (2012), they focus solely on the interaction between distance to labor offices and the number of in-

spectors per firm at the state level, choosing not to interpret distance coefficients in isolation due to the potential endogeneity in the placement of labor offices. In contrast, we address endogeneity by employing an instrumental variable approach. This method exploits variations in both the timing of labor office openings and the number of inspectors per region. We recognize that the location of offices is typically based on the economic criteria of the host municipality; therefore, we exclude these municipalities from our analysis, under the assumption that the opening of an office in a neighboring municipality is not influenced by its specific characteristics. Additionally, we control for potential confounding factors by including municipality and year fixed-effects in our model.

To pinpoint the origins of the differences in our results, we begin by adjusting our classifications of formal, informal, and self-employed workers to exclude public sector employees and domestic workers. Table OA.2 demonstrates that these exclusions do not contribute to the observed discrepancies in our findings.

Table OA.2: Effects of Enforcement on Labor Outcomes with Stricter Definition of Formal Workers

	(1) Share of formal workers	(2) Share of informal workers
$\text{Enforcement}_{m,t-1}$	0.113***	−0.027*
	(0.036)	(0.016)
Municipality FE	Yes	Yes
Census FE	Yes	Yes
Mean dep.var	0.174	0.164
Observations	10672	10672
Kleibergen-Paap rk LM statistic	19.41	19.41
p-value	0.002	0.002
Kleibergen-Paap rk Wald F statistic	4.37	4.37
Hansen J statistic	25.14	46.20
p-value	0.000	0.000

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). Standard errors are clustered at the municipal level. All regressions include municipality and year fixed-effects. $\text{Enforcement}_{m,t-1}$ refers to the log of the mean number of inspections per 100 firms from the year prior to the current census year. All shares are based on the total active population. For a description of the census variables see Appendix OA.1. We only consider employees in the private sector who are not domestic workers as in Almeida and Carneiro (2012).

Another significant difference in our data is the inclusion of an additional census year, which enables us to analyze within-municipality variation related to the location of labor offices. We demonstrate that our results remain robust when we focus solely on the 2000 census data in Table OA.3. In this analysis, we replace municipality fixed-effects with state fixed-effects and incorporate the same set of controls used by Almeida and Carneiro (2012), except for distance and its interactions, since these are correlated with our chosen instrument.

Table OA.3: IV Approach for 2000

	(1) Share of formal workers	(2) Share of informal workers	(3) Share of self-employed workers	(4) Share of non-workers
$\text{Enforcement}_{m,t-1}$	0.079***	−0.031***	−0.018***	0.004
	(0.008)	(0.006)	(0.006)	(0.008)
State × census year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.201	0.174	0.180	0.393
Observations	5063	5063	5063	5063
Kleibergen-Paap rk LM statistic	126.31	126.31	126.31	126.31
p-value	0.000	0.000	0.000	0.000
Kleibergen-Paap rk Wald F statistic	12.77	12.77	12.77	12.77

* p<0.10, ** p<0.05, *** p<0.010. IV estimates of equation (3). The sample is restricted to the year 2000. Standard errors are clustered at the municipal level. All regressions include state × year fixed-effects, and time-varying controls. $\text{Enforcement}_{m,t-1}$ refers to the log of the mean number of inspections per 100 firms from the year prior to the current census year. All shares are based on the total active population. For a description of the census variables see Appendix OA.1.

Thus, the primary distinction between our approach and that of Almeida and Carneiro (2012) lies in the empirical strategy rather than differences in the data. The authors employ a reduced-form model focusing solely on the interaction between distance and the number of inspectors. When we replicate their methodology using our data, as shown in Table OA.4 for the year 2000, our findings are consistent with theirs, except for the effect on non-employment which is not significant. However, when we include the 2010 data and incorporate municipality fixed-effects in Table OA.5, we observe a negative and significant effect on informality, in line with our main findings.

Overall, while the results concerning self-employment and non-employment vary depending on the methodology and measurement, our findings related to formality and informality remain robust across several analytical approaches – that is, when using the reduced-form model similar to Almeida and Carneiro (2012) including data from 2010, when adopting the instrumental variable (IV) strategy that excludes 2010, and when employing stricter measures of both variables.

Table OA.4: Reduced-Form Approach for 2000

	(1) Share of formal workers	(2) Share of informal workers	(3) Share of self-employed workers	(4) Share of non-workers
Distance × inspectors per office	0.0008***	0.0000	−0.0006***	0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0002)
State FE	Yes	Yes	Yes	Yes
Census year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.187	0.196	0.178	0.378
Observations	5168	5168	5168	5168

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates. All regressions include distance to the nearest labor office, its square, inspectors per office, state fixed-effects, year fixed-effects, and the same set of controls as in Almeida and Carneiro (2012).

Table OA.5: Reduced-Form Approach for 2000 and 2010 census

	(1) Share of formal workers	(2) Share of informal workers	(3) Share of self-employed workers	(4) Share of non-workers
Distance × inspectors per office	0.0008***	-0.0006***	0.0001	-0.0009***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Municipality FE	Yes	Yes	Yes	Yes
Census year FE	Yes	Yes	Yes	Yes
Mean dep.var	0.222	0.178	0.171	0.393
Observations	10336	10336	10336	10336

* p<0.10, ** p<0.05, *** p<0.010. OLS estimates. All regressions include distance, inspectors per office, municipality and year fixed-effects. Standard errors are clustered at the municipal level.